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THE LONDON MEDICAL GAZETTE,

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OF

Medicine and the Collateral Sciences.

FRIDAY, MARCH 26, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXVII.

Treatment of chronic hydrocephalus: internal remedies: mechanical expedients; bandages, tapping. Symptoms of spinal disease. Inflammatory conditions of the spinal marrow.

THE cure of chronic hydrocephalus may be attempted by internal remedies, or by external mechanical expedients, or by both.

Internal remedies.—The internal remedies by which most good appears to have been effected, and from which, therefore, most is to be hoped, are diuretics, purgatives, and above all, mercury, which is believed by many to have a special and powerful influence in promoting absorption. Conjointly with these, the abstraction of small quantities of blood from the head, by means of leeches, has been found beneficial.

Gölis advises that calomel should be given in half-grain doses, twice a day; or if that should purge too much, in doses containing only one-fourth of a grain. At the same time he would rub a scruple or two of mercurial ointment, mixed with ointment of juniper berries, upon the scalp, every night. He recommends that the head should be kept constantly covered also by a woollen cap. Infants require, he says, no other nutriment than good breast milk; while older patients should take a moderate quantity of meat. In mild weather they should be as much as possible in the open air. Under this plan of treatment he affirms that he has known the circumference of the head decrease by half

an inch or an inch in a period of six weeks or three months; and that perseverance in this method has frequently, in his experience, been followed by perfect recovery both of the mental and of the bodily powers. If no improvement should be perceptible in two months, he advises that diuretics should be given, *with* the former remedies; the acetate of potash, or squills, or both: that an issue should be made in the neck, or in each arm, and be kept discharging for several months. And he thinks that when convalescence has once begun, it is often much accelerated by minute doses of quina; the fourth of a grain, for example, thrice daily.

In a disease so unpromising as chronic hydrocephalus, we are warranted in trying any plan that has been found or supposed to be useful. An apothecary of considerable experience—now dead—once took the pains to write out and send me the particulars of two cases in which he had seen a peculiar mode of administering mercury successful. I will give you them nearly in his own words.

He had a lad, fourteen years old, whose name was Scott, under his care, with chronic hydrocephalus, in the year 1817. He had been ill two or three years. He was nearly blind, had very little use of his lower extremities, and could not walk across the room without support. He suffered violent pains in his head, and was unable to bear the least pressure on his scalp. His bowels were constipated, and his pulse “oppressed.” Cupping and blistering, and the blue pill, and drastic purgatives, and ordinary diuretics, tried in combination and succession, gave him temporary relief; but no permanent benefit was obtained. Dr. Gower then suggested a plan which he had himself found successful in such cases, and which had first been used by Dr. Carmichael Smyth, who had recorded ten cases of recovery under its adoption. Dr. Gower’s plan was to rub down ten grains of crude mercury with about

a scruple of manna, and five grains of *fresh* squills: this was to be one dose; and it was to be repeated every eight hours.

My informant rubbed the quicksilver down with conserve of roses, and then added the fresh squills, making the whole into the consistence proper for pills, with liquorice powder. The patient took this dose three times a day, for nearly three weeks, without any ptyalism being produced. Its effects were great prostration of strength, and loss of flesh, with gradual relief of all the boy's sufferings. It operated profusely by the kidneys. The medicine was continued twice a day, and at length once, for another fortnight; when all the symptoms of the disease had disappeared. The boy was greatly emaciated: he was then ordered an ounce and a half of Griffiths' mixture thrice daily; and soon regained his health and strength, and got quite well. And he remained well eight years afterwards.

The success obtained in this case led to the pursuance of a similar course in that of the son of a well-known fishmonger in Old Bond Street. He was about twelve years old, and afflicted in nearly the same manner as Scott, except that the pain in the head was more acute, and caused violent screaming: *relief* had been repeatedly given for a time, by cupping. The physician in attendance was unwilling to try the plan, when it was proposed to him, but said that he would give what was equivalent—small doses of blue pill, with squills in powder. The result was salivation in a few days, without any amendment. In about three weeks, the effects of the mercury having subsided, and the patient then suffering extreme pain in the head, loss of sight, and want of power over the lower extremities, my informant was desired to adopt any measures he thought fitting. The medicine was given as in the former case, and with the same happy consequence; it acted, as before, without producing ptyalism, but with a great reduction of strength and flesh: health was restored by steel, after the symptoms of hydrocephalus had disappeared. This cure also was permanent.

I think you will give me credit for not being over fond of recommending what I may call *conundrums*, instead of well-tryed and approved means of cure; but I say that in such a complaint as chronic hydrocephalus, we have generally the opportunity of testing the virtues of many reputed remedies, one after another; and we are not to despise or neglect any measures that have been found beneficial, merely because they are out of the way, or because we cannot see in what manner they can excel the more common formulæ.

You will observe that these were cases in

which the disease came on some time after the sutures of the skull had closed.

Mechanical remedies.—The mechanical remedies of chronic hydrocephalus are two: and they have a totally opposite mode of action. By the one, the brain is compressed; by the other, it is lightened of its pressure: yet both of them have proved successful. What does this show? what, but a confirmation of the doctrine that there are different states of the encephalon, very dissimilar in their essential character, yet having some symptoms in common; and those the most likely of all to catch our attention. Such common symptoms resemble an algebraical symbol, which derives its value from the plus or minus sign prefixed. Surely it is of vital importance to study, and if we can to settle, the differences whereby these inverse conditions, requiring contrary remedies, may be discriminated.

Bandages.—*Bandaging* the head is one of these two expedients; *puncturing* it the other. Neither of them is practically applicable after the bones of the skull have united.

Bandages appear to have been suggested by the notion that the increase of the fluid within the head, and probably some of the symptoms too, might depend, more or less, upon the want of firmness and proper resistance in the outer containing parts; in the feeble and half solid skull. A certain amount of support and pressure seems requisite for the due exercise of the cerebral functions. Beyond this amount all increase of pressure is hurtful. The middle point of safety it may be hard to hit. It is certain that the easy yielding of the bony walls of the head, by reason of the membranous interspaces that exist in the early periods of life, proves oftentimes the safety of these patients. If the skull did not expand as the water gathered, morbid symptoms would ensue. Great nicety must therefore be required in the use of this remedy. While the head is palpably enlarging, compression by means of plasters or bandages would probably be mischievous. When the disease is stationary, and the unconnected bones of the skull are loose and fluctuating, and the child is pale and languid, much benefit may be expected from moderate and well-regulated support. The late Sir Gilbert Blane was the first, I believe, to suggest this mode of treatment; but its safety and efficacy have been more recently demonstrated by Mr. Barnard, of Bath, who has related several examples of complete success from the employment of bandages. In these cases the children were pale, bloated, and feeble, with flabby muscles: the bones of their heads were moveable and floating, and the functions of the brain more or less impaired. Mr. Barnard applies strips of adhesive plaster, about three quar-

ters of an inch wide, completely round the head from before backwards; covering the forehead from the eye-brows to the hair of the head, as low down on the sides as the ears will permit, and lapping over each other behind. Then, cross strips are carried from one side of the head to the other over the crown; and lastly, one long slip, reaching from the forehead, within half an inch of the root of the nose, over the vertex to the nape of the neck. In his first trial of this plan, but never afterwards, Mr. Barnard laid pieces of linen, wetted with cold water, over the plasters. The only internal medicine given was castor oil, to regulate the bowels. The effects, in all this gentleman's cases, were these: a gradual diminution of the size of the head; mitigation, and ultimate disappearance, of all head symptoms, such as strabismus, rolling of the eyes, starting of the muscles, and convulsions; and at the same time, increased tone of the muscular system, an improved appearance of the skin, and of the secretions from the bowels. These are striking results: they show that, in certain conditions of chronic hydrocephalus, a part of the danger arises from a lack of due support and confinement of the brain; and they prove that compression alone may be equal to the cure.

But in children who are not of this pale and feeble habit, and in whom ossification of the skull goes on, the period when the walls cease to yield is the period of danger. The water continuing to accumulate, inordinate pressure begins to take place. To such heads the application of bandages or plasters must, if nothing else be done, be insufficient or unsafe. The brain-case being no longer capable of expansion, there remains to be attempted a reduction of the quantity of the liquid which it contains.

Tapping the head.—Now any considerable diminution of the accumulated fluid, through the agency of mere absorption, is scarcely to be expected; even although we endeavour to aid that process by applying leeches and cold to the head, and by purgatives, or diuretics, or diaphoretics. Some mode, more certain and effectual, of emptying the distended cavity, has therefore been earnestly sought after; and the second mechanical expedient of which I have spoken offers a very sure method of attaining this object. He must have been a bold physician who first proposed to decant the water from the brain, by means of a perforation, made with a trocar, through the membrane of the fontanelle, the membranes of the brain, and even the expanded cerebral substance itself. But the success of the project has amply vindicated his happy audacity. It is not a very new suggestion, but it has received particular attention in this country of late years: and though tapping the brain

in chronic hydrocephalus has been denounced as useless and cruel by some high continental authorities, by Gölis and Richter especially, it furnishes one of the best of the few chances of safety to the patient. Of course I mean ultimate safety, for the operation itself is attended with the present risk of accelerating the patient's death. Other means, however, failing, we are justified in advising that hazard. We have to consider that by performing the operation we incur the danger of abbreviating the existence of a being, whose life, without it, could scarcely be long continued, or capable of enjoyment: but then we afford *some* chance of a perfect cure. A speedy death, or an uncertain life of mental and bodily imbecility, or complete restoration: these are the three events to be looked at. Of the three, the second is, in my judgment, incomparably the worst; and if the case were my own, if I had to decide the painful question in reference to one of my own children, I would accept the alternative of probable speedy death on the one hand; possible complete recovery on the other.

To say the truth, the *immediate* danger is not so very great as you might suppose; provided that the operation be skilfully and cautiously performed, and only a moderate quantity of water drawn off at a time. That even a very rough operation is not necessarily fatal we learn from a singular case related by Mr. Greatwood. A child, 15 months old, afflicted with chronic hydrocephalus, fell down, and struck the back part of its head against a nail, which penetrated the skull. Above three pints of water gradually flowed out at the orifice thus made, and the child was cured.

I will mention a few instances in which tapping the brain has been performed; for I know no better mode of shewing you the manner in which the operation should be done, the cautions to be attended to in doing it, and what kind of success it has had.

There is an account of the performance of this operation by Lecat, in the *Philosophical Transactions* for the year 1751. This date is subsequent to the period when the Rev. Mr. Stevens suggested the propriety of trepanning Dean Swift's cranium. In 1778, Dr. Remmett, of Plymouth, punctured the head of a hydrocephalic child on five several occasions, with a lancet, and took away, in all, no less than 80 ounces of fluid; five pints, as pints were measured in that day. The child died 17 days after the last tapping. A very interesting case of the same kind is related by Dr. Vose, of Liverpool. His patient was an infant seven months old. Its head was more than twice the ordinary size. Three operations were performed; the first with a couching needle. Upwards of three ounces were on that occasion evacuated;

and it was estimated that about the same quantity dribbled away afterwards. The child thereupon became very weak, but was presently revived by some cordial medicine. About six weeks afterwards, the liquid having collected again, an opening was made with a bistoury, and 8 ounces were removed; and 9 days after that 12 ounces more, without any bad consequences. The head diminished in size, the patient got apparently well, and the case was published as a successful one. Unfortunately, however, the complaint afterwards returned, and the child died of it.

Mr. Lizars, of Edinburgh, operated upon a little patient of his twenty times in the course of three months; using a small trocar. Upon the escape of the water, squinting, and dilatation of the pupils, which previously existed, ceased immediately. The child recovered. Another very striking and instructive instance is recorded by Mr. Russell, of Edinburgh. The patient was an infant three months old, with an enormous head: twenty-three inches in circumference, and fifteen inches and a half from one ear to the other. The child was affected with strabismus, and a perpetual rolling of the eyes. The usual routine measures, compression among the rest, had been employed without any success. By four operations performed at intervals of about ten days, the size of the head was considerably diminished: but, the fluid continuing to collect, calomel was given in small and frequent doses, and the gums became sore, and the child got well. At eight months old the dimensions of the head were less, by four inches in circumference, and by two inches and a half across the vertex, than they had been previously to the first tapping; and the sutures had entirely closed.

But Dr. Conquest, of Finsbury Square, has, more than any other person, given authority to these operations. In a paper published in the *MEDICAL GAZETTE* in March 1838, he tells us that he had then tapped the heads of 19 children for this complaint, and in 10 of the 19 cases the children survived. He introduces a small trocar through the coronal suture below the anterior fontanelle, and cautiously makes pressure upon the head afterwards by means of strips of adhesive plaster; and he closes the wound in the integuments carefully after each time of puncturing. The greatest quantity of liquid withdrawn by him, at any one time, has been twenty ounces and a half; and the greatest number of operations on any one child has been five, performed at intervals of from two to six weeks. The largest total quantity of water removed was 57 or 58 ounces, by five successive tapplings.

This expedient, therefore, though doubtless hazardous, is really a valuable one.

The rules relating to its performance may briefly be summed up. The operation should scarcely be had recourse to until other means have failed. The trocar should be small, and it should be introduced perpendicularly to the surface, at the edge of the anterior fontanelle; so as to be as much as possible out of the way of the longitudinal sinus, and of the great veins that empty themselves therein. The fluid should be allowed to issue very slowly; and a part only of it should be evacuated at once. The instant that the pulse becomes weak, or the dilated pupil contracts, or the expression of the child's countenance manifestly alters, the canula should be withdrawn, and the aperture in the skull closed. Gentle compression should be carefully made, to compensate, in some degree at least, the pressure that has been removed with the fluid. Should the infant become pale and faint, it must be placed in the horizontal posture; and a few drops of sal volatile, or of brandy, mixed with water, should be given. Sometimes slight inflammatory action comes on in the course of a day or two after the tapping. When this happens, we must apply cold lotions, and leeches, and use the other remedies which I mentioned before, as proper to subdue such inflammation.

I once got a surgeon to perform the operation upon the infant of a poor woman, after I had tried in vain all the other measures that I have spoken of. To our horror, when the trocar was withdrawn from the canula, instead of clear serosity, a fine stream of purple blood spouted forth. The opening was at a considerable distance from the longitudinal sinus; but the trocar was not so delicate as it might have been, and I presume that one of the larger superficial veins had been pierced. I do not think, either, that the instrument was introduced in a sufficiently perpendicular direction. Of course the risk of hitting a vein is increased when the trocar is carried obliquely inwards: and a larger portion of the cerebral mass is also wounded. We naturally thought it was all over with the child, which presently became deadly pale and faint. A verdict of *infanticide by misadventure* stared us in the face. But under the use of stimulants the infant revived again; no hæmorrhage went on internally, as we apprehended it would; but the child, after a day or two, was very much the better for the loss of blood. This amendment, however, did not last; and the mother, who had been terrified by the immediate consequences of the operation, feared to come near me, lest I should wish to have it repeated: and at length our patient died. I had no opportunity of examining the condition of the head internally, which I had much desired to do.

On one subsequent occasion I have witnessed the operation. The subject of it was an infant about eight months old. Four months after its birth, its head was observed to grow inordinately large. At the time of the operation the fontanelles were exceedingly tense; the child screamed frequently, occasionally vomited, and was slightly convulsed; the features were pinched, and the eyeballs distorted downwards; but the pupils were not dilated. Four ounces of transparent liquid were let out by puncturing the anterior fontanelle. A few hours afterwards the child was tranquil, and much improved in aspect; the distortion of the eyeballs had disappeared. Three ounces more were taken away the next day. For two days thereafter all the symptoms appeared to be mitigated; but the skull was flaccid; yielding, like a broken egg, to the gentlest pressure. On the evening of the fourth day after the first tapping, the respiration became hurried, the child grew dull, and, before midnight, expired. In this case it appeared to me that the chance of success was balked by the want of external support subsequently to the tapping.

You will not expect me to draw any comparison between the merits of compression and of paracentesis, as substantive remedies. They are opposite measures, and adapted to different and opposite conditions of the brain. The one supplies defect of pressure; the other relieves its excess. To hold the balance even requires much care, a steady and gentle hand, an accurate judgment, and incessant vigilance. Either expedient may suffice, alone. Both may be (and have been) profitably employed in the same case, in succession, according to its varying circumstances. If the walls of the head be tight and firm, the trocar should precede the bandage; if lax and moveable, compression should be cautiously tried, and followed, if need be, by the puncture.

I have now done with the *inflammatory* affections of the *brain*: in conjunction with which I have also considered some other morbid conditions, that are either connected with inflammation of the contents of the cranium, or resemble it in some of their phenomena. Thus, I have spoken of *delirium tremens*, which is apt to be mistaken for inflammation of the brain: of *softening from disease of the cerebral arteries*, which is liable to be confounded with inflammatory softening: of *tumors* of different kinds, which tend to produce inflammation, or symptoms like those belonging to inflammation: and of *chronic hydrocephalus*, which sometimes is the sequel, sometimes the precursor, of acute hydrocephalus; and has other points of analogy with that disease, the encephalitis of children.

Spinal cord.—Before I take up the subject of apoplexy, and of cerebral hæmorrhage, I wish to direct your attention to the inflammatory conditions of the spinal cord.

The whole pathology of this portion of the nervous system is extremely interesting; but it has not yet been so thoroughly made out as to enable any one to give a very systematic or satisfactory account of it. In addition to those numerous difficulties with which I shewed you in a former lecture that the entire subject of the diseases of the nervous apparatus is beset, there is this farther obstacle to our studying diligently the disorders of the spinal marrow—that much labour and expense of time are required for exposing the interior of the vertebral canal; which is, therefore, too often neglected in examining the dead body.

There are certain points in the anatomy and physiology of the spinal cord which it is necessary that you should bear in mind, if you would have any clear notions even of what has been learned in respect to its pathology.

1. In the first place the spinal cord (including the medulla oblongata) is the seat and centre of that remarkable property, the reflex function; by which so many of the automatic movements of the body are governed.

2. In order that we may feel, or be conscious of, what occurs in any part of the trunk or limbs, and in order that our will to move any such part should be successful and obeyed, it is necessary that there should be a continuity of nervous matter between the part in question and the brain. If the cord be cut across at any point, or so crushed as to be thoroughly disorganized at that point, a complete abolition of sensation and of *voluntary* motion ensues in all those parts of the body that receive their sentient and motor nerves from that portion of the cord which lies beyond the place of the injury, reckoning from the brain; and what is true in this respect of the mechanical division of the cord, is equally true of such disease as pervades and spoils the nervous matter composing it.

Now it follows from this, that the effect of disorganizing forms of disease—as well as the effects of injury—vary greatly according to the part of the cord they occupy.

Thus any injury or disease situated in the spinal marrow, and pervading its whole thickness, where it is contained within the upper cervical vertebræ, is inevitably fatal at once; producing suffocation by paralyzing those muscles by the play of which the motions of respiration are performed. You know that the intercostal muscles and the diaphragm have at all times the main share in carrying on the mechanical actions of respiration; and probably they execute the

whole action in every case of ordinary breathing. Now the intercostal muscles are supplied with nerves from the spinal cord all along the dorsal vertebræ; and the diaphragm is principally supplied by the phrenic nerves, which are chiefly derived from the third and fourth cervical nerves. These muscles obey the will; but they act also independently of the will. The pneumogastric nerve, with respect to them, is an excitomotory nerve, and calls into play a reflex power which is transmitted from the medulla oblongata. Hence any profound injury of the spinal cord, above the origin of the phrenic nerves, stops both the voluntary and the involuntary movements of the respiratory muscles, and the individual perishes by apnoea in as strict a sense as though the access of air to the lungs had been suddenly prevented by a ligature drawn tightly round his wind-pipe.

Again, when a segment of the cord, however small, is disorganized in its cervical part, between the origin of the phrenic and the origin of the upper intercostal nerves, the breathing is not instantly suspended; but is performed entirely by means of the diaphragm, the intercostal muscles having no share in it. The ribs cease to rise and fall; and the abdomen is alternately protruded, and sinks back again. In each case I suppose the disease of the cord to be such as suffices to paralyze the parts supplied with nerves from it, below the seat of the disease. If disease of this kind occur below the giving out of the intercostal nerves, the breathing is not affected; we have *paraplegia* only, palsy and loss of feeling in the lower extremities, and, perhaps, in the hips, or even higher. Now a person in this condition *may* live a long time. When the disease is situated between the origin of the intercostal nerves and the origin of the phrenic, he may live a few days, but he seldom lives a week, and he never survives a month: and when the disease is higher still, in the very upper part of the cord, above the origin of the cervical nerves, he perishes outright. The *kind* and *degree* of disease, therefore, being the same, the character of the symptoms, and the amount of danger, differ remarkably according to the *seat* of the disease.

3. Although sense and voluntary motion cease upon the disruption of the communication with the brain, the excitomotory functions of the separated portion of the cord are not necessarily suspended. On the contrary, they seem to acquire increased activity. The automatic power is apt to run riot, as it were, when the controlling influence of the sensorium is lost. Each of you has probably seen the limbs of a recently decapitated frog thrown into violent action by the stimulus of galvanism. I have witnessed the same thing in the human

body after death by hanging. What is still more curious, you may have unequivocal manifestations of similar phenomena in the *living* body. I have lately been informed, by Dr. William Budd, of a case in which a man was afflicted with paraplegia; in consequence of disease of the vertebral column. He was totally deprived of the power of moving his lower extremities. Sensation in them was almost, yet not entirely extinct. A sharp pinch, or the prick of a pin, he could feel; but slight friction he was quite unconscious of: yet (as he himself said) his limbs were not; for when the inner edge of the foot was brushed or tickled by the hand of another person, the corresponding leg, over which he had no voluntary control, would start up, and be briskly convulsed. The same thing took place, in both limbs, whenever he passed his urine or fæces; so that he was obliged to have an apparatus of straps and ligatures to keep the legs down on such occasions. I have seen something like this myself*.

Separate and different filaments of the spinal cord connect themselves with, or help to form, different nerves which emerge from the cranio-spinal axis. A knowledge of this fact enables us to understand how it happens (as it sometimes does happen) that the upper extremities are deprived of sensibility, or of voluntary motion, or of both, by disease of the cord, while the same functions remain perfect in the lower and more distant limbs. Here the disease must have spared those strands or filaments of the cord which pass down to connect themselves with the nerves given off at the lower part of the spine; while it has affected those strands or filaments only which belong to certain nerves from the upper part.

4. You must bear in mind also the important discovery of Sir C. Bell, that the two roots by which each spinal nerve arises have distinct and different functions; the anterior roots being composed of motor fibrils, the posterior of sensiferous.

It is not so clear, although that opinion is, I believe, a prevalent one, that the anterior *columns* of the spinal cord are subservient to the purposes of motion, and the posterior to the faculty of sensation. This has been inferred, too hastily perhaps, from the ascertained endowments of the anterior and posterior *roots* of the *nerves*; and cases are cited which appear to favour such a notion: but then other cases go completely to contradict it. Thus Mr. Stanley has recently published an account of a patient who died in St. Bartholomew's Hospital. For some time before his death he had been completely unable to

* This very interesting case has since been published, in detail, with several others resembling it, in the 22d volume of the Medico-Chirurgical Transactions.

move his lower limbs, throughout their entire extent; while there was no discoverable impairment of sensation in any part of either limb. The spinal cord was the only part found diseased; and the disease was strictly limited to its posterior half or column. This portion of the cord, in its whole length, from the pons to its lower end, was of a dark brown colour, and extremely soft and tenacious. The anterior half, in its entire length, exhibited its natural whiteness and firm consistence. The roots of the spinal nerves were unaltered.

It was remarkable, and illustrative of the difficulty of these subjects, that with the change of structure which the cervical portion of the cord had undergone in this instance, there was no impairment either of motion or sensation in the upper limbs.

5. We must not forget that the brain, and the spinal cord, which are distinct from, but yet continuous with each other, sympathize largely and mutually under disease. This circumstance throws an additional obscurity over the study of their morbid conditions. It is one, however, which we cannot avoid, but which we must estimate and allow for, in our observation of diseases, as we best may.

6. There are a few remarks made by Dr. Abercrombie in relation to some of the anatomical dispositions of the cord and its investing membranes, which may help us to comprehend better some of their morbid contingencies. Thus, with respect to the dura mater of the cord, it is practically of importance to recollect "that it adheres very slightly to the canal of the vertebræ by a very loose cellular texture; and that it adheres very intimately to the margin of the foramen magnum. In this manner a cavity is produced betwixt the membrane and the inner surface of the spinal canal (external, *i. e.* to the membrane), which cavity may be the seat of effusion, and which has no communication with the cavity of the cranium. On the other hand, the space between the dura mater and the pia mater (or membrane immediately covering the cord), communicates freely with the cavity of the cranium; so that fluid may pass easily from one to the other, according to the position of the body."

Inflammation of the Spinal Cord.—I shall pursue the same order, in speaking of the inflammatory affections of the spinal cord, as I followed in respect to the analogous conditions of the encephalon. And, first, let us inquire what has been noticed of inflammation of the *membranes* of the cord. They may undergo inflammation, independently of the substance of the cord, and independently of the brain: but this is

not very common. Usually, when we have meningitis of the cord, we have the same disease also within the cranium: usually also, with meningitis of the cord, we have more or less inflammation of the nervous matter composing it. The commonest symptoms of inflammation of the meninges of the cord (for I do not pretend to speak of the several membranes separately) appear to be pains, often intense, extending along the spine, and stretching into the limbs, and aggravated usually by motion, and simulating therefore rheumatic pains: rigidity or tetanic contraction, and sometimes violent spasms of the muscles of the back and neck, amounting in some instances to perfect opisthotonos: a similar affection of other muscles also, as those of the upper or lower extremities: a sense of constriction in various parts, in the neck, back, and abdomen, as if those parts were girt by a tight string: a feeling of suffocation: retention of urine: obstinate constipation: and with these symptoms, rigors often.

You are not to expect all the symptoms which I have been enumerating in every case: they will vary according to the seat and extent of the inflammation. We need not wonder at the spasmodic symptoms, when we recollect that the nerves which issue from the body of the cord receive a covering from its pia-mater. The pain felt along the course of the spine itself is said to be aggravated by percussion of the spine, but not by simple pressure; and this seems very likely.

I know of no way in which I can so well hope to awaken an interest in you about these diseases, or to offer you instruction respecting them, as by instances. The following I take, abridging it somewhat, from Dr. Abercrombie. A man, twenty-six years old, had for several years been subject to suppuration of the left ear; suffering occasional attacks of pain on that side of the head, which were followed by a more copious discharge from the ear. In the first week of April he became ill, with pain of the forehead and occiput, disturbed sleep, and loss of appetite; but no fever. At the end of the week he complained of pain extending along the neck. This pain gradually passed downwards in the course of the spine, and deserted the head; and at last, after many days, it fixed itself with intense severity at the lower part of the spine; shooting thence round the body towards the crests of the ilia. He became affected also with great uneasiness over the whole of the abdomen, and with great pain and difficulty in passing his urine. About the end of the second week in April his sufferings had become extreme. He could not lie in bed for five minutes at a time, but was generally walking about the

house in a state of great agitation, grasping the lower part of his back with both his hands, and gnashing his teeth with the intensity of the pain. He had no interval of ease, and was sometimes incoherent and unmanageable. On the 16th he went to take a warm bath, walking down three stairs, and into an adjoining street, with little assistance. His speech afterwards became somewhat affected: there were convulsive twitches of his face, and difficulty of swallowing. Some transient squinting also was observed. The pulse was now very frequent. On the 18th, while sitting in a chair, he suddenly threw his head backwards with great violence, and immediately fell into a state of coma, in which he remained for two hours, when he died. During the whole disease there had been no paralysis, except the slight affection of his speech; no difficulty of breathing; no vomiting; and no convulsion except the twitching of his face the day before his death. The pulse was small and irregular. The bowels were easily kept open, but the pain in his back was much increased by going to stool. Two days before his death he had several attacks of shivering; and much purulent matter was discharged from his left ear during his illness.

Upon a very careful examination of his body, every part of the brain was found to be in the most healthy state. Some gelatinous deposition was found under the *medulla oblongata*; and purulent matter flowed, in considerable quantity, out of the spinal canal. The spine being entirely laid open, the cord was found covered with a coating of purulent matter, which lay betwixt it and its membranes. The matter was most abundant at three places; at the upper part, near the foramen magnum—about the middle of the dorsal region—and at the top of the sacrum: but it was also distributed over the other parts with much uniformity. The substance of the cord was soft, and separated into filaments in some places. All the other viscera were healthy.

You may find several interesting examples of this form of disease in *Ollivier's Treatise on the Spinal Marrow*. The prominent symptom was generally *pain*, referred to some part of the spine, and *increased by motion*; and what is curious, sometimes little complained of except upon motion. In general, also, it extended along some of the limbs, and was accompanied by muscular rigidity, or tetanic spasms. Palsy occurred in one case: but this seemed to have been owing to *softening* of the cord itself. Constantly there was increased sensibility; a circumstance which Ollivier thinks calculated to distinguish inflammation of the membranes from inflammation of the substance of the

cord, which is usually attended with *diminished* sensibility. In the case that I have quoted from Dr. Abercrombie, the intense pain underwent no remission or abatement. In one of Ollivier's examples, there was, at the commencement of the disease, a striking intermittence of the pain; it came on with intense severity at ten at night, and lasted till three in the morning.

The causes of spinal meningitis are not always to be discovered. It sometimes extends from within the cranium. It may be excited by external violence to the spine, of which a good specimen has been recorded by Sir Charles Bell:—A waggoner sitting on the shafts of his cart, was thrown off by a sudden jerk, and pitched upon the back of his neck and shoulders. He was taken to the Middlesex Hospital, where he lay for a week, without complaining of any thing except stiffness of the back part of the neck. He could move all his limbs with freedom. On the eighth day after his admission he was seized with general convulsions and locked jaw. After a few hours he was affected with a singular convulsive motion of the jaw, which continued in violent and incessant movement for about five minutes. This was followed by maniacal delirium. He then sunk into a state resembling typhus fever; and after four days was found to be palsied and insensible in his lower extremities. The day before his death he recovered sensation in his legs.

On dissection, a great quantity of purulent matter was found within the spinal canal. It appeared to have formed about the last cervical and the first dorsal vertebræ, and to have dropped down, by its own gravity, to the lower part of the canal; where it produced palsy and anæsthesia of the inferior limbs by the pressure it occasioned.

Inflammation of the *substance* of the spinal cord leads to the same changes in its texture which have been already spoken of as being often the results, in the brain, of inflammation of the *cerebral* matter. Softening—induration—suppuration. I need not, therefore, again describe the physical characters of these alterations.

The symptoms which flow from inflammation of the nervous pulp of which the spinal marrow is composed, are by no means uniform; nor can we expect that they should be so, when we recollect what has been already stated of the different effects that must ensue according as different parts of the cord happen to be implicated. The phenomena will vary likewise, according as the inflammation is acute or chronic. If we bear in mind how many parts of the body depend for their power of motion, and for

their sensibility, upon the integrity of the spinal cord, we shall not be surprised at the diversity and multiplicity of the symptoms that flow from disease of the cord. We shall expect to find, and we actually do find, some such an arrangement of those symptoms as the following. When the upper portion of the spinal marrow is inflamed, or partly disorganized, we look for convulsive affections of the head and face; inarticulate speech, loss of voice, trismus, difficult deglutition: as we go somewhat lower we should anticipate difficulty of breathing, irregular action of the heart, constriction of the chest: and proceeding lower still, vomiting, pain of the belly, sensation of a cord tied round the abdomen, dysuria, retention of urine, incontinence of urine, constipation, tenesmus, involuntary stools. And with respect to the voluntary muscles corresponding to these parts of the spinal marrow, convulsions, or palsy; or palsy succeeding to convulsions.

I must again have recourse to examples, to put you more fully than any attempted abstract picture could put you in possession of such forms of inflammation of the cord as you may expect to meet with in practice.

A man, 56 years old, was exposed to severe cold, while travelling on the outside of a coach. After this he was attacked with pain in the right arm and leg, most severe about the shoulder, but affecting the whole side, and he had also considerable headache. He soon perceived some loss of power in the affected limbs; and the progress of this was very curious. It began at the upper part of the arm, and extended downwards so gradually, that he was able to *write* distinctly, after he had lost the power of raising the arm, or bending the elbow. Then the leg became affected in the same gradual manner, and after ten or twelve days from the commencement of the disease, the whole leg and arm had become completely paralytic. Some pain continued in the parts, and it was occasionally severe, especially in the leg. Repeated blood-letting, and purgatives, and blistering, were employed. His mind remained quite entire. His pulse was 84, and rather weak. After some time the *left* arm became paralytic, rather suddenly; but it was not so completely motionless as the limb on the right side: the left leg was not at all affected. Slight delirium occurred, but passed off again. At the end of two months, after the exposure to cold, he again became delirious, and his pulse got feebler and rapid: he then fell into a state of stupor, muttering incoherently, but answering questions distinctly when he was roused. He lost his speech a few hours before death. For the last eight or ten days there had been considerable sloughing of the sacrum.

The brain was found to be healthy throughout. Much bloody fluid was discharged from the spinal canal into the cavity of the cranium before the spine was laid open. On displaying the spinal cavity itself, the cord was found in a state of complete softening, from the second to the last cervical vertebra. The portions above and below that part were quite healthy (*Abercrombie*).

Comparing this case with the one I detailed of meningitis, we find that pain was present in both, but more severely so in the case of inflammation of the membranes: we find, also, that *stiffness and spasm of the muscles* marked the *meningitis*; *palsy* the *inflammation of the substance of the spinal cord*. In neither of them were the intellectual functions disturbed till towards the last. I believe that the characters now pointed out belong to these forms of disease respectively.

I shall take, from the same store-house, one more case, in which both the membranes and the cord were simultaneously inflamed; and which, therefore, was analogous to encephalitis. And I quote it the rather because it possesses one or two points of peculiar interest. A young man, of unhealthy constitution, 18 years old, had suffered for some time from ulcers in various parts of the body, accompanied by exfoliations of bone from the leg, thigh, and sacrum. For several months before his fatal attack he had a sore on his head, as big as a shilling, with caries of the bone beneath it. At length he began to complain of pain in the loins, without fever. On the 2d of October this pain had increased; it was chiefly seated among the lower dorsal vertebræ, and extended downwards in the course of the ureter, with a frequent desire to pass urine. Then the pain descended lower, into the sacrum, and the symptoms referable to the bladder ceased. But soon afterwards pain in the belly came on, and numbness of the inner side of the thighs, and retention of urine; and in two days after this there was *perfect palsy* of both thighs and legs, *without loss of feeling*; retention of urine, and involuntary stools. He had still some pain in the lower part of the dorsal region. He died at length, on the 14th of October, having continued quite sensible till about six hours before. There had not been the smallest approach to a renewal of power of the lower extremities, but their sensibility remained. There was *palsy*, but no *anæsthesia*.

All was quite sound in the brain, except some old thickening of the dura mater in the neighbourhood of the diseased bone. In opening the spinal canal, some purulent matter flowed out, during the sawing, from about the middle of the dorsal region; and one of the vertebræ at that place was found

carious. There was an extensive deposit of flocculent matter, having a purulent appearance, upon the outside of the membranes of the cord. Bloody sanious fluid was discharged from beneath its dura mater, and its pia mater was highly vascular. The substance of the cord was found most extensively disorganized along nearly the whole extent of the dorsal portion. The *anterior columns* of this part were completely broken down into a soft diffuent pulp; on the *posterior part* the cord was *more entire*. When the whole cord was taken out, and suspended, it hung together by the posterior columns of the dorsal portion, while the anterior part of it fell off entirely, in a soft half fluid state. The parts above and below the diseased portion were quite firm and healthy.

The complete palsy in this instance, going along with the destruction of the *anterior columns*; and the persistence of the sensibility, the *posterior columns* being comparatively entire; invest the case with a remarkable degree of interest. We might infer from it that the anterior half of the cord is the channel through which the power to move the limbs is transmitted, while their sensations are carried along its posterior half; but we are checked from so concluding, by such cases as the one recorded by Mr. Stanley. These apparent inconsistencies may puzzle, but they ought not to discourage you. That time, and our advancing science, will at length explain and reconcile them, I cannot doubt. Meanwhile they teach a lesson which many practitioners much need—viz. that it is unphilosophical and unsafe to draw general conclusions from single cases of disease.

We are much instructed in regard to the effects of inflammation, or any other cause of disorganization, *confined to a limited portion of the cord*, by observing what takes place in those injuries in which the bones of the vertebral column are broken, or displaced. Of course I do not dwell upon these accidents, for they belong to surgery; but I have seen a good many of them, and watched them with much interest. The symptoms are much more uniform than when inflammation occurs within the vertebral canal, independently of external injury; simply because the injury to the cord is more definite and local. But such cases are very valuable objects of study to the physician. I remember several that occurred when I was a dresser in St. Bartholomew's hospital; and I will state very briefly the particulars of one, as an exemplar. In the year 1820, a man was brought there who had been thrown out of a tilt cart, in consequence of a dray running foul of it. He had pitched upon his head, which shewed however no

trace of injury. He became powerless, both in the upper and lower extremities, immediately on the receipt of the injury. His stools passed from him without his being aware of it, and it was necessary to use the catheter to empty his bladder. He breathed entirely by the diaphragm—that is, his thorax was motionless, and his abdomen rose and fell with every alternate act of inspiration and expiration. These symptoms are perfectly distinctive of injury to the cord between the origins of the phrenic and intercostal nerves. He suffered pain about the middle part of the neck behind. He went on exceedingly well for four or five days, and then the nurse very foolishly acceded to his request to be turned on his side, which caused his death in a very few minutes. This is not the only instance, by the way, in which life has been suddenly extinguished by similar imprudence. The lesson may be useful. There was another patient in the same hospital, who had fractured the spinal column about the cervical vertebræ. Among other remedial measures, the surgeon had directed that his head should be shaved. The barber had performed half his task, and was turning, with his hands, the unfortunate man's head into a more convenient position for completing it, when he suddenly expired. The twist was fatal to him.

On the examination of the body of the patient whose case I was mentioning, a very remarkable state of the spinal column was found. The fifth and sixth cervical vertebræ were dislocated from each other *without any fracture*: a thing which has sometimes been pronounced impossible. The articular processes were fairly separated; and the vertebræ were also forced asunder, by the detachment of one of them from the intervertebral substance. The nervous matter of the cord opposite the point of dislocation was quite soft.

There is one very common and distressing consequence of such disease of the spinal marrow as produces paraplegia, not particularly noted in any of the cases which I have related, but always to be looked for. The muscles, by means of which the bladder empties itself, are apt to participate in the palsy; and then the bladder empties itself no longer. The urine accumulates in it, and distends it, and the ureter even becomes distended; and in this way not only the present but the prospective danger is increased. For the foundation of future disease in the kidneys is often thus laid, even when such distension of the bladder by its retained contents occurs independently of any disease of the spine; as it may do from stricture; from enlargement of the prostate; or even from the voluntary reten-

tion of the urine beyond a certain period, through feelings of delicacy. You are to look out, I say, for this distension of the bladder, and relieve or prevent it by the introduction of a catheter through the urethra. You must not be deceived by being told that the patient passes plenty of water; that it even runs from him. Incontinence of urine is, in fact, in these cases, though it may sound paradoxical, a sign of retention of urine. The urine dribbles away because the bladder admits of no further distension; it overflows, and runs out at the natural orifice, but the bladder remains constantly full and stretched. You must make an examination, therefore, of the hypogastric region with your hand. If you find that part of the belly hard and resisting, and giving out a dull sound on percussion, you may be sure, in these cases (where there is paralysis of the lower extremities, and the water dribbles away), that the bladder is full, and has lost the power of expelling its contents. Sometimes you may recognize the fluctuation of the urine in the distended bladder, and ascertain the globular shape of that organ. It will rise even beyond the umbilicus. But what I chiefly wished to point out to you is, the circumstance that the bladder becomes diseased, and the urine altered in quality, under this state of palsy. The urine becomes thick, ropy, and alkaline, and exhales a very offensive ammoniacal smell; and the inner surface of the bladder is found, after death, to be thickened, red, and covered with adhesive mucus—in a state of chronic inflammation, in short.

CLINICAL LECTURES,

BY DR. CORRIGAN,

Delivered at the Hardwicke Fever and Whitworth Hospital, Dublin,

During the Session, 1840-41*.

LECTURE III.—(FEVER, No. 1.)

Present epidemic a primary or secondary disease? Cases of Geoghegan, Purcell, and of Walsh. Considerations on Pathology in relation to treatment, &c.

It has been observed by Sydenham, and indeed by every practical physician, that the type of fever is frequently changing; and that there is, for the treatment of that disease, no knowledge more desirable than an acquaintance with the epidemic constitution of the time being. Sydenham, one of the most accurate of observers, has further noticed, that when the type changed he was frequently very unsuccessful in his practice,

until observation made him acquainted with the nature and tendency of the new type setting in. We shall devote a sufficient number of our clinical lectures, commencing to-day, to an examination and attentive observation of our present epidemic fever, and endeavour to make our observations as purely practicable as possible, recollecting that the whole and final object of clinical medicine is the treatment of disease. The prefatory observations on the nature of fever I shall make as short as possible. The different opinions as to its nature may be divided into two classes; the first comprising those of, generally speaking, our own physicians, who consider fever as an essential or primary disease; and the second those of many celebrated schools and physicians, among whom is reckoned the celebrated Louis, who considered fever to have no existence as an independent or primary disease; but regard it as the result or consequence of a local disease; and of those holding this opinion, by far the greater number consider typhus as the constitutional disturbance produced by inflammation of the glandulæ aggregatæ of the intestines. Dr. Christison, however, states that Louis has latterly much modified his opinion; and has admitted that typhus may exist without the peculiar affection of the intestinal canal. I think it is hard to contemplate our cases of typhus without coming to the conclusion that our opinion in this country is correct, which views our typhus fever as an essential primary disease, and existing without any local structural lesion.

Let me just recal your attention to the case of Geoghegan, in No. 1. His disease was a well-marked specimen of our maculated typhus fever; we saw him comatose, lying on his back, with muttering delirium; sordes on his tongue and teeth, with a pulse of 132, a skin thickly maculated; with very great prostration of strength, and involuntary stools; with sensibility so much diminished that the eyelids remained fixed, and we were obliged to use the catheter to draw off his urine. A crisis took place; and in less than two days there was nothing to be contended against but debility; pulse became regular, tongue clean, and sensibility natural; sleep returned, and not a trace or symptom of local structural disease of intestinal canal remained. It is not possible to suppose, in such a case as this, that if those symptoms depended on structural disease, or local inflammation, they could so rapidly subside. It is not possible to suppose that local inflammation or ulceration could so rapidly disappear. We do not see, in ordinary cases, local inflammation, even of a slight degree, so rapidly subside; still less of so severe a form or nature as to give rise, according to

* In Dr. Corrigan's last lecture (vol. xxvii. page 907), for "naturalized cells" read "natural-sized cells."

the localizers, to fever, or to symptoms carrying with them such hazard of life.

Contrast, now, Geoghegan's case with Purcell's, in the same ward. In Purcell's there arose, in the course of the fever, an undoubted complication of disease of the mucous tissue of the intestinal canal; but how different has been the progress of the case. His illness has been prolonged, he has lost much flesh, the amendment has been very slow; relapses of diarrhoea and tormina have been frequent, and it is only at the end of some weeks from the termination of the fever that we can now safely pronounce him convalescent. We have, however, still more decisive evidence on this point. Honoria Walsh died on the fourteenth day of fever, with all the ordinary symptoms of severe maculated fever. The immediate cause of her death was sudden effusion on the brain and into the bronchial tubes. She died just at the time when the maculated fever was at its height; and had the fever been dependent on local disease in the follicles of the intestinal canal, we should have found it extensive, or in some degree corresponding either in extent or intensity with the severity of the fever. Instead of this, on a careful examination, we could only find one trifling patch of the *glandulæ aggregatæ* more slightly developed than natural; but neither redder than natural nor ulcerated. It would be obviously absurd to attribute to a local alteration so trifling as this such tremendous consequences as the production of typhus fever through the whole constitution. It would be out of place here, in a clinical lecture, to go into further proofs of the accuracy of our views of the essential or primary nature of fever, than are furnished by the cases falling, at the present time, under our immediate observation.

Supposing, however, we were even to admit that the local disease could be a cause of those constitutional derangements to which we give the name of typhus fever, let us inquire how far—for this is a very important practical consideration—how far this is to be permitted to influence practice. There is no reflection I know more likely to arise in the mind of the observer than this. Let us learn from pathology the local disease or injury that causes the symptoms or constitutional disturbance. We may then disregard the latter; it is only an effect. All we have to do is to cure the former; when the cause is removed the effect will cease. This sounds very plausible; but the man who would act on it would make a very bad physician. There is no aphorism more true than this: that you may cure a disease and kill a patient. A patient may die cured. Admitting local disease to be either incidentally present in fever, or even going farther, and

granting it, for the sake of reasoning, to be *the disease*, of which the general fever is but the consequence; still, when you look at a patient, in fever, even if you take this view, you must not forget that there is also the constitutional derangement, and the local alteration, to be taken equally into account, and that even though the constitutional derangement be but an effect, it will often require more of your consideration than the local disease. What would you think of a surgeon who, in a case of calculus in the bladder, or diseased extremity, in which the strength was gone, and constitution shattered, by irritative fever, would proceed at once, utterly regardless of the state of the constitution, or general functions of the system, to operate on this principle, arguing thus: that as the constitutional derangement was only the effect of the local injury, he should therefore disregard altogether the constitutional derangement; that it was only an effect; that he would therefore direct, at once, his whole energy to the removal of the acting primary cause, in the expectation that the removal of the effect would follow. You know what the result would be: his patient would die cured. Now the very same fault would be committed by a physician who, standing at the bed-side of a patient, in typhus fever, would see nearly all the important functions of life deranged, would nevertheless disregard the state of the system generally, and overlooking all this, would fix his attention exclusively on some local alteration, and direct his efforts alone against that. *His patient, too, would die cured.*

Thus, if we even admit the pathology of fever to be, what many of our continental physicians believe, a local disease, we must still be carefully on our guard that this pathology shall not lead us into a dangerous error in treatment. If we admit the existence of local disease in fever even, as an incidental occurrence, or as the primary cause, still let us keep this consideration in our mind:—There is local disease, and there is disturbance of vital functions; from which of these two is the greater danger at the present moment to be dreaded? It may happen that measures against both may be combined; but if not, then it is obvious that if some one or more of the primary functions, on the continuance of the action of which even life depends, be so deranged as to threaten rapid wearing out of life, even though that derangement be but an effect, it claims for the time our first care. It is not from the amount of structural change in the intestines, even admitting this to be the cause of typhus, that a patient dies; it is from the derangement of some one or more of the vital functions. To remove or repair that derangement—to restore or support those

primary functions, then, our efforts in fever must most often be directed, no matter what our views of the pathology of the disease may be. From pathology we cannot deduce our treatment of fever. We have much more than the local pathology (even supposing such to exist) to contend with, and a consideration of the attempts at definition of fever will fix this still more strongly on us. Attempts have been made to define fever. There is great difficulty in the way—a difficulty similar to what exists in the attempt to define inflammation. There can be no definition of inflammation, because what we call by the name is an appearance or phenomenon produced by the lesion of several primary vital functions, of circulation, of secretion, of innervation, &c. These functions are possessed in different proportions by different organs; and hence the phenomenon will present peculiar features, according to the organ inflamed; and not only this, but these different primary functions will even in the same tissue be affected occasionally in different proportions, so that the characters belonging to inflammation will vary even in the same tissue. So it is with fever; several most important vital functions are simultaneously deranged. The cerebral and spinal functions, the respiratory and digestive functions, the functions of circulation and secretion, are affected; but the proportions in which they are deranged will, perhaps, not be the same in any two individuals; and hence the definition, or even the description which applies accurately to one case may not be suited to a second. The fever will derive its most distinguishing character from the function or the functions in which there is the greatest departure from the healthy state. If this cause of difficulty in the way of attempting a definition of fever be admitted, although it will follow that a definition cannot be achieved, still the disease will be easily recognized, and an understanding will be attained of the variations which the disease presents, and which are almost as varied as the individuals attacked. But although the cases may differ from one another in the way described, still they will all agree in this, that all are characterized by disturbance of most of the vital functions without primary local disease. We may have the circulation little disturbed in a case; but, then, we shall have much disturbance in the cerebral and nervous functions. We shall have in another case comparatively little disturbance of the cerebral functions; but there is considerable departure from health in the state of the circulating and secreting functions; and a third case may present its most prominent features in derangement of the digestive functions; but, just as in inflammation, the functions whose sum of derangement constitutes inflammation are so intimately

connected, that, let the first alteration commence in what function it may, all will be soon implicated if the first derangement be continued for any length of time. So it is with fever. The fever may commence, as in common inflammatory fever, with derangement of the function of circulation; but it lasts for a short time only before cerebral and respiratory functions are more seriously engaged; or, as in our present maculated fever: let the disease commence with derangement or depression of cerebral and nervous functions, and, although there is at first little alteration in the circulation, still, if we watch it from day to day, it will become more involved, until at last its disturbance or departure from a healthy state may demand as much attention as the primary derangement of nervous function.

Thus we can, I think, form a correct idea of fever for practical purposes: if, as we stand at the bed-side, we consider it as a disturbance of the whole, or the greater number, of the vital functions:—the vital function, whatever it may be, that is most affected, stamping the peculiar feature on the individual case, and requiring the most attention, always, however, recollecting that from the intimate relation chaining, as it were, those vital functions together, an aberration from health commencing in one will soon run through the whole, and then the fever will present itself in its most terrific form, when cerebral, circulating, respiratory, and digestive functions are all simultaneously and in equal amount deranged. Having now explained the view in which I would wish you to look upon fever, we are, I hope, the better prepared to proceed with our practical considerations.

ON THE COLOURLESS GLOBULES, AND OTHER ELEMENTS OF THE BLOOD.

To the Editor of the Medical Gazette.

SIR,

IF we very narrowly watch the coagulation of a drop of liq. sanguinis, placed between two slips of glass, we first observe the colourless globules swimming in it; and then the formation of slightly opaline streaks and films, which envelop and draw together a great many of the globules; ultimately forming a thin, diaphanous, tough membrane.

If a mass of fibrin, resulting from the coagulation of the liq. sanguinis (Case 9, p. 692, Jan. 1841), be cut into thin slices, and macerated in clean

water for a day or two, it becomes of a pure ivory white; and when examined with the lens, or in the microscope, its texture is seen composed of the minutest granules, intersected by lines, fibres, and filaments, having the toughness, cohesion, and elasticity of organized membrane.

By maceration in caustic potass (liq. potassæ) it becomes in two or three days of a reddish brown colour, and the liquid is tinged of the same colour; it also loses much of its cohesion, and becomes more brittle, breaking short and easily.

If another slice is macerated in strong acetic acid, in a day or two the opaque ivory-white is entirely changed; the fibrin swells a little, and, without losing its figure, becomes quite transparent, forming a clear jelly, which has still more elasticity than the original fibrin, and quite as much cohesion. If a portion of the transparent gelatinous mass resulting from the maceration of fibrin in acetic acid, be examined, it still shews a multitude of fibrous-looking lines and filaments, evincing organized structure; these are mingled with other lines, which result from thicker portions of the mass being pressed together over each other; but the organized fibrous filaments may, by a little practice, be easily distinguished.

When serum is slowly coagulated by heat alone, particularly if about a fourth of its bulk of water be previously added, it forms a soft semi-opaque solid, somewhat resembling soft blanc-mange. If a very minute portion of this coagulated albumen be examined by the lens, pressing the portion quite flat between two pieces of glass, it appears in transparent films or flakes, crossed by little lines, which are caused by the thin films lying over each other. The films are very brittle, having only a slight cohesion or tenacity, and no elasticity, and presenting the same appearance that any other semifluid transparent animal jelly would do. If a drop or two of vinegar be added to the serum before the coagulation; or if the heat be more quickly applied, and no water added, the coagulation is more complete, the mass is more opaque, and is then composed of an innumerable quantity of variously-sized granules and globules, mixed with a few of the transparent flakes; and on pressing

or gently rubbing the two slips of glass together, with a very little of this coagulated albumen between them, these several objects are rendered very distinct by the lens.

On placing a little coagulated albumen in caustic potass, it is entirely dissolved in two or three days, and the fluid is tinged reddish brown. By acetic acid the blanc-mange appearance is altered, and the semi-transparent portions become more opaque.

From the chemical relation and ultimate analysis of fibrin and albumen, they are no doubt modifications of the same animal principle, yet physically, or mechanically and physiologically, they differ most materially.

A minute film of fibrin is not separable into smaller portions, nor is it to be resolved into distinct granules or globules by the firmest pressure, nor by rubbing the slips of glass against each other; repeated washing does not remove any thing from it, nor is it altered by boiling. On the other hand, coagulated albumen is friable, brittle, and has the slightest possible cohesion; the slightest pressure between the slips of glass breaks it down, and resolves it into a multitude of extremely minute detached granules and globules, and flakes, of various forms and sizes.

The blood can hardly be said to be organized, yet, from the physical characters of coagulated fibrin, it is evident that while circulating in the body it holds in solution a peculiarly endowed animal principle (fibrin), requiring only a moment's rest to constitute solid organized (I do not say vascular) tissue. The texture, formation, cohesion, toughness, elasticity, and unalterable condition by boiling, are all proofs of its organized nature; and it at once, without further aid or assistance, assumes all these properties even after its removal from the body in venesection, or after death in its own vessels.

There seems, then, to be but one step between the delicate microscopic capillaries and the fibrin of the blood. The former are minute fibrinous canals, carrying blood; the latter may (not inappropriately) be termed organized tissue in solution, circulating with the blood. Fibrin, then, is the connecting link between the solids and the fluids. As in other departments of

nature we find the most gradual and imperceptible transitions, so it is with the solids and circulating fluids of the living body. The capillary tufts are carried out to such an extent of minuteness and subdivision, that they lose many of the physical characters of a solid; while the blood, circulating with force and rapidity through all these delicate channels, can hardly be preserved from depositing the organized (or organizable) material it so abundantly contains.

Putting out of consideration the minute saline, and other elements of the blood, we may consider this fluid as composed of red globules—a highly organizable and vital form of albumen (called fibrin); and non-vital, or much lower organizable (or organized) albumen. As soon as the blood has escaped from its vessels, or as soon as it ceases to move, the vital albumen becomes solid, by virtue of its vitality assuming an elastic, tough, membranous form; while the non-vital, or less vital albumen, remains in solution with the watery part of the blood, and constitutes the serum. If serum be made to solidify by the application of external agents, such as heat, acid, &c. it forms merely a multitude of irregular incoherent granules.

Fibrin forms the staple material of the elastic tissues of the body—muscular fibre, cellular tissue, and blood-vessels; the interstices and meshes being filled up by the watery portions of the blood, and regular globules of coagulated albumen: for when nature solidifies this material to contribute to build up organized tissue, it is effected by the formation of regular globules, of various sizes.

The agents, therefore, at work in building up the body, and also in the formation of abnormal structure, are the capillaries, the fibrin, and the albumen. The red globules are never found in any normal secretion—except perhaps menstruation—nor are they, in health, in any part out of their proper vessels; their office, therefore, whatever it may be, whether in aiding the endosmosis of the several secretions, or in keeping the fibrin fluid, is confined to the blood; they have nothing to do out of the blood-vessels.

The capillaries constitute the extreme parts, or the limits, of the solid

structures. The albumen is presented to us in solution, in the watery parts of the blood; while the fibrin, which solidifies as soon as it escapes from the channels of the circulation, is peculiarly the connecting link between them. All normal structure is built up by the capillaries, by the fibrin and albumen; and all morbid or adventitious growths and deposits may be classified as more peculiarly belonging to one or other of these important agents. Thus bronchocele, nævus, and scirrhus, arise from too active a state of the capillaries; they branch out, or shoot forth, and organise a mass which is not only not required, but which is prejudicial and destructive of health and life. In inflammatory deposits of lymph, false membrane, and morbid adhesions, we find the fibrin the sole or principal agent; whereas, in tubercle, we recognise the globular, incoherent form of albumen.

In some of these instances the capillaries retain their activity and action for a long period of years, as in bronchocele, and a few other tumors, without any material interference with any important vital function. In other cases, after a pretty active display, for a time they fail in their action, and their abnormal increase is in some places stopped, and they give way, and the skin, or normal structures which they before supported, dies. Hence results the ulceration in cancer, fungous hæmatodes, and other sores. Sometimes in these singular formations there are alternations of their growth, vigour, and decay; or, side by side, the two actions may be going on at the same time: we have then destructive ulceration, as it is called, and fungous growths, alternating with each other; or even in the same sore a fungous capillary tuft may shoot forth in all its activity, forming an irritable red fungus, and yet be surrounded by other decaying tufts forming phagedenic, or cancerous ulceration, where the elements of the blood, in the form of mucous and pus, escapes, or even the blood itself is poured out from the open ends of the decaying capillaries.

WILLIAM ADDISON.

Great Malvern,
March 14th, 1841.

CASES OF
PTOSIS AND ECTROPIUM,
COMMUNICATED BY T. B. CURLING, ESQ.
(For the Medical Gazette.)

*Double Ptosis and Divergent Strabismus
from Palsy of the muscles supplied by
the third pair of nerves.—Ptosis on one
side cured by operation.*

A stout gentleman, æt. sixty-four, a captain in the merchants' service, who had passed forty years of his life at sea, came under my care in January 1841, on account of ptosis of both superior palpebræ. The lids hung loose and motionless over both eyes, and so completely obscured the patient's vision that he was constantly obliged to raise one of them with his fingers in order to relieve himself from blindness. He was also affected with double divergent strabismus, and had lost all power of directing the eyeballs in any other direction, the distortion being fixed and permanent. These symptoms were evidently occasioned by paralysis of the levatores palpebrarum of the inferior oblique, and of the superior, inferior, and internal recti, muscles supplied by the third pair of nerves. The superior oblique, likewise, appears to be paralysed. This affection came on gradually after a fit, and had existed for four years. He had since experienced several slight fits of the same kind; and was subject to occasional twitchings of the muscles of the limbs. Otherwise he enjoyed good health, and had no other symptom of paralysis. He was not subject to pains in the head, and his vision was sufficiently good to enable him to read a moderate sized print. He suffered, however, very great inconvenience from the ptosis, being obliged, whenever he moved about, to keep his hand constantly raised to his head in an awkward and fatiguing position, in order to enable himself to see his way. The paralysis of the affected muscles was so complete, and of so long duration, that I could anticipate no benefit from medical treatment; and consequently none was adopted. I noticed that he constantly made the attempt to raise the lids by putting the occipito-frontalis muscle into strong action, and that, in consequence, the eye-brows had become

much elevated and unusually arched. It occurred to me that if the superior palpebra could be brought under the influence of this muscle the ptosis would be removed, and that he might thereby regain the power of uncovering the eye. The patient being very willing that something should be done for his relief, I performed the following operation. The integuments between the eyebrow and lid, which were very abundant, were raised with the thumb and forefinger of my left hand, and those of an assistant, to such an extent that it was just possible for the patient to close the lids, when, by a single stroke of the scissors, the skin included between our fingers was removed, leaving an elliptical wound which extended from a little below the eye-brow to about one-third of an inch from the margin of the lid and the whole width of the palpebra. Scarcely any bleeding ensued. The edges of the wound were approximated and retained by two sutures, and the part united by the first intention, and perfectly healed in eight days. No ectropium or deformity resulted. The patient afterwards could completely close the lids, and, as was anticipated, could raise the superior one, and uncover the eye, simply by the action of the occipito-frontalis muscle. The vision of the right eye being the most perfect, the operation was performed on that side, and as I found that the sight was confused, and giddiness produced by exposure of both eyes, owing to want of correspondence between the globes, I thought it better not to meddle with the other side. In consequence, also, of the atonic and paralytic state of the other recti muscles, I considered the case unfavourable for the cure of strabismus by division of the rectus externus. The patient, therefore, was obliged, in walking, and at other times, to turn his head a little sideways, an inconvenience to which he soon became reconciled.

A somewhat similar operation has, I believe, been performed by Mr. Hunt, of Manchester, in a case of traumatic ptosis of one lid, consequent upon a division or laceration of the levator palpebræ muscle; but I am not aware of its having been practised in any case of paralytic ptosis. The above case shews, however, that the operation is equally applicable to the latter form of

the affection, and that it may be undertaken with a confident reliance on the occipito-frontalis muscle being afterwards adequate to elevate the drooping lid.

Case of Ectropium consequent on the contraction of the cicatrix after a burn, cured by operation.

A servant girl, aged sixteen, applied to me for removal of the deformity produced by eversion of the upper lid of the right eye. She stated that six years ago she had met with an accident by which she received a severe burn on the face and scalp, and afterwards, as the sore healed, the lid gradually turned out. There was considerable eversion of the lid, chiefly towards the outer angle, and the mucous surface presented a prominent granular appearance of a deep red colour, which was very disfiguring to the countenance. The skin above was very tense, the eyebrow was completely destroyed, and there was an extensive cicatrix on the forehead, right temple, and side of the face. The integuments, however, were not adherent to the frontal bone. She was unable to close the lids completely, so that she was constantly subject to attacks of conjunctival inflammation from the effects of wind and dust. On this account, and as the unpleasant deformity rendered her such an object of aversion to her fellow servants, that she found great difficulty in getting into service, she was very anxious to undergo an operation for relief. I made a semilunar incision through the integuments along the upper and outer edge of the orbit, so as completely to set free the palpebra. The upper lid could not, however, be restored to its natural position, or brought into contact with the lower, owing to the protrusion of the granular and thickened conjunctiva. This membrane was freely excised with a pair of curved scissors, in the performance of which a small portion of the tarsal cartilage was cut away. The lid then covered the eye in a natural manner. After the bleeding had ceased the external wound was dressed with dry lint. At the end of a week both wounds had assumed a healthy granulating appearance. The healing of the external one was checked by the application of a plaster made of cantha-

rides. Notwithstanding this, both wounds completely healed in sixteen days after the operation. The cicatrix in the conjunctiva fully succeeded in counteracting the contraction externally, and in preventing the return of the eversion. The girl was consequently greatly improved in appearance. The lids could be brought much closer together, sufficiently so as to prevent annoyance from foreign substances coming in contact with the eye, but could not be perfectly closed; the margin of the upper lid having assumed somewhat of an arched form, owing to previous elongation. This, however, was diminishing when the patient returned to service.

The particulars of this case may prove interesting, as that form of ectropium occurring after burns is one of the most difficult to cure, and as the operation which succeeded was a very simple one. I at first contemplated removing a wedge-shaped portion of the eyelid, in order to counteract the morbid elongation, but afterwards abandoned it; and I am inclined to think that this proceeding may often be spared; and that in many cases, as in the above, after the removal of the eversion, and obviating the causes tending to produce this deformity, the lid will gradually resume its natural form.

In connection with the subject of ectropium I may detail the following case:—

Frightful deformity and destruction of both eyes from double ectropium, consequent upon cicatrization of an extensive burn.

A poor Irish woman has long been in the habit of visiting the London Hospital under the following painful circumstances:—She has been subject to fits since her infancy, and some years ago during an attack she fell into the fire, and her cap and clothes catching fire, she sustained a severe burn on the head and face. The greater part of the scalp, including both eyebrows, was completely destroyed. After the separation of the sloughs, as the wounds began to heal, the eyelids on both sides became gradually everted. This was particularly the case with the upper lids, which were remarkably stretched and drawn upwards, to compensate for the extensive loss of the cutaneous

tissue on the head. Inflammation of the conjunctiva was necessarily the result. In the right eye, the inflammation extended to the interior of the globe, caused suppuration, and the destruction of the organ : the collapsed tunics of which still remain at the bottom of the exposed orbit. In the left eye, conjunctival inflammation has produced prominent staphyloma of the whole cornea, and entire loss of vision. The destruction of the scalp was so extensive, that the parts remained unhealed for several years, presenting a suppurating sore, which assumed sometimes a healthy character, and at other times an unhealthy and ulcerative surface, according to the varying states of the patient's health. The cicatrix on the head presents a pale, smooth, glistening appearance, and is extremely tense. The exposed conjunctiva of the lids of both eyes has a red granular aspect, and is constantly discharging. The everted upper lid forms a red conical patch above the orbit on each side of the forehead. It may easily be conceived that the deformity consequent on the state of parts just described is of the most hideous and painful character. In addition to loss of vision, the poor woman is a severe sufferer in other respects. She continually experiences a distressing sense of constriction in the cicatrix, which sensation is increased whilst the part remains healed. So forcible, is the contraction that the surface of the skull beneath presents a number of irregular projections, owing to partial atrophy of the bone from pressure. The sore on the head occasionally ulcerates, and is rendered painful, and the unprotected conjunctiva is constantly subject to become irritated and inflamed from the contact of foreign bodies, notwithstanding the means taken to obviate these annoyances. Her general health is a good deal impaired. Such has been her state, with little variation, for seven years, during which she has been under my observation ; and it has rarely been my lot to witness a more truly distressing case, in which so little could be done in the way of relief.

1, Mount Place, London Hospital,
March 6, 1841.

ON SUBCUTANEOUS OPERATIONS IN SURGERY.

BY ROBERT HUNTER, M.D.

Professor of Anatomy, Andersonian University,
Glasgow.

[*For the Medical Gazette.*]

THE range of subcutaneous operations has, till lately, been much circumscribed, extending chiefly to the section of a few of the muscles of the body, and to the evacuation of certain abscesses and cysts ; but such operations appear to me of immense value in surgery, and worthy of a more extended and diversified application. The advantages of the subcutaneous plan of operating, over the common method, may be comprised in the following particulars: 1st, less infliction of pain; 2d, less subsequent inflammation; 3d, no suppuration; 4th, little or no constitutional disturbance; and, 5th, a rapid return of the parts operated upon to their normal condition.

We all know the dangers attending the incautious opening of a psoas abscess, and the advantage to be derived from the partial application of the subcutaneous plan, as recommended by Mr. Abernethy ; and those who have seen the subcutaneous section of the dorsal muscles, as performed for lateral curvature of the spine, and watched the progress of such cases, can speak of its paramount superiority. I have now performed the last mentioned operation twenty-five times, and watched carefully the results, and although, in five of the cases, the latissimus dorsi, the serratus posticus inferior, the longissimus dorsi, and sacro-lumbalis, were divided, and the subcutaneous incision extended from the spinous process of one of the lumbar vertebræ across to the outer margin of the sacro-lumbalis, dividing the above-mentioned muscles, yet the pain was so trifling that the majority of patients hardly murmured. The flow of blood never exceeded a few drops. The skin over the track of this long and deep incision never changed its natural temperature or colour ; in no case did suppuration follow ; constitutional disturbance of any kind never once supervened, and in three or four days after the incision the puncture had cicatrized, the swelling from the effused blood had generally disap-

peared, and frequently by that time not a vestige of the operation could be discovered. During the time I sojourned in Paris last summer, M. Jules Guérin operated upon a patient affected with ankylosis of nearly all the body, arising from contraction of the muscles. He cut across forty-four muscles, and spread out the limbs, and although the successive incisions occupied more than sixty minutes, yet the system of the patient received so small a shock, that the pulse was not sensibly affected, and the patient slept soon after the operation, and neither irritability nor febrile excitement followed.

I attribute the mildness of the effects of subcutaneous operations to two causes: 1st, to the exclusion of the atmospheric air, and, 2d, the small injury inflicted upon the highly organized and sensitive structure of the skin.

That the atmospheric air will excite irritation and suppuration on the surface of a wound is a matter of daily observation. When the cuticle only is removed from a blistered surface, an immediate accession of pain is the consequence, and the serous effusion is also speedily changed into the purulent. If the atmospheric air, then, is capable of acting as a source of irritation, and if a wound of the skin is the more painful and the more dangerous as it is the larger (and of this last there can be no doubt), it follows that, in the performance of subcutaneous operations, regard should be had to two circumstances—1st, the puncture through which the instrument is carried under the skin should be as small as is compatible with the due performance of the other steps of the operation; and, 2d, means should be used for preventing the accession of atmospheric air both during and after such operations.

In the further prosecution of this subject, I shall shortly advert, 1st, to the cases in which subcutaneous operations have already been advantageously applied; and, 2d, to those affections to which, I think, they may with propriety be extended. The first division may be said generally to include two classes of morbid affections: those on which the muscles or their appendages are chiefly concerned or implicated, and those in which fluids require to be evacuated; and the second division will probably, at no distant

period, be found to include a long, but at present indeterminate, list of diseases.

1st. *Diseases which have been treated by the subcutaneous operations, and for which this plan of treatment has been eminently successful.*—At the very top of this class I would place such affections as require for their treatment the section of muscles or their tendons. The muscles are peculiarly adapted for the subcutaneous operations, their situations, forms, and relationships, are so easily determined; and when their section is required, the resisting nature of the texture to be divided, render all such operations, even though under the skin, at once safe and simple. Such sections are now in daily use in wry-neck, certain curvatures of the spine, muscular stiff joint, club-foot of every kind, and I must also add strabismus; for although the operation for that affection is at present performed without regard to the subcutaneous principle, still it cannot be viewed as a perfect operation till that principle is adopted, which, I think, it is likely to at no distant period.

I believe I was the first in this country to perform the subcutaneous section of the dorsal muscles for the cure of lateral curvature of the spine, and as I have now had some experience upon this subject I shall lay the result of my observations briefly before your readers. In no instance has the operation of *itself* produced a cure; but in all the cases on which I have operated, with one exception, it manifestly placed the patient in a more favourable state for the performance of a cure. The operation itself appears to me to effect no more than to take off, either in part or whole, the power of muscles that are interested in maintaining the curvature, and thus placing the spine in a condition to be more easily influenced by mechanical and physiological causes.

The cases which have been treated by me have all been of long standing, none less than seven years, and some ten, sixteen, and twenty years, and all with considerable torsion and gibbosity, as well as lateral curvature. In fact, they were cases which were either considered absolutely hopeless, or on which medical skill had been exerted for years without the slightest benefit. In some instances the section of the muscles was instantaneously followed by

an obvious improvement in the state and appearance of the back; in other instances I could discover no change whatever.

I perform the subcutaneous section of the dorsal muscles at four different places of the back. 1st, I weaken the tension of the deepest seated layer of muscles—that formed by the multifidus spinæ—by dividing the thickest part of that muscle, as it lies comparatively superficially upon the dorsum of the sacrum, opposite the posterior superior spinous process of the ilium. 2dly and 3dly, I remove the tension of the middle layer of spinal muscles, that formed by the longissimus dorsi and sacrolumbalis, by cutting these muscles across, sometimes in the lumbar region and sometimes in the costal region, according to the circumstances of the case; but more frequently in the lumbar region, near the origin of these muscles. 4thly, to destroy the tension of the flat and more superficial muscles, I divide these muscles by a longitudinal incision, close to the spinous processes of the vertebræ, at the place where the tension of these muscles appears to be the greatest. In one instance I cut through, with considerable effect, the *latissimus dorsi*, at the side of the chest, and consequently at some distance from the spine. The muscle crossed the contracted and concave side of the trunk, and appeared to be accessory in huddling in the ribs of that side. When the patient attempted to elongate that side, a cord, as thick as the little finger, was seen stretching from the crest of the ilium to the scapula; as soon as this rigid cord of muscle was cut through, the ribs became less huddled together, and the side could be elongated to a much greater degree, and the spine materially affected.

In cutting the dorsal muscles I have chiefly, though not exclusively, followed the method of Guérin. Guérin employs always two instruments; I have frequently employed only one: Guérin uniformly cuts the muscles by commencing at the cutaneous surface of the muscle, and proceeding deep-seatedly; I have repeatedly and with greater safety cut in the opposite direction, that is, by carrying the instrument more deep-seatedly than the muscle, and cutting towards the skin. As Guérin, however, is a great authority

upon this subject, his mode of operating is worthy of being mentioned. He makes the subcutaneous incision with a narrow sharp-pointed bistoury, about two inches and a half in length. He then withdraws that instrument, and introduces into the track thus made a sickle-shaped instrument of the same length and breadth as the former, the cutting edge of which is on the convex margin. Having carried this instrument across the muscles to be cut, he turns the convex cutting edge to the muscles, and their division is effected not by one cut but by a number of successive touches, the patient at the same time calling his dorsal muscles into action by throwing his head forcibly backwards to impart a greater degree of resistance to the muscles, and to enable the incision to be effected partly by the pressure of the contracting muscles upon the edge of the knife.

The cutting of the dorsal muscles is only the first, though an important step in the treatment of spinal deformities. The means that are afterwards employed in conducting such cases to a successful issue are both mechanical and physiological. The first consists in the application of pressure, made in various ways and by various means to assist in the gradual return of the parts to their natural places, and the second, without which the first would be useless and unavailing, consists in infusing power into the muscles which have become weak or dormant from disease, by simply calling these muscles frequently and in various combinations into action.

If I mistake not, I was the first likewise in this city who performed the subcutaneous section of the muscles for the cure of club-foot. I have seen the operation performed by M. Guérin for valgus varus and equinus, and with astonishing success. I have performed the operation on three patients, all of whom were affected with varus. Two of these patients were infants, and the third a female about thirty years of age, who had been deformed from birth. The sections of the tendones-achilles in the infants enabled me at once to bring down the heels, and to place and maintain the feet in their natural position by an appropriate apparatus. In the adult the deformity was very great—a varus,

as Guérin would express it, in the third degree; that is, the heel was raised, the foot thrown transversely inward or to the opposite foot and laid upon its fibular margin, and lastly the toes turned into the sole of the foot, and the foot itself curved from the great toe towards the heel.

In this case I first cut through the tendo-achillis, which permitted the heel at once to descend; I then divided the tibialis anticus muscle, which was extremely rigid and short, the section of which was attended with a very loud crack. The division of this tendon did not, however, effect so much benefit as I was led *à priori* to expect. The foot still continued turned much inwards, and the gathered-up state of the foot itself not at all improved. To overcome the still remaining obstacles I cut through, by one subcutaneous incision in the plantar aponeurosis, the abductor pollicis, the flexor brevis pollicis, and the long flexor of the great toe, and the effect was immediate and remarkable. The foot, which before had as much the appearance of a closed fist as of a foot, at once assumed its natural character. The toes were turned forwards, and more spread out; the heel down, and the sole of the foot flat, and turned to the ground. The patient, delighted with the effect of this last incision, exclaimed, "Doctor, this is the best cut of all!" After the operation, Mr. Campbell, surgeon, in Calton, to whom the case belonged, and who assisted me in the operation, placed the foot in a very simple apparatus which he had previously prepared, but which fulfilled all the indications required in the treatment of such cases, viz. to maintain the foot fairly upon its sole, with the heel down, the toes turned forwards and a little outwards, with the free play of the ankle-joint. It is now seven weeks since the operation was performed, and the case I am enabled to state is progressing most satisfactorily.

The evacuation of fluids enclosed in cysts has also for some time been effected upon the subcutaneous principle, and probably the practice is applicable, in a greater or less degree, to encysted tumors of every kind; psoas abscess should invariably be evacuated in this way, and circumscribed abscess of a specific or unhealthy nature might be treated in a similar manner. My

friend and colleague, Mr. Watt, has for some years been in the practice of evacuating the matter of bubo on this principle. He introduces a narrow lancet through the sound skin at a little distance from the bubo, and carries the instrument under the skin till it nearly touches the abscess. He then withdraws the lancet, and carries into the track thus made a grooved needle, with which he penetrates the sac. The matter then flows along the groove of the needle till the abscess is sufficiently emptied. The needle is now withdrawn, and a small bit of gold-beater's leaf accurately and elegantly closes the external aperture. In this way the matter may be removed as often as required, followed by no ulceration of the integument, no open and intractable sore, and no subsequent unseemly cicatrix.

Ganglia and melicerous tumors have also for some time been treated by the subcutaneous operation, both with safety and great effect. Dr. Cumin, formerly of Glasgow, was among the first to recommend the adoption of this practice. His plan consisted in introducing a cataract-needle under the skin for some way, then transfixing such tumors, and pressing the fluid into the surrounding cellular tissue. The cyst may thus not only be transfixed in different directions from one puncture of the skin, but its inner surface freely scarified, or deeper incisions made, so as to destroy the integrity of the secreting membrane, and give to the fluid an easy exit into the surrounding cellular texture.

By an easy gradation a similar practice may be extended to tumors of various kinds, particularly those of a simple and non-malignant nature, dependent for their vitality and growth on the surrounding cysts. Thus in an adipose, atheromatous, or steatomatous tumor, the introduction of a cataract-needle, so as to destroy the natural cohesion of its parts, or, as some would express it, its organization, and break up at the same time its cyst, might be the means of procuring the death and ultimate absorption of such a tumor, and thus save the pain and disfiguration of extirpation. Some years ago, the late Mr. George Macleod, of this city, stated, that he had been successful in dissipating tumors of the eyelids in this manner, and if I mistake

not, he proposed to apply the practice to tumors of every kind. I do not think this practice should be resorted to in carcinomatous or malignant tumors of any kind; total extirpation of the diseased mass is then indispensable, though unfortunately not always a permanently successful mode of cure.

2d. *Diseases to which the subcutaneous mode of operating may be extended.*—I am strongly impressed with the idea that, as surgical science advances, the principle upon which subcutaneous operations are performed will be more favourably regarded, and the operations themselves, under various forms and modifications, successfully applied to the treatment of numerous and diversified surgical affections. At present I can venture only to refer to a few morbid conditions which, from analogy, or the nature and structure of the parts proposed to be operated upon, appear to me adapted for such an operation.

1st. Subcutaneous incisions might be used in the treatment of inflammation of the fibrous membranes of the body at any place where these membranes are situated, from the inner surface of the skin to the exterior surface of the bones. In inflammation of these membranes, or in the parts immediately under them, the pain and intensity of the inflammation are subdued by incisions through these membranes. This is well known, and has been long acted upon. But the practice of the present day is to involve also the skin in the operation. In fascial, and subfascial inflammation, as well as in periostitis, and all diseases of this kind, we make a free incision through the skin; not that the skin is the seat of the disease, or even materially affected, but apparently for the purpose of reaching the seat of the disease; as if the necessary incision through the deep-seated textures could not be effected without first seriously injuring the cutaneous investment. The object of these incisions is twofold; to unbridle or remove tension from certain parts, and to unload the vessels of the textures affected. Both of these objects could obviously be gained by the subcutaneous operations. In fascial and subfascial inflammations, in periostitis, paronychia, and other diseases implicating directly or indirectly the white fibrous membranes of the body, I would propose, then, to incise these membranes by introducing a narrow

sharp-pointed bistoury, or other instrument adapted for the purpose, under the skin—an operation which can in all cases be easily performed, which would occasion little pain, which would fulfil the indications required, and produce no external disfiguration. 2d, In inflammation of the skin itself these operations might be useful. In phlegmonous and circumscribed erysipelatous inflammations, in particular, this treatment might be adopted. Instead of the application of leeches to the skin—instead of numerous punctures with the lancet, or long incisions through the skin, as recommended by Mr. Hutchison, and daily used in the treatment of erysipelas, I would propose to scarify the inner surface of the skin—an operation which would be attended with less pain than the incisions at present in use—which would affect vessels the opening of which would have a much more controlling influence over the inflammatory action than the ordinary applications to the free surface of the integuments. In the early stages of phlegmonous and erysipelatous inflammations, and in various kinds of cutaneous eruptions, I would introduce a small lancet or cataract needle through the skin, near the point where the inflammation first appeared, and scarify freely the inner surface of the chorion. This would unload the vessels into the subcutaneous cellular texture. The effused blood thus produced would moderate, I presume, in various ways, the cutaneous inflammation: by the quantity of fluid effused; by the mechanical compression which the effused fluid would have on the vessels entering the inner surface of the skin, and probably also by the stimulus to the process of absorption which the effused fluid might superinduce. 3d, Hydrops articuli, and purulent effusions into the joints, might be relieved or cured by the subcutaneous operations. When the dropsy of a joint is not speedily relieved by general or such local treatment as blisters, compression, or friction, I would recommend the employment of the subcutaneous puncture, and allow the fluid to drain into the subcutaneous cellular membrane, as in the plan now so successfully followed in the treatment of hydrocele.

There is an inherent dread in the profession of exposing the cavity of any large articulation; and this appre-

hension of danger has, I have no doubt, frequently prevented the necessary evacuation of fluids that were straining the textures, and otherwise producing, mechanically, local and constitutional disturbance. But the danger arising from the penetration of a joint is always in the ratio of the degree of communication of the atmospheric air with the cavity of the joint. If the joints are opened with a due regard to the exclusion of the atmospheric air, the risk of danger is not great; probably not greater than that attending the subcutaneous incisions of muscles. I draw this conclusion from actual observation. I have seen M. Jules Guérin frequently cut across the lateral ligaments of the ankle and knee, and penetrate these articulations, in his subcutaneous treatment of deformities of these joints, and in no instance did the slightest bad consequence follow. Guérin, in his lectures, states that he is in the daily practice of performing such operations, and that his practice is not only free from danger, but eminently successful. In penetrating the cavity of a joint for any necessary purpose, the openings through the skin and capsule should be small, and never directly opposite each other. In opening the knee-joint, for example, for the purpose we will suppose of removing a dropsical effusion, I would introduce a slender sharp-pointed bistoury or cataract needle through the skin at the lateral and lower part of the ligament of the patella; I would then carry the instrument directly upwards under the skin, for an inch, or an inch and a half, penetrate the capsule, and with greater or less freedom, according to the degree of viscosity of the contained fluid: I would then withdraw the instrument, close up carefully the puncture in the skin, and by pressure assist in forcing the fluid through the opening thus made in the capsule into the subcutaneous and neighbouring cellular texture.

In tapping for dropsy of every kind regard might be paid with advantage to the subcutaneous mode of operating, even where the fluid, from its situation, quantity, or the nature of the cause producing it, requires to be drawn off entirely and directly from the system. The ordinary or direct mode of tapping for ascites is sometimes followed with inflammation of the peritoneum; but if

a small trocar and canula were used, and the instrument carried for some distance under the skin before the abdominal cavity was penetrated, such an accident could hardly occur.

4thly, The treatment of varicose veins by the subcutaneous incision would, I think, be an improvement in surgery. The ligature on such veins is apt to excite phlebitis, with its train of dangerous and often fatal symptoms; and although the cutting across of these veins, as performed in the usual mode of operating, is a practice not free from danger, yet I conceive the danger would be much lessened, if not entirely removed, by introducing a slender instrument, such as a cataract needle, an inch at least from the vein to be cut, and dividing the vein with the least disturbance possible to the adjacent parts. This view is strongly corroborated by the fact that the application of the ligature, even upon the subcutaneous principle, to certain varicose veins, is attended with little or no danger. I have seen Ricord, of Paris, apply the ligature to the spermatic veins for varicocele; and in September last he had performed the operation twelve times without one unsuccessful result.

5thly, Neuralgia may also be treated with effect upon the subcutaneous principle. In the surgical treatment of this disease the object is to divide the affected nerve with as little external disfiguration as possible. This remark is particularly applicable to neuralgia of the fifth pair. Some years ago I was called to a case of tic-douloureux of the infra-orbital nerve. An attempt, I learned, had previously been made to divide the nerve, and the cicatrix which presented itself resembled more the mark of a sabre-wound across the face than the effect of a delicate surgical operation. Since the occurrence of that case I have inculcated the propriety of dividing the branches of that nerve without disfiguring the countenance, and have shewn how this could be accomplished by introducing small instruments of a particular form, either through the skin, or, so far as the infra-orbital and mental branches are concerned, through the mucous membrane of the mouth. This last mode I consider in some respects superior to the other, as the skin of the face is left entirely untouched, the atmospheric air

more effectually excluded, and the nerves divided probably with less pain, at all events with more facility and certainty.

Such is a short list of cases to which the subcutaneous operations have been, or I presume may be, extended. In the greater number of these cases I have either performed the operation myself or seen them performed by others. Or, where the suggestion is entirely novel, or the practice not yet introduced, I have either proved its practicability on the dead subject, or satisfied myself of its applicability to the cases in question, from reasoning, and the light of analogy.

33, North Hanover Street, Glasgow,
6th March, 1841.

ON HIP DISEASE AND LUMBAR ABSCESS.

By WM. OLIVER CHALK, Esq.

Resident Surgeon to the Royal Sea-Bathing
Infirmary, Margate.

[Continued from Vol. XXVII. p. 949.]

As real shortening of the limb is uncommon without dislocation of the femur, I shall take this opportunity of relating another instance; the only one, with the exception of those already mentioned, that has come under my observation. The case is highly interesting, from my having been enabled to watch its progress during a period of five years, and from the complications that have arisen during that time. T. E. King, æt. 5, admitted June 20th, 1836, suffering under disease of the right hip. The local symptoms were as follows: fulness of the nates, with perceptible fluctuation behind the great trochanter: the limb was drawn up and shortened to about an inch and a half; the toe was everted; pain in the groin sometimes acute, particularly when recumbent. Percussion over the trochanter caused great suffering; any attempt at walking produced much inconvenience. His general health was greatly impaired; he was labouring under febrile symptoms. The pulse was quick; skin hot and dry; the tongue covered with a thick white fur; great irregularity of the alvine secretions; severe pain on pressure in epi-

gastrio: there was a remarkable malformation of the chest, the xyphoid cartilage being, as it were, pressed inwards, forming a deep hollow. His mother stated that about Christmas, 1835, he began to complain of fatigue after walking, and that whenever she moved him he cried from pain: he walked with the toe everted, and dragged the limb after him. It was in the month of February that she first observed the swelling of the nates. About five months previous to the appearance of the local disorder he had an attack of scarlatina, which was short in its duration; but it had scarcely subsided when whooping-cough appeared, and lasted for twelve weeks. About this time he suffered repeated attacks of thoracic inflammation, which left him in a very debilitated state. In the May following he was attacked with measles, from which he recovered shortly. He came to Margate in that month, and in June, as already stated, became an out-door patient. His mother attributes the local disease to an accident which happened whilst lifting him out of a warm bath, in doing which the limb was suddenly twisted; he suffered great pain at the time, but it soon passed off; shortly afterwards, however, the symptoms just now described ensued. My attention was first directed to the state of his general health.

Leeches were applied to the epigastrium, followed by a blister; aperients of calomel and jalap were ordered twice a week, and the bowels were frequently evacuated with enemata of warm water. He took a mixture of Liq. Ammon. Acet. and Tartarized Antimony; these, together with a strict antiphlogistic regimen, were persevered in until the febrile symptoms were subdued. The warm salt-water bath was used at intervals, as the circumstances of the case would admit, and formed the only local treatment at this time. His general health, together with the local symptoms, soon improved. The treatment was persevered in, with occasional intermissions, up to August 20th. He now took a mixture of Iodide of Potass and Cinnamon-water (2 grs. to the 3j.) three times a day, which he continued up to September 13, when the Decoct. Cinchonæ was substituted for Aq. Cinnam. The bowels were at this time inactive, and the excretions still un-

healthy, to obviate which he was ordered occasional doses of Pulv. Scammon. Co. and Pulv. Jalapæ Co., and to persist in the use of enemata. On the 22nd of October he began to complain of pain in the left knee, which soon became somewhat swollen, accompanied by slight heat. Flexion and extension produced inconvenience; and he generally kept the knee extended. He was discharged at the closing of the House, October 28th, having on the whole derived great benefit.

As I was interested in the case I continued my attendance. In the course of two or three weeks the affection of the knee-joint increased rapidly, and soon presented all the symptoms of acute synovitis. The pain became so great that he could not bear the slightest movement, or even the pressure of the bed-clothes: the heat and swelling increased; the limb was now rigidly extended; his nights were sleepless. These symptoms were accompanied by intense febrile accessions. Leeches were again applied to the epigastrium, and a strict antiphlogistic treatment once more adopted. Cold lotions were applied to the knee, and every means used to maintain the limb in a state of perfect quietude. It was some weeks before the synovitis yielded to this treatment, and when it did he began to complain of pain about the hip on the same side: a few leeches were applied in the vicinity of the joint, followed by the application of ioduretted lotions. It is unnecessary to follow out the treatment any further at this period; and it need only be said that by constant and unremitting attention, and by the aid of tonics of Bark, Inf. Gent. Co. &c., together with a generous diet of meat, wine, porter, &c., he recovered slowly from these symptoms, and was readmitted as an outdoor patient, May 19, 1837, greatly improved in every respect, and was discharged the 18th of October following. His health was excellent at this time; he did not suffer any pain in either hip or knee-joints, and could walk tolerably well on crutches. The right limb was everted and shortened to about an inch and a half; the left limb was still extended: the thigh could be bent on the pelvis, and extension made without causing any pain. Soon after his return home I observed

that the nates on both sides were becoming fuller; fluctuation was more apparent on the right side (the first affected), whilst it was sufficiently perceptible on the left. He was still allowed to move about on his crutches, as he suffered no pain. As the suppurative process proceeded I applied an ointment of the iodide of mercury (5 grs. to the oz. of lard) spread on lint, over each swelling, every night at bed-time, in order to decrease the size of the abscess by means of partial absorption: this was effected to a certain extent. When the ointment caused too much irritation on the skin, or any tendency to iodism was evinced, it was omitted for a time. Within about a month from the period of the opening of the abscesses, which occurred in the first week of the April following, 1838, the only local application used was a rag moistened with water, and covered in with oiled silk: the matter on the left side was first discharged from a small orifice, which formed of itself on the upper and outer side of the thigh, and about half a pint of serous fluid, with a slight admixture of tuberculous matter, escaped: about three days afterwards the abscess opened on the right side; the discharge being precisely similar to the other, but less in quantity. He was again received at the Infirmary, July the 25th. The wounds were still open: by the end of June following, that on the right side had healed, but the left orifice was discharging rather profusely at the time of his return home Oct. the 17th. It closed shortly afterwards. On the ensuing 5th of Nov. I was suddenly called to see him: his mother had taken him a day or two before to Hackney in a cab: he complained of considerable pain from the jolting of the vehicle, at the upper and inner part of the thigh, which continued to increase up to the time of my visit. On examination I found a tumor about the size of a large orange, with great heat and redness, and a sensation of acute throbbing: it was evident that suppuration was taking place with great rapidity. These symptoms were accompanied by irritative fever, great thirst, and heat of skin: pulse above a hundred beats in a minute: the suppurative process was soon completed, and about four ounces of healthy pus discharged: the abscess healed in about a fortnight, and un-

der suitable treatment he soon recovered his health and strength. He was again admitted, as an out-patient, June 20, 1839. He could now walk with a stick, but in a very awkward manner,—both limbs being everted. He underwent a course of bathing, chiefly the douche, warm and cold, applied over the hip-joints. He took Decoct. Cinchonæ at times, and was still allowed a generous diet: the bowels were carefully regulated as before. He left the infirmary October 21st, greatly improved in every respect. From this time I saw little of him until Nov. of the year 1840, when I was requested by his parents to examine him. I now found a fluctuating swelling immediately behind the great trochanter, and it was evident that suppuration was going on. Upon making inquiries into the cause of this, I learnt that he had received a blow from one of his school-fellows just above the popliteal space, which he said caused some slight pain about the hip at the time: this happened about five or six weeks previous to his leaving Margate in Sept. last. He suffered scarcely any pain or inconvenience during the formation of the abscess. A small quantity of whey-like fluid was discharged recently from an opening in the situation of the former cicatrix, which has since ulcerated to about the size of a shilling, and at the time of writing this is nearly healed. He now walks on crutches without any inconvenience; they are used for the sake of guarding against accident, rather than from any inability to do without them. I have recently examined both articulations; he has recovered the use of them. The trochanter of the right side is considerably higher than natural, being scarcely more than an inch from the crista ilii, yet the shortening of the limb is now scarcely perceptible: there is no eversion of either foot. He suffers no inconvenience about the left knee, and the action of the joint is perfect. It cannot, I think, be doubted that the neck of the femur is altered in its position, as in the case of H. Eagle; the form of the nates and the situation of the trochanter being precisely similar. Suppuration cannot have taken place within the capsule of either joint, otherwise their action could not have been restored. It will be remembered that the abscess formed

on the left side soon after the synovitis of the knee: in all probability the suppuration was sympathetic: it was also more superficial; and although it formed full six months subsequent to that on the right side, was the first to break. The constant tendency to the formation of matter on the occurrence of the slightest injury in this case is remarkable; in proportion, however, as his general health improved, each recurrence of the local disease was less severe. The constitutional symptoms having now subsided, there seems to be little doubt of his ultimate recovery.

It would appear from the two following cases that dislocation of the head of the femur from the cotyloid cavity may take place independent of any disease of the articulation itself, from position adopted by the patient for the relief of other parts:—

John G. Butter, æt. 13, admitted May 9. 1837, suffering from necrosis of the left tibia, ostitis of the left sternoclavicular portion of the left clavicle, and from a small strumous ulcer on the chest: previous to his arrival he had experienced an attack of rheumatic fever, and the leg had been severely affected. There was partial dislocation of the head of the femur, which could be distinctly felt resting on the rim of the acetabulum: the limb was lengthened to about an inch and a half. He had never suffered the least pain in the articulation. During his illness he had been in the habit of crossing the leg affected as far over to the sound side as possible. He left the infirmary Oct. 21st, quite well, the head of the femur having regained its position, slight weakness of the extremity alone remaining.

Wm. H. Elsworthy, æt. 14, admitted Sept. 8, 1837, suffering under necrosis of the left tibia, consequent on rheumatism, with dislocation on the dorsum ilii; the limb was turned inwards, and was considerably shortened: the head of the thigh-bone could be distinctly felt, and it was exceedingly moveable. He had never suffered any pain in the articulation, nor could he or his friends say how or when the dislocation took place. He left the infirmary October the 27th, the local disease being somewhat ameliorated, and his general health improved. It is curious that in both cases the dislocation should take

place as a consequence of necrosis of the tibia. There cannot, I think, be a doubt that the capsular ligament must have undergone elongation, (particularly in the latter case), as mentioned by Mr. Stanley, in a paper already alluded to; and this circumstance, together with the position and atrophous state of the muscles consequent on emaciation and inactivity, must have combined to produce luxation.

The diseases with which *morbus coxæ* may be confounded, such as hæmorrhoids, sciatica, &c. &c. have been ably described by Sir Charles Bell, but by far the most serious and most important to distinguish is psoas abscess. I will, however, take this opportunity of relating a case of simulated coxalgia from the presence of a foreign body, as it is the only one I have ever met with, and may serve to place us on our guard when examining these cases. Ann Harrison, æt. 22, admitted July 23d, 1839. The patient stated that she had been suffering under disease of the right hip-joint for five months past, which originated in a fall from her chair on the 11th of March last. The limb appeared shortened: when she stood on her crutches the toe scarcely touched the ground. The shortening arose from contraction of the knee-joint: flexion, extension, rotation, &c. gave no pain, nor was there any on percussion over the articulation. Whilst under examination she called my attention to a particular spot behind and rather below the great trochanter, which she said was always excessively painful when touched. At this point I found a small rounded body about the size of a pea, which seemed immediately beneath the integuments. Her general health was much deranged; pulse quick and feeble; tongue furred; bowels costive; great tenderness in epigastrio, as well as in the flexures of the colon. She had lately suffered from hectic fever. The catamenia had been suppressed for several months. Her health had been delicate prior to the accident. My attention was first turned to the relief of the general symptoms. The hepatic treatment was adopted, and her health soon underwent an obvious improvement.

I could not satisfy myself as to the precise nature of the local symptoms, and I therefore determined on a closer

investigation of them*. By careful questioning, I found that she was doing some needlework at the time she overbalanced herself. In falling she remembered to have felt a snapping sensation in the hip, followed by acute pain, which she always referred to the spot already mentioned. She did not apply for advice until the end of the week following the accident, when a medical gentleman was called in. Leeches were applied to the hip, followed by a blister, and these means were persevered in, at intervals, up to April 16th, when, having derived no relief, she entered a hospital. A large blister was now placed over the articulation, and kept open. Leeches, to the number of seventy, were applied at different times; she was strictly confined to the recumbent position, and a suitable medical treatment adopted. At the end of eight weeks, the symptoms being somewhat abated, she was allowed to quit her bed and walk on crutches. Soon afterwards, however, she was seized with violent pain in the knee of the same side, which was more acute at night. She continued much in the same state up to the time of her admission into the infirmary. It now occurred to me that the local symptoms might have originated in the presence of a foreign body imbedded in the soft parts, and that it might be a needle, the circumstance of her being employed at the time in needle-work rendering it probable; and on examining the small rounded body (which had undergone some diminution since the improvement in her general health), I thought I could detect a hard pointed substance by the peculiar resistance afforded to the finger. On September 11th, just six weeks from the time of her admission, I made a crucial incision through the integuments about a quarter of an inch in depth, and after a little time succeeded in withdrawing with my forceps a portion of a needle, about an inch and a quarter in length, which had penetrated with the eye foremost. It is scarcely necessary to add that after this the local symptoms subsided. The patient was discharged quite well October 11th. The pain in the knee, in this instance, arose no doubt from the accumulations in the colon, which

* She had been in the house about a fortnight at this time.

were unusually great: in proportion as this organ was relieved, it subsided, and was nearly gone before the needle was removed.

The causes of morbus coxæ may be thus enumerated:—First, local injuries, such as falls, blows, sprains, &c. during the existence of the tubercular diathesis; second, injuries, more or less remote, acting as an exciting cause whenever the system is predisposed to take on the scrofulous or tuberculous character; third, rheumatic action; fourth, external violence, without any predisposing constitutional causes, the latter are the least frequent. Whenever we are called on to treat diseases of the hip-joint, it will be necessary to ascertain the circumstances under which they may have arisen, as the success of our treatment must be mainly dependent thereon. As scrofulous affections of the articulation are by far the most numerous and important, I shall confine myself principally to a consideration of them. Our first endeavour should be, if possible, to ascertain how long the strumous diathesis may have existed: to do this it will be necessary to obtain a minute history of the diseases under which the patient may have suffered from birth up to the time of the appearance of the local disorder. A most careful examination should be next made of the state of the nutritive functions; the thoracic and abdominal viscera should be attentively examined, and the extent to which they may have suffered duly estimated. In this way we shall be better able to judge of the probable success of our endeavours to arrest the progress of the local malady; for according to the length of time the constitutional symptoms may have existed, so will be the difficulty in subduing it. If the lungs should happen to be the seat of tubercular deposit to any great extent, or the liver be disorganized, all our attempts will prove unavailing. The first indication is to remedy the constitutional mischief, and if our efforts are successful in this respect, the relief of the local malady is almost certain. If hepatic congestion (as is commonly the case) be present, the treatment before described should be adopted without delay. In the earlier stage of the disease the patient should be confined to the recumbent posture as long as any

pain exists; but as soon as it subsides, he should be allowed to get about on crutches, and take exercise in the open air. Long confinement to bed is highly prejudicial to persons suffering from strumous disorders of any kind. The system of rest, in my opinion, has been carried to far too great an extent in chronic affections of the joints generally. Ioduretted and poppy fomentations may be applied over the articulation, or mild ointments of iodine and iodide of potass spread on lint. If there be much heat of the parts, the ioduretted lotions may prove serviceable. The warm salt water bath will also be useful at this time. As the use of counter-irritants, such as moxæ, issues, &c. have been generally recommended at this stage of the disorder, and have so long found favour among surgeons in the treatment of it, I shall take this opportunity of saying something concerning their use. The constant employment of them, and their equally constant failure in arresting the progress of the disease, is strikingly exemplified in the cases received year after year at this infirmary; and I consider them not only unproductive of benefit, but exceedingly hurtful to the patient, more especially in their latter stages. I have frequently succeeded, by the healing of a moxa, issue, &c. in allaying hectic symptoms that were otherwise unmanageable. When the integuments become distended by the altered condition of the parts, and by suppuration about the joint, the cicatrices frequently slough, or form outlets to the abscess; or, if this does not happen, they are often highly sensitive, and in this way the sufferings of the individual are frequently enhanced, especially if the case terminates fatally.

In one case I remember to have seen the carious head of the femur protruding through a large opening, that had originally been an issue, placed behind the trochanter. Sometimes all attempts at healing moxæ, &c. are unavailing, and they assume the form of strumous sores, adding to the superficial ulceration that is often present, especially when local depletion (by repeated cuppings and leeches), and counter-irritation, have been carried to a great extent: the parts being thus weakened, are ever more prone to the

ulcerative process. Whenever these applications are used in the vicinity of the joint, during the existence of the tubercular diathesis, it is my firm conviction (based on continued and often-repeated observation) that a serious injury is inflicted, not only in these, but in all other strumous affections of the articulations generally, and that they are only so many additional causes of irritation and suffering to the patient.

If the disease were independent of constitutional causes, we might perhaps hope for relief; but all the while the deposition of tubercular matter is going on, or cartilage, bone, or ligament, are undergoing disorganization, from an altered condition of the nutritive organs, little hope of benefit can be entertained from local measures, unless the constitutional mischief be first remedied. Every endeavour should be used, in the earlier stages of morbus coxæ, to strengthen the patient by all means in our power. A nutritive and stimulant diet should be adopted in combination with the hepatic treatment, and the bowels should be carefully regulated by means of aperients and enemata of warm water, even after the hepatic symptoms have been subdued, as there is a constant tendency to a recurrence of them. Local depletion should be particularly avoided, for the loss of blood even by a few leeches produces a visible effect on the constitution. Scrofulous patients cannot bear the abstraction of blood, for the supply of it to the system is necessarily deficient, from the defective conditions of the assimilative organs. I have just now observed that the patient should be allowed to walk on crutches as soon as pain subsides, and, if not of the adult age, it is of great importance to maintain them of a proper length, for as they wear down, and the growth of the individual advances, they will of course be too short: if no attention is paid to this circumstance, and the patient be allowed to walk on them, the body will be bent forward, and the limb affected acquire a bad position. In my earlier acquaintance with these disorders I was often struck with the peculiar deformity so produced, without being able to account for the cause. The chest is thrown forward, the lumbar vertebræ curved inwards, and the nates protruded. If we are unsuccessful in quelling the ear-

lier symptoms, and the disease proceeds (as will often be the case, despite of our efforts to the contrary) to the suppurative stage, we must endeavour to moderate it by the application of the ointments of iodine and hydriod. of potass, or the iodide of mercury spread on lint, and applied immediately over the parts. We should be careful to avoid iodism, for if it be carried to any great extent it is generally injurious. I have invariably found that opening of the abscess (and this applies to strumous abscesses generally) is prejudicial; it should be allowed to break of itself, the situation forming no exception to the rule. I have frequently tried the various means recommended by authors, more especially the valvular opening, as practised by Mr. Abernethy, but without success*. The patient, if possible, should not be confined to bed, even during the suppurative stage, but should be allowed to take exercise in the open air, either in a carriage, or by walking on crutches, if not productive of much pain. The milder iodine ointments form an excellent application to the superficial ulcers and orifice of the sinuses in the latter stage. A lotion composed of nitric acid and vin. opii, with camphor mixture, forms an excellent application, especially when the sores take on a sloughing action. The following is the formula of the lotion:—

R Acid. Nitrici, ℥xx.; Tinct. Opii, 3ij.;
Mist. Camphoræ, ʒxvj. M. ft. lotio.

Opium itself may be sometimes used in place of vin. opii. The lotion is generally applied by means of charpie, and the edges of the ulcer are carefully supported by thin strips of adhesive plaster. The dressings may be removed once or twice in the day, according to the exigency of the case. A carrot poultice applied over this dressing is often of service, and forms a most comfortable and cleansing application. The carrots should be boiled and pounded to a soft pulp. The extract of conium mixed with a bread poultice produces an excellent effect in some forms of irritable ulcer. The water dressing, as recommended by Mr. Liston, is frequently serviceable. Great attention should be paid (a circum-

* When speaking of lumbar abscess, I shall have occasion to revert more particularly to this subject.

stance too frequently overlooked) to cleanliness of the surrounding integuments, as the comfort of the patient is very essentially promoted by it. The practice (formerly much used) of applying linseed poultices to scrofulous sores seems now to be falling into general disuse, and very justly so, for a more improper and uncleanly application can scarcely be devised. Sea-water applied in any way during ulceration is generally injurious, and it is almost impossible to heal wounds whilst the patient is undergoing a course of sea-bathing. Bathing is always hurtful during hectic symptoms. Injections to the sinuses have been used by some surgeons, and I was formerly in the habit of using them myself, but I cannot say that they ever appeared to me beneficial; on the contrary, in two or three instances I remember to have seen sloughing induced by the practice. Nothing is so important to the patients in the latter stages as the emptying of the lower intestines by enemata; for even when we are unable to stay the progress of the local disorder, much suffering may be thus avoided. Aperients are often too weakening; besides which they will not have the desired effect.

Narcotics of every description have been used to allay pain in the treatment of these cases, but for my own part I cannot say that I have found them productive of much benefit. If they are exhibited in large and continued doses, their influence on the general symptoms is commonly hurtful. Formerly the Ext. Conii was extensively used at the infirmary by Dr. Brown, and it may certainly be said to be more useful than most other preparations.

The next and most formidable disease to which the hip-joint is liable is rheumatism; it sometimes attacks scrofulous patients, and when this happens it forms a dreadful complication. It is essentially necessary to ascertain if any rheumatic action exists, by a careful inquiry into the case, for the disease runs its course, generally speaking, with more rapidity than strumous affections of the articulation.

Of all internal remedies I have found Iod. of Potass, combined (if there be no fever) with Decoct. of Cinchonæ, the most serviceable: the various preparations of Colchicum are also useful. As

a local application, the ioduretted fomentations are by far the best, and often productive of the greatest relief. It is almost unnecessary to say that the warm salt-water bathing is highly beneficial. I shall hereafter have occasion to speak more particularly of the iodine treatment, and of the effect of sea air, bathing, &c. in these and lumbar affections; and I shall now proceed to a consideration of the symptoms which characterize the latter.

[To be continued.]

ON THE INSERTIONS OF THE STRAIGHT MUSCLES.

To the Editor of the Medical Gazette.

SIR,

IN perusing Mr. Lucas's excellent work, entitled, "A Practical Treatise on the Cure of Strabismus," I was struck with the account he gives of the insertions of the four straight muscles, in the following paragraphs:—

"In the well-proportioned eye of an adult, the centre of the tendon of the superior rectus, and that of the inferior rectus, are at an equal distance from the cornea, viz. four lines; the inner edges of each are about the same distance, whilst the outer edges are considerably more removed, being distant seven lines."

"The centre of the internal rectus muscle is distant from the cornea about three lines, its superior edge is distant four lines, and its inferior edge five lines."

"Contrasted with the tendon of this muscle, that of the external rectus is distant at its centre from the cornea nearly five lines, and its superior and inferior edges are distant nearly six lines."

Mr. Lucas does not say what he means by a line. If he means one-tenth of an inch (see Johnson's Dictionary), his measurements are very inaccurate; less so, if he means one-twelfth of an inch.

I do not pretend to doubt that Mr. Lucas has stated the measurements, in the foregoing paragraphs, after actual examination; but, either from using an inaccurate means of comparison*,

* To those who are in the habit of measuring minute parts of the body, I would recommend a

from having before him at the time a subject in whom the attachments were extraordinarily wide from the cornea, or from some other circumstance which I cannot explain, he has set down distances which certainly do not correspond with the average insertions of the straight muscles. That there are varieties in the distances from the cornea at which these muscles are inserted, is extremely probable; but as I conceive that, by following Mr. Lucas's statements as general rules in performing the operation of myotomy for the

cure of strabismus, the operator might be led to attempt the division too far from the cornea, and thereby render difficult what is, in general, the simplest of all operations, I have again carefully examined the insertions of the straight muscles, and beg leave to state the following measurements from the eyes of an adult male subject now before me.

The differences between Mr. Lucas's measurements and mine will be best exhibited by placing his in one column, and mine in a second.

Rectus internus.

Insertion of centre of tendon	3 lines . .	4-20ths of an inch.
—————superior edge of tendon . .	4 lines . .	6-20ths „
—————inferior edge of tendon . .	5 lines . .	6-20ths „

Rectus externus.

Insertion of centre of tendon	5 lines . .	5-20ths „
—————superior edge of tendon . .	6 lines . .	7-20ths „
—————inferior edge of tendon . .	6 lines . .	7-20ths „

Rectus superior.

Insertion of centre of tendon	4 lines . .	7-20ths „
—————inner edge of tendon	4 lines . .	6-20ths „
—————outer edge of tendon	7 lines . .	8-20ths „

Rectus inferior.

Insertion of centre of tendon	4 lines . .	5-20ths „
—————inner edge of tendon	4 lines . .	5-20ths „
—————outer edge of tendon	7 lines . .	6-20ths „

While making the measurements stated in the second of these columns, I noted the following particulars:—

1. The internal and external straight muscles are inserted symmetrically, in the same horizontal line.

2. The superior and inferior straight muscles are not inserted symmetrically, nor in the same vertical line; for the inferior is inserted exactly in the middle line of the eyeball, but the superior is considerably nearer the nose than the middle line of the eyeball.

3. The internal is the thickest and shortest of the straight muscles.

4. The external is the longest of the straight muscles, and is next to the internal in point of thickness.

5. The internal and external straight muscles are inserted concentrically to the edge of the cornea.

6. The inferior straight muscle is not inserted concentrically to the edge of the cornea, and the insertion of the superior is placed still more obliquely to the edge of the cornea. The obli-

quity of the two is not equal, as Mr. Lucas's measurements would indicate.

It is a question of considerable importance, to what degree the straight muscles contract after being divided. Bernouilli* shewed that their greatest contraction, in the motions of the eyes, equalled very nearly one-fifth of their natural length. If this degree of contraction takes place when one of these muscles is divided, the point of re-adhesion to the sclerotica must always be behind the transverse axis of the eyeball, unless previous inflammation of the cellular substance around the muscle prevents it from being retracted to the usual extent.

Mr. Lucas's description of the fasciæ in connection with the muscles of the eyeball is very instructive. I have often thought, however, that a separate bursa mucosa existed beneath the tendons of each of the recti.—I am, sir,

Your obedient servant,

W. MACKENZIE.

Glasgow, March 6, 1841.

comparative scale of the English and French inch, sold by Ross and Co., 33, Regent Street, as a very neat and convenient instrument.

* *Commentarii Academiæ Petropolitanae*, tom. i. p. 304. Petropoli, 1728.

VACCINATION.

To the Editor of the Medical Gazette.

SIR,

IN answer to Mr Crawford's inquiry, whether a person can conscientiously be registered as having been regularly and successfully vaccinated, in whom, out of four incisions made, one only takes effect, and that one is retarded in its normal course by forty-eight hours, Dr. Gregory affirms that he may be registered, and gives as his reasons—1st, that any attempt to reproduce the disease after the lapse of one, two, or three years, will fail, proving that the system has *received* from one vesicle a certain influence which it did not previously possess; and this insensibility to renewed insertions of the vaccine virus I hold to be the only legitimate criterion of perfect vaccination; 2dly, that the retardation of the vesicle does not preclude its legitimate influence on the system: in both of which conclusions I perfectly agree with Dr. Gregory: but with all due submission to so high an authority, I must contend that the reason by which he arrives at his conclusion is false, inasmuch as he asserts that the system *receives* from this one vesicle that influence which protects it from an invasion of variola; whereas, in my humble opinion, the system becomes affected by the absorption of the virus, and the vesicle is the result of its effect on the constitution, enabling it to secrete a specific virus, capable of producing the same disease.

To say, then, that the constitution receives its influence from the vesicle is incorrect reasoning, and liable to produce false views of the disease, in those members of the profession who are apt to take the opinions of talented men for granted, without being at the trouble to think for themselves. To prevent so false an impression is the only object of this communication; as the higher the authority is from which such reasoning comes, the greater necessity there is for its being refuted. This may appear to some members of the profession, perhaps, of little importance; but, on reflection, I think it must be granted that, where any principle of pathology is involved, we cannot be too strict in our reasoning to

enable us to come to a correct and legitimate conclusion.—I am, sir,

Your obedient servant,
THOMAS TAYLOR.

Cricklade, March 22, 1841.

MEDICAL GAZETTE.

Friday, March 26, 1841.

“Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri; potestas modo veniendi in
publicum sit, dicendi periculum non recuso.”

CICERO.

CAMBRIDGE STUDIES.

THE Dean of Ely, as our readers will have learned from our last article, wishes to abolish, as nearly as may be, the system of private tuition in the University of Cambridge. He is so indignant at every thing which resembles cramming, or which tends to destroy the freedom of study in that great seminary, that he would not only prohibit private tutorage by express enactment, but would willingly lessen the temptation to it by diminishing the amount of knowledge required both for honours and the common degree. This is going too far, surely; do not throw away the baby with the bath, say the Germans*; because some are crammed, do not forget that others are taught; and above all, do not diminish the minimum for a B.A. degree, when its increase has already produced the best possible effects. “There is no doubt,” says Dr. Peacock, “but that the increased severity of the examinations for degrees has already augmented the amount of study amongst those students of the University whose industry is rather stimulated by their fears than by their ambition and love of knowledge, and that the best effects have been consequent upon this change, both as regards the orderly habits and

* Verschütt' Er nur nicht gar
Das Kindlein sammt dem Bade!
Bürger.

general occupations of a great majority of under-graduates."

This is sufficiently cheering; but as the web of life is ever of a mingled yarn, we can easily conceive that these great advantages have been purchased by some sacrifices, and we sympathize with the Dean, when he laments the consequent decline of attendance on University lectures, and the decreased cultivation of individual tastes in the pursuit of knowledge. Yet, on the whole, the balance is obviously in favour of the new system; and as time moves on, the less brilliant under-graduates will think of the coming examination with less terror, and will find that they have abundant time for attending lectures on history or chemistry.

Dr. Peacock's aversion to the stimulus of private tuition has partly influenced him in proposing another alteration, where we are again compelled to differ from him. Composition in Greek and Latin verse at present always forms a part of the examination for the Classical Tripos; but Dr. Peacock wishes to abolish it. If we were inclined to settle the question by authority, it might be sufficient to observe that this species of composition is cultivated at classical schools in England with a diligence nearly proportioned to the excellence of the school—a rule to which we believe the exceptions are but few. Now, in mechanical language, "action and reaction are equal, and in opposite directions;" and as English schools of the higher class are naturally governed in many points by Cambridge methods, so the University herself does not disdain to be influenced by the customs of the best grammar schools. The ocean, say travellers, is sometimes tinged by the waters of the great rivers which flow into it.

These lesser preliminary Universities

have not given up Latin verse-making but of late years have added Greek. The reason is a simple one. Experience has shown that there is no method of familiarizing the student with the beauties of the ancient writers so certain as that of making him attempt to imitate them. In many instances the attempt is a feeble one, but even then, as in the old fable, though the hoped-for gold is not found, the vineyard is much enriched by the digging. Others are more successful; and though we are far from concurring in the extravagant praises which have sometimes been lavished on these imitations*, it must be acknowledged that they often display surprising ingenuity. It may be urged, indeed, with great truth, that the heights of poetry can be attained only by him who writes in his native tongue, where alone he can be a ποιητής, or creator. Besides the in-born inferiority of professed imitation, it is scarcely possible for the most laborious compiler of a classic cento to avoid errors. If Theophrastus was instantly found out to be a foreigner by the Athenian herb-woman, from the very excess of his atticisms, what faults, what solecisms, would be detected in the compositions of our modern Theophrasti, could some Aristarchus rise from the grave, to prefix the fatal obelus to their lines!

We allow the weight of these observations, and are far from confounding modern Bristol stones with the ancient diamonds; but, at the same time, we are convinced that no better way of cultivating literary taste has been devised than the method in question. Dr. Peacock would continue the translations into Greek and Latin prose, though hostile to those in verse. To

* Cowper thought Vincent Bourne's longs and shorts better than those of several of the best real Latin poets; and Lord Monboddo pronounces Milton's first elegy to be equal to any thing of the "elegiac kind to be found in Ovid, or even in Tibullus." (!)

us it seems that both rest on the same theory, and may be defended by nearly the same arguments, excepting that composition in verse is more spirit-stirring and more improving. Our readers will observe, that we do not enter upon the vexed question of classical education in general, nor attempt to ascertain for what classes of persons other styles of mental discipline might be more advantageous, but confine ourselves simply to the minor point:—granting that a number of years are to be expended on the finest models of literary excellence, how is the spirit of these models to be transfused into the thoughts and language of the student?

We may remark, that the compositions in prose which the Dean approves of, are also much neglected at inferior schools; so that the student who has passed his boyhood at one of these academies will find himself in want of a tutor at Cambridge, if he wishes to be distinguished in classical examinations. And if it should turn out that there are half a dozen schools in England which, though tolerable in other respects, neglect composition in verse, would it not be better that they should rise to the Cambridge level, rather than that the University should sink to theirs?

Dr. Peacock laments the condition of the student who is compelled to imitate so many kinds of ancient poetry: “he must write sapphics, after the manner of Sappho; iambics, and sometimes trochaics and anapaestics, in imitation of the Tragedians and Aristophanes; hexameters, after the model of Homer: whilst, in Latin, he must be able to imitate the lyrical metres of Horace (including his alcaics, which no other Latin poet attempted to compose); and the hexameters and elegiacs of Virgil, Ovid, and Tibullus.”

Greek sapphics are written in the

contest for Sir William Browne’s medal; but if they are set in the examinations for the Classical Tripos, we would willingly see them struck off the list; for it requires something almost equal to poetic inspiration to imitate a writer of whom so few fragments have been preserved. It is almost too much to say to any student, but of the highest stamp,

Tu longè sequere, et vestigia semper adora—

when time has left only a few interrupted tracks, and the rest must be supplied by the imagination!

But even if no other ancient poet followed Horace in composing alcaics, this metre has been used by so many modern writers, and is so deservedly a favourite, that it would be one of the last to be given up. Milton, Gray*, and Dr. Johnson, are among those who have made the attempt, and it is not uncommon at most of our higher schools.

The Dean of Ely informs us that the introduction of Greek composition into the University of Cambridge took place not more than twenty-five years ago, and that it has lowered the standard of excellence in writing Latin. This may be easily conceived; but is Greek composition therefore inadvisable? Surely not. If we are asked why so few persons can read Greek with any facility or correctness, *ad aperturam libri*, we should answer that, in addition to the extreme difficulty of the language, the chief reasons are, first, the neglect, until a very recent period, of Greek composition; secondly, the use of Greek and Latin, instead of Greek and English Lexicons, together with the scantiness of those of the latter kind; thirdly (which we suggest with diffidence), the custom of perplexing

* Gray’s ode, beginning “O tu severi Religio loci,” written in the album of the Grande Chartreuse, in Dauphiny, in August 1741, has considerable elegance.

the student with the most exquisite minutiae of grammar and criticism, before he can read an easy author with fluency.

On the subject of professional education, the Dean of Ely offers several suggestions worthy of consideration, and, perhaps, in some points, of adoption.

After remarking that at least one-half of students in the University are designed for the church, he adds that no provision is made for their professional education, except the lectures of the Norrisian professor. By provision, the Dean evidently means compulsory attendance: other theological lectures are given, but the student hears them or not, as he pleases. Nor must we overlook the provision made for theological education of this class of students in common with all other undergraduates, such as the University examinations in the Acts of the Apostles, Paley's Evidences, &c.; and the College examinations in the same or similar subjects. Still, something might be added with advantage. The Dean of Ely proposes that six regular courses of lectures should be given every year, by different professors; two of them by professors to be hereafter appointed, namely, of Biblical Criticism, and Ecclesiastical History. These lectures would generally be attended in the year following the B. A. degree, though they might also be attended by under-graduates. All candidates for holy orders, says Dr. Peacock, should be examined by the several professors, and their joint certificate should replace the present testimonial of the Norrisian professor.

As this alteration would either interrupt the usual course of study, or would entail the expense of an additional year's residence, it is easy to foresee that it will be strongly objected to. We would therefore propose that, in addition

to the present certificate of the Norrisian professor, candidates for orders, at any time after their B. A. degree, should pass an examination in the New Testament in Greek, in Ecclesiastical History, and the doctrines and liturgy of our church. As to medical students, the Dean of Ely proposes what most of them would accept as a boon, a shortening, namely, of the time for obtaining the degree of M.D. This demands caution; and though we do not object to lopping off a year or two, yet, in common with every friend to the University, we should shrink from the thought of its annually sending forth a shoal of young men nominally qualified to teach medicine. Dr. Peacock, however, proposes that the degree of B. A. should always be a preliminary, which would greatly tend to avert such a catastrophe, and maintain the social standing of Cambridge medical graduates*.

Such are a few of the points touched upon by the masterly pen of the Dean of Ely. We have no room to give an account of the Master of Glomery, the nature of whose office is a *crux* to Cambridge antiquaries; nor of "the enter-ryng of a master in gramer," a degree not conferred since the year 1543†; nor of many other ancient and curious custom.

Dr. Peacock's book will not be thrown away; for the University of Cambridge is ever ready to stand in the van of educational progress, and to yield to the agreeable necessity of continual improvement.

* Those who wish to know what were the medical studies of Cambridge three centuries ago, may see them detailed in Dr. Peacock's Appendix, p. lii. *note*. Galen was then read in Latin translations from the Arabic.

† "Then shall the Bedell purvey for every master in Gramer a shrewde Boy, whom the master in Gramer shall bete openlye in the Scolys, and the master in Gramer shall give the Boy a Grote for hys Labour, and another Grote to hym that provydeth the Rode and the Palmer, &c. de singulis. And thus endythe the Acte in that Facultye."

From an account drawn up, in the sixteenth century, by Matthew Stokys, M.A. (Dr. Peacock's Appendix, p. xxxvii.)

SURGICAL LETTERS FROM PARIS.

NO. I.

To the Editor of the Medical Gazette.

SIR,

THE following brief notices may not be unacceptable to those of your readers who take an interest in what is going on in the Parisian surgical world.

Your obedient servant,

EDWIN LEE.

Paris, March 1841.

There is but little alteration in the leading features of the surgical practice of Paris since my last visit to its hospitals, though a considerable improvement has taken place in the practice of some individuals. Operations are perhaps as much in vogue as ever, but little having as yet been done with regard to the medical treatment of surgical diseases, in which department English surgeons so greatly excel, and which frequently obviates the necessity of operating. An exception must however be made in favour of M. Lisfranc, who continues to prescribe a variety of internal medicines, and general constitutional treatment, in many local complaints, though his employment of these measures falls very much short of the English practice. M. Velpeau also employs medicines in some cases, though they are mostly exceptional. More attention is, however, now paid to the general treatment after operations and accidents than was formerly the case; patients being much less frequently left with constipated bowels for several successive days, and kept on a rigid diet for a long period after the symptoms of inflammation have subsided, though the ghost of Broussaism still renders practitioners very chary in the use of laxatives, or of solid articles of diet, in these cases. Wounds and stumps are still, as heretofore, loaded with charpie, which prevents their healing as soon as they otherwise might do, and causes abundant suppuration, which not unfrequently gives rise to serious constitutional symptoms. Several amputations of the leg have been, within the last twelve months, performed with success, six or seven inches above the ankle, to enable patients to wear an artificial leg, and obviate the unsightly appearance of a wooden one. Simple fractures are fre-

quently treated by the starched bandages, especially those of the upper extremity; but I have not seen patients walking with crutches about the wards of the Paris hospitals, a few days after fracture of the leg, which they are enabled to do in London by the application of the immoveable apparatus; whether starch bandages and pasteboard splints, Amesbury's splints, or the tin ones (as at the North London Hospital,) be employed.

Diseases of the eyes are now much better understood and treated in Paris than a few years ago; which I consider partly owing to M. Sichel, a German by birth, who came to reside in Paris, and established an ophthalmic institution, which students will do well to attend, as they may there see a great variety of these diseases, the peculiarities of which, and the rationale of their treatment (a combination of the German and English methods) are fully explained by M. Sichel, than whom I do not know any one in whose opinion I should be disposed to place more confidence in any doubtful case. While mentioning diseases of the eyes I may notice the operation of extracting capsular cataract by the sclerotic, which has latterly been performed several times by M. Sichel. The last of these operations at which I was present was performed about a fortnight ago. The pupil was obliterated by the opaque membrane, which protruded into the anterior chamber, and adhered strongly to the iris. An incision by puncture, about a line and a half in length, was made with Jæger's triangular-shaped knife, through the sclerotic, about three lines from the margin of the cornea, through which a small forceps was introduced into the posterior chamber, and several attempts were made to seize the membrane, which could not be effected, on account of the difficulty of passing one of the branches before it. A considerable portion of the vitreous humour escaped; but its further escape was arrested by closing the eye for a few seconds after each ineffectual attempt. M. Sichel at last succeeded in seizing the membrane, and drew it towards the aperture, though the adhesion was so strong that the iris was separated for some extent from the ciliary ligament, and a small quantity of blood effused. A minute portion of the membrane was,

however, snipped off, and a circular pupil was the result. The after-inflammation was but slight; the remains of the capsule were drawn to the circumference, and a large central pupil existed a few days after the operation, enabling the patient to see very well.

An ingenious speculum has just been invented by M. Charriere, which widely separates the eyelids without pressing upon the globe of the eye, and which would be invaluable in operations on this organ in country practice, as it enables the operator to dispense with an assistant. It consists in an elastic steel semicircular bow, which fixes to the back of the head much in the same way as a truss. Its anterior part is divided into two branches, which, by their elasticity, keep the eyelids separate. It was used a few days ago, for the first time, by M. Guerin, in his operations for strabismus. And this leads me to say a few words respecting this operation, which has been made a handle by some persons to puff themselves in the papers. Thus one individual was said to have been operated upon one day, and his appearance so much altered, that the following evening he mystified his friends at one of the masked balls, without wearing a mask. Another story was circulated of a husband who, wishing to occasion an agreeable surprise to his wife, was operated upon. The lady, however, would not recognize him, and he was obliged to sue for a restitution of conjugal rights. These pitiful attempts, though they may procure the inductors a temporary notoriety, cannot fail to lower them in the estimation of the profession, and ultimately of the public. But to return to the operation, the original suggestion of which is generally considered to be owing to Stromeyer, whom I had the pleasure of meeting at Erlangen last April, and with whom I had a long conversation on the subject of muscular contractions, several cases of which, cured, or in progress of cure, he shewed me. Since that period he has succeeded Graefe at Berlin. M. Guerin, however, states that, in his conferences upon deformities, he suggested this method of remedying squinting before its announcement by Stromeyer, but was deterred from putting it into practice by the dread of serious inflamma-

tion of the eye. Be this as it may, the sub-conjunctival method of Guerin, though somewhat more difficult of performance, appears to me to be attended with less inconvenience, and less risk of inflammation, than the other methods, in which the muscle is exposed for a considerable extent previous to its being divided, or a portion of it cut out, as is practised by Mr. Phillips, and other operators. In the former instance the only inconvenience, in the great majority of cases, is the sanguineous extravasation beneath the conjunctiva and the eyelids, which lasts for several days. The muscle is prevented from reuniting, and consequently the squint from returning, by the simple contrivance of spectacles, with the inner half of the glass, corresponding to the operated eye, rendered opaque by pasting a piece of paper on it; so that the patient, in order to see, is obliged to turn the eye rather outwards. After the latter method of operating a fleshy excrescence is produced after cicatrization, which requires to be excised: inflammation is sometimes severe, and in several instances the muscle attaches itself to the globe of the eye too far back: a degree of divergence from the central line, and consequently double vision, is produced; or, from the attachments of the internal rectus having been so extensively divided, its antagonist draws the eye outwards, and the same result ensues.

An operation was performed by M. Guerin about three weeks ago, for congenital luxation of the thigh, in a girl about twelve years of age, which consisted in dividing the rectus, and the portions of the adductors which were felt to be tense beneath the skin. Traction was subsequently made by attaching to the limb a weight, which passed by means of a pulley over the end of the bed, and a few days afterwards the reduction was effected without the necessity of employing a great deal of force. The circumstances which render reduction applicable in many cases of congenital luxation, even after several years have elapsed, were explained by M. Guerin to the pupils; the muscular contraction being the chief impediment, the ligaments not being ruptured but merely elongated, and the cavity not being obliterated as occurs in luxations which are the result of

violence, and in which an artificial capsule has formed.

An immense crowd was collected at the last sitting of the Académie de Médecine, in expectation of the uproar which took place during the preceding one, on the occasion of M. Amusat presenting some patients who had been cured of stammering by the division of the genio-glossi muscles, when M. Gerdy exclaimed, that people now-a-days performed operations right and left without regard to propriety, and that he saw in these operations little else than mercenary motives. It was, however, decided, in order to prevent a recurrence of the scene, that the patients should be examined by a committee.

In my next letter I will mention some of the more interesting cases which have fallen under my observation within the last few weeks.

CLINICAL LECTURE,

Delivered at University College Hospital.

February 9, 1841,

BY SAMUEL COOPER, ESQ.

Senior Surgeon to the Hospital, &c.

GENTLEMEN,—It is observed by Scarpa, that the femoral hernia seldom occurs in young girls, and still more rarely in men. Yet I have certainly seen many more instances of it in the latter than the former, and am disposed to suspect that the preceding statement, though coming from a high authority, is somewhat questionable. The following is not the only example of femoral hernia that has been noticed in the male patients in this hospital.

CASE I.—*Strangulated femoral hernia in a male subject; operation; gangrene of the bowel; recovery.*

Charles Lutwyth, æt. 51, admitted under Mr. Quain, Jan. 30, 1841, about 3 o'clock in the afternoon. Twenty-one years ago he perceived a tumor, "smaller than a nut," in his *left* groin; it gradually increased, but gave him no uneasiness, and had never been reducible. About twelve months since, another tumor having formed on the *right* side, the patient procured a double truss.

He describes this last swelling as having always been easily replaced, but liable to descend again whenever the truss was laid aside. At present there is no tumor on this side. Last night he was laboriously employed in carrying sacks of coal in a gas

manufactory, and this morning, at 9 o'clock, while he had his truss on, he became very unwell, feeling an inclination to vomit, and to go to stool, with pain in the abdomen and in the tumor of the *left* side, which was found to be larger and harder than usual. He went home, and took an ounce of Epsom salts, which was soon followed by a scanty solid motion.

On his admission, he complained of pain over the whole abdomen, which was much distended, tympanitic, and tender on pressure. He experienced continual nausea, was restless and agitated, and occasionally vomited up a greenish fluid. There was no tumor on the right side; but in the left groin was found an oval, tense, painful swelling, about as large as a goose's egg, slightly moveable on its base, which was narrow, and extended transversely. The upper margin of the tumor was concave, and thrown over Poupart's ligament, which could be partially traced beneath it; and, on the other side, the femoral artery was felt pulsating. There was no preternatural heat, nor discolouration. The taxis having been tried without success, the patient was placed in a warm bath; and as soon as collapse and faintness had been thus brought on, the taxis was unavailingly tried again.

At 6 o'clock, Mr. Quain, finding that there was no chance of returning the parts by the taxis, recommended the operation; but this proposal was at first declined. The pulse was now 54, and rather small; the belly more tense and tympanitic; the tongue dry, but clean; the vomiting continuing; but the griping pain less severe. On the principles advocated by Dr. O'Beirne, an œsophagus tube was introduced about two feet up the large intestines, but no flatus escaped; and, consequently, no opportunity was afforded of testing the efficacy of such discharge of air in promoting the success of the taxis.

Passing over some minor circumstances, I will next notice the operation. The hair having been removed, and the integuments held up in a fold, Mr. Quain made an incision across the narrow diameter of the tumor, about three inches long, and directed obliquely inwards, and then another about one inch in length, directed inwards, and joining the first about its centre at a right angle. The sac was then carefully dissected down to, and a piece of it having been lifted up with a pair of forceps, was opened, and the opening enlarged with the aid of a director, whereby a considerable mass of brownish omentum, full of highly congested vessels, was exposed; and on turning this up, some folds of intestine were perceived at the inner side, of a deep and uniform slate colour, and without any visible ramification of vessels. The sac contained no

fluid, and the omentum was adherent to the neck of the sac (a circumstance explaining the irreducible state of the hernia, even from an early period of it); but the intestine itself was quite free from adhesions. Mr. Quain having reached the stricture, which was a remarkably tight one, introduced Sir Astley Cooper's hernia knife, under the guidance of the left fore-finger, and divided the stricture to the extent of a few lines; but finding this insufficient, he made another slight cut. Both incisions were almost directly inwards. He then began to return the intestine, and, whilst doing so, about half an ounce of thin faecal matter escaped into the sac; and, on examining the intestine, an opening was detected in its anterior part, at the distance of an inch and a half from the stricture, about four lines in diameter, and the edges of which were very soft, dark coloured, and irregular. Within a few lines of this opening was also noticed a dark spot of nearly the same size, and evidently in a state of gangrene. Mr. Quain next passed a loop of thread through the sound portion of intestine at each side of the opening, and, with some difficulty, reduced the rest of it, leaving the opening itself in the neck of the sac, where it was retained by means of the loop of thread, the ends of which were tied over a fold of lint. The protruded omentum having been cut off, the wound was simply covered with lint. Soon after the patient had been put to bed, the house-surgeon found it necessary to tie two small omental arteries, and, at this time, during a slight effort made by the patient, a gush of thin serous fluid, slightly tinged with blood, took place from the wound. Lint, wetted with cold water, was now applied.

January 31st, 1 o'clock A.M.—Occasional discharge of the same kind of fluid from the wound, but no return of bleeding. Patient feels easier; belly is less tense, though tender on pressure; no vomiting. Pulse 60.

℞ Hydr. Chlorid. gr. ij.; Morphiae Hydr. Chlor. gr. $\frac{1}{8}$; ft. pil. quaque hora sumenda. Poultices to the abdomen.

8 o'clock A.M.—Has slept since 4 o'clock; is now remarkably easy. Tenderness of abdomen diminished; tongue yet dry, but clean; no vomiting. Pulse 68. Bed-clothes thoroughly wetted with the thin fluid, slightly tinged with blood, discharged from the wound. Some air escaped from the opening in the bowel, and the griping pains which had been occasionally felt were relieved by it. Same means continued.

1 o'clock P.M.—Tenderness of abdomen continues.

Ant. Tart. gr. $\frac{1}{4}$, with the Calomel, every four hours; and sinapism applied to the abdomen.

10 o'clock P.M.—Discharge of fluid from the wound much lessened; belly rather tympanitic, and tender on pressure; pulse 76; disagreeable smell emitted from the wound, and the lint stained with dirty brown matter; bowels not open. To continue the same plan, with eighteen leeches to the hypogastrium, followed by fomentations and poultice.

February 1st.—Has rested well; a free discharge of fecal matter from the wound; pulse 86.

2nd.—Small doses of the hydrarg. cum creta substituted for the pills of Calomel, &c.

3d.—Tenderness of the abdomen diminished. A copious evacuation of fecal matter from the rectum: that from the wound lessened. The loop of thread is withdrawn.

Vespere.—In the early part of the day five or six loose stools, with griping and tenesmus, and some traces of blood in them.

To discontinue the pulv. hydrarg. c. creta, and have an enema of starch mucilage, with ʒj. Tinct. Opii, and take $\frac{1}{4}$ gr. of morphia.

From the wound thick green feces discharged.

4th.—Pulse 60. Tongue furred. Bowels moved three times in the night. Tenesmus continues, with tenderness in the umbilical region. Ordered

℞ Liq. Opii Sedativ. mʒj.; Aq. Menth. ʒss. ft. haustus 4tis horis sumend. A sinapism to the epigastrium, and fomentations.

Vespere.—Bowels still much relaxed. Pulse 70, and compressible.

Omit. Liq. Opii Sedat. and have

℞ Pulv. Cretæ Co. c. Opio, ʒj.; Pulv. Catechu, gr. x. 4tis horis.

5th.—Tenesmus and griping relieved; two or three fecal evacuations in the night, principally from the wound. Beef-tea, with wine and milk.

In the evening a considerable discharge of feces from the wound; tenderness of the epigastrium; loss of appetite. Pulse 72, and fuller. Tongue white.

Hirud. xii. ad epigastrium, and omit the powders.

6th.—Feces abundantly discharged from the wound. Patient seems low. No tenesmus, yet frequent inclination to go to stool.

7th.—Generally better. Discharge of feces sometimes from the wound; sometimes from the rectum. Wine with arrowroot.

8th.—A free evacuation this morning per anum. The fecal discharge from the wound lessened.

9th.—Bowels opened three times in the night. Six leeches on epigastrium, for pain in that region. Wound granulating.

10th.—Improving. All tenderness of ab-

domen has ceased. One regular motion, and less fecal discharge from the wound. A mutton-chop allowed.

14th.—Went on well to this date. A good deal of discharge of feces from the wound to-day, and parts around excoriated, which were relieved by being smeared with lard, and latterly bathed with a lotion, composed of oxide of zinc diffused in water.

15th.—An aperient clyster administered.

16th.—This produced a copious evacuation from the large intestines, and much general relief to the patient. One mutton-chop and two ounces of wine daily.

20th.—No blood with the stools, and less discharge of fecal matter from the wound.

24th.—Vegetables prohibited; wine, beef-tea, &c. continued. Going on well.

March 4th.—No fecal discharge from the wound since yesterday evening, and it appears to be healed. Bowels open every day.

5th.—Cicatrization complete, and health good.

REMARKS.—No doubt this case was originally an omental hernia, and complicated at an early period with adhesion of the omentum to the neck of the sac, so as to account for its always having been, as the man described, irreducible. In his laborious employment at the gas manufactory, on the morning specified, a piece of bowel protruded, and the case changed into a strangulated entero-epiplocele. The femoral hernia, though not common in males, is not so rare as Scarpa supposed, but often overlooked on account of its small size. In men, however, of laborious employments, like our patient, whose work consisted in carrying heavy sacks of coals, and who have been long afflicted with such a rupture, the tumor, if not kept up (as this seems not to have admitted of being) will gradually attain a size that must excite the patient's notice; and in the case before us we find that the swelling was, after the addition of the bowel to the contents of the sac, as large as a goose's egg. In women, though the hernia is likewise generally small, examples are to be found in which it occasioned a tumor extending two-thirds of the way down the thigh.

The case at present under consideration at all events cannot fail to make you remember that men are sometimes the subjects of femoral hernia; and that, with reference both to the taxis and the operation with the knife, you will be likely to commit serious and dangerous blunders, unless you make out the true nature of the hernia by careful examination. Now the femoral hernia may always be known by Poupart's ligament being above the neck of the tumor. In an inguinal hernia, the spine of the os pubis is

behind and below this part of the neck of the sac; but in the femoral hernia, it is on the same level, and on the inside of it.

In the particulars of the case before us, Poupart's ligament is mentioned as being perceived to extend over the neck of the tumor, the fundus of which is also described as being thrown up over that ligament, as usually happens when the hernia is of a certain size. The remarkable tightness of the stricture—a circumstance particularly common in the femoral hernia of the male subject—was likewise exemplified in the present instance.

The gangrenous and ruptured conditions of the protruded bowel, as discovered when the hernial sac had been laid open, though discouraging, did not, as you have seen, absolutely preclude the possibility of a favourable termination. Here the adherent and diseased portion of the omentum was removed; but the partially gangrenous bowel, which had burst, not being adherent, and having but a limited opening in it, was reduced, and the burst portion of it kept in the neck of the sac by means of the threads tied over a roll of lint. This was done that the fecal matter might escape outwards, and not pass into the cavity of the peritoneum. Had the bowel been fixed by adhesions to the neck of the sac, then, of course, no ligature would have been necessary, or proper.

The copious discharge of serous fluid from the wound, as related, came, as I need hardly say, from the peritoneal cavity. I have witnessed several instances of the same occurrence.

One point deserving notice in this case was the mildness of the constitutional disturbance, notwithstanding the complication of the hernia with gangrene and rupture of the intestine.

The case also adds another instance to several which have been seen in this hospital, where, under strict and persevering antiphlogistic treatment, and especially under judicious diet, and with proper attention to cleanliness, nature not only saves the patient, under the disadvantages which this man experienced, but prevents the continuance of that loathsome affliction—an artificial anus.

The discharge of serum, and the tenderness of the belly, were regarded as indications of a tendency to inflammation in the peritoneum, and the calomel and other means prescribed were intended to control any increased action in that membrane. The calomel and tartarized antimony, however, were discontinued when signs of gastric irritation began; or when the mucous, and not the serous membrane, appeared to be the seat of disorder.

CASE II.—*Traumatic Tetanus*

Is the most fatal form of the disorder,—that over which the most skilful treatment and the most powerful medicines seem to have but little influence. The proportion of persons, who recover from an attack of it, is exceedingly limited. It may be excited by a wound, an ulcer, or a burn; and is much more rarely cured than idiopathic tetanus.

Edward Hays, æt 37, a robust bricklayer, of intemperate habits, admitted under Mr. Quain, Feb. 19, 1841. A month ago he trod upon a rusty nail, which penetrated the sole of his right foot, immediately below the outer ankle. He went on a few yards, and then took off his shoe, and extracted the nail from the part. Notwithstanding the injury caused a good deal of pain, he continued to work, but in three or four days he was obliged to rest himself, and apply a bread and water poultice. On the following day some purulent matter was discharged from the wound, and he felt great relief from it. At the end of two more days, he found himself able to return to his usual labour, in which, besides living intemperately, he was much exposed, first to frost, and afterwards to rain.

On Friday, Feb. 12, without having been conscious of any premonitory symptoms, or having suffered any uneasiness in the injured foot, he began to feel considerable stiffness and difficulty in moving his jaw, and pain at the back of his neck and in the pharynx. On the same night he slept but little, and on Saturday the rigidity of the jaw and the pain were increased; he was feverish, and his bowels confined.

15th.—Pain and numbness all over him, except in his arms. Able to stand up, but afraid to walk; and, in the course of the day, the muscles of the trunk and lower extremities were seized with spasm, from which, however, those of the arm remained free. The pain was most severe under the sternum, and the paroxysms of spasm came on twice or thrice every hour. If he happened to be standing at the moment of one of these attacks, he was thrown down on his face, the body being violently flexed forwards. He could neither sleep nor put solid food into his mouth, but took some porter and tea.

16th.—Had taken no medicines; spasms more frequent.

17th.—Visited by a medical practitioner, who bled him *ad deliquium*, and gave him some pills, which acted upon his bowels twice. The frequency of the spasms, and the severity of his sufferings, do not appear to have been lessened by the venesection.

On his admission, the 19th February, at ten o'clock A.M. his countenance was expressive of great anxiety and suffering, and

he was covered with profuse perspiration. The paroxysms came on every two or three minutes; but the spasms did not extend to the upper extremities at all, and the disorder had now put on the form of opisthotonos. The paroxysms only lasted for a few seconds, but created such agony, especially in the epigastrium, that the patient screamed out. In the intervals there was no tonic spasm; but any pressure on the abdomen, or any attempt to move, brought on a paroxysm. During the intermissions, however, the jaw was fixed, though not completely closed, and the apex of the tongue could be protruded. Pulse 120, and very feeble; bowels open; feeling of sickness; purplish discolorations in various parts of the lower limbs; respiration somewhat hurried, and laborious.

R Ferri Sesquioxidi, ʒj. ; Theriacæ, ʒj.
Omni hora sumend.

Beef-tea ad libitum.

Half-past 11 A.M.—Passed a copious loose dark-coloured motion, of very offensive smell; difficulty in swallowing and speaking.

In the afternoon he had two motions, with which some blood and shreds of lymph were blended; heat about anus, and constant tenesmus; a paroxysm of spasm every second or third minute; pulse 120; debility greater. The medicine and the beef-tea readily taken.

Half-past 2 P.M.—Paroxysms now less frequent, but not less severe. They come on every five or ten minutes; and in the interval the patient doses. Gurgling cough; mucous rattle heard all over the chest, but most loudly on the left side; breathing more difficult; upper extremities cold and clammy, lower tolerably warm; face and trunk warm, but covered with a clammy perspiration; pulse 120, and feeble; little, if any, tonic spasm.

Half-past 3.—Getting weaker; pulse 126; two stools similar to the last.

4 P.M.—Bowels moved twice during the last hour; pulse the same; spasms more violent, but less frequent; no sleep during the intervals; difficulty in swallowing the beef-tea. Oxide of iron continued.

R Camph. gr. iij. ; Opii, gr. j. Ft. pil.
omn. hora sumend.

Acetate of cantharides applied to upper part of the leg, to raise blister; and after removal of cuticle, six grains of hydrochlorate of mercury to be applied under the water dressing.

Half-past 4 P.M.—The acetate of cantharides failed to produce vesication; trunk and limbs rather cold; and the patient's face, chest, and arms, covered with cold clammy perspiration: great restlessness; bowels moved twice during the last half hour; pulse

132, feeble and irregular; breathing laboured; mucous rattle all over chest; paroxysms the same in force and frequency.

Takes beef-tea frequently; and at his desire some common tea is being made for him.

Half-past 5 P.M. — Attacked with a violent paroxysm of spasms, extending to the upper as well as the lower extremities; and this being soon followed by another, he expired.

Sectio cadaveris, 65 hours after death.— Membranes of the brain more vascular than usual; the arachnoid somewhat opaque and thickened, and both over and under it a large quantity of transparent serous fluid.

The vessels at the posterior aspect of the spinal cord were enlarged, of a bright scarlet colour, and presented a remarkably tortuous appearance; the vessels on the fore-part of the cord were of a dark colour, and less numerous.

The bronchial tubes contained a large quantity of viscid frothy mucus, and the vessels of the mucous membrane were a good deal congested; the lining of the larynx, pharynx, and trachea, exhibited a highly congested state; but that of the œsophagus was natural in its appearance.

In the stomach was found a quantity of the oxide of iron, very little changed.

The body externally presented the discolorations observed before death: a small black spot near the right heel denoted the point where the nail had entered; but no lesion of any branch of a nerve could be detected, and there was no suppuration or abscess discoverable in the part.

REMARKS.—Some wounds are found to be more likely to occasion tetanus than others. Lacerated and punctured wounds more frequently bring it on than incised ones; and lacerated injuries of the thumb, fingers, or toes, and punctured wounds of the sole of the foot, are well known as having frequently been followed by tetanus. The case before us confirms this observation.

In hydrophobia, a longer interval takes place between the period of the local injury, and that of the commencement of the constitutional disturbance, than in traumatic tetanus. If the latter disorder did not begin within twenty-two or twenty-three days after the occurrence of the wound, our soldiers in Spain were deemed safe from its attack. Difference of climate, however, may make differences in this respect, for in Egypt the 17th day was found, by Larrey, to be the latest of the attack. Our patient, according to the particulars, was probably attacked about the 23d or 24th day after the accidental injury. Tetanus begins either when the wound is healing, healed, or in a

most unfavourable state, foul, or even gangrenous. Our patient affords an example of the puncture having healed up before the disorder commenced.

He died, also, at one of the most usual periods of traumatic tetanus, viz. about the 6th day from its first attack; though you should remember that the disease is sometimes more chronic, and then the patient may live much longer, and has a better chance of recovery. One remarkable feature in our case was the great number of evacuations of bad quality from the bowels: more generally, there is obstinate constipation, requiring croton oil and other powerful cathartics. Abernethy believed that tetanus was intimately connected with a wrong state of the secretions in the digestive canal: the bad quality of the evacuations in this instance would rather give support to his view.

The sesquioxide of iron was tried on this patient; but, as you see, without any evidence of its having even produced a mitigation of the disorder. I have tried it also; but without success. I mention this not to dissuade you from having recourse to it, because it seems to me to deserve further trials, several well-authenticated instances of recovery under its employment being related. This patient came into the hospital, however, too late for a prospect of benefit from any plan. The appearances noticed in the *post-mortem* examination mostly agree with the descriptions given by the generality of careful observers; especially the congested state of the mucous membrane of the pharynx and respiratory tubes, and the enlarged tortuous appearance of the vessels of the spinal cord.

No injury of any branch of the external plantar nerve could be detected in the dissection. I advert to this, as bearing upon the question what would have been the effect produced on the course of the disorder by the division of the posterior tibial nerve behind the inner ankle on the commencement of the attack on the 12th of February; a week before he entered the hospital. This operation has been performed with decided success, as you may learn from the particulars of a case recorded in one of the volumes of the *MEDICAL GAZETTE*. To afford any chance of benefit, however, I believe that very early recourse to this proceeding is essential. It must be done on the tetanic symptoms first shewing themselves. The incisions, sometimes practised round the wound, appear to be undertaken on the same principle, namely, that of cutting off all communication between the injured parts and the centres of the nervous system.

THE HOSPITALS OF METZ.

THE hospitals of Metz do not compose a whole, but preserve the distinctive stamp of their respective foundations, and are not united by that financial bond which constitutes one of the advantages or inconveniences of centralization. The congregation of which St. Vincent de Paul was the creator, and is still the life and soul, manages three *hospices*, namely, the charity and orphan-house, and the hospitals called *Bon Secours* and St. Nicholas. The last one is of a mixed kind, and resembles those which in most of our great provincial towns are called general hospitals, as at Montpellier, Toulouse, &c. St. Nicholas's hospital is at once a retreat for indigent old age, and a medical asylum for the scrofulous, the epileptic, and different incurable affections; foundlings and deserted children are also admitted there. In spite of the order which reigns in this mansion of so many guests, it is impossible to approve of such an arrangement. It is not at Metz alone that we have to lament the extent of such an institution; these immense hospitals are a piece of municipal vanity, when they are not the unwholesome result of buildings successively heaped together by economy or routine.

The *Bon Secours* is the real hospital of Metz, the clinical theatre which from year to year, from season to season, displays the phases of local disease; it is there alone that there is a succession of patients, and that observation can be exercised on fresh objects; but we are sorry to say that it is far from being an institution which should have had the double object of liberal charity, and the progress of the art of healing. It is situated near the ramparts, in a part of the town where the indigent are crowded together; its back looks on the stream of the Moselle, from which it is separated only by a small garden; nor does its internal arrangement compensate for the badness of its position. It cannot hold more than 140—160 beds. When I visited the hospital, however, with my friend, Professor Forget, who was then going the circuit at Metz, as president of medical juries, the number of beds was more than enough, as there were not more than fifty patients in the house; but we learned from the superior sister, that at certain periods of the year, disease increases so suddenly, that it is necessary to refuse admission to a great number of applicants, who are obliged to content themselves with being visited at home and receiving medicines gratuitously. There are two large wards on the ground floor; they are paved, and it must be difficult to warm them in winter. There is the same arrangement on the next floor, with

some additions. Fresh air is easily introduced, and one observes that exquisite cleanliness (*cette coquetterie de lustre et de propreté*) which distinguishes houses entrusted to the rule of female corporations. All the wards are not yet supplied with iron bedsteads, and the space between the beds did not seem to us in proportion to the mass of atmosphere; this inconvenience is increased by enormous pillars, which might be usefully replaced, as at the Val-de-Grâce, by thin cast iron-columns, the slim and elegant supporters of the upper story. In going over the wards we saw venesection performed by one of the sisters; and we learned, not without surprise, that the hospital has neither in-door nor out-door dressers, and that the dressings, as well as the small operations of surgery, are executed by the sisters. There is no house-surgeon; no apothecary; the shop also belongs to the sisters; one of them presides over the composition of diet-drinks, and the preparation of the daily prescriptions. The hospital receives the chief medicines and chemical products from a druggist in the town; and a dexterous and exact sister weighs, doses, combines, mixes, triturates, dissolves, edulcorates, and adds the usual ornament of a label to the daily series of her elaborations. It would be impossible for us not to do justice to the symmetry of the internal arrangements of the laboratory, the cleanliness of the pots and utensils, the brilliant polish of the scales, the spotless white of the linen in the wards, the affectionate attention of the sisters to the patients, and the quickness of their movements round beds where patients were to be dressed; but we confess that all this was unable to reconcile us to a hospital without in or out-door pupils, without apothecaries, without any scientific impulse in the details, without the steady control of a superior jurisdiction. These offices ought not to be confided to female hands: give a house-surgeon to this collection of patients, among whom Science, like the Deity whom their pain invokes, ought to be always present, always attentive; open these wards to the intelligent observation of pupils, who, at a later period, after having received the doctoral stamp, will be to Metz practitioners early familiarized with the morbid forms of the place, and the genius of indigenous disease. The sisters, moreover, have the exclusive right of walking in the only garden of the hospital; it is clear that the patients ought to be admitted to this privilege. We diligently inquired of the superior what was the diet of the house, and both M. Forget and I were convinced that it is insufficient for convalescents, who should be nourished as well by the quality as the quantity of food. This defect, and many

others, (such as the want of particular clothing for the patients) arise from the scantiness of the funds at the command of the governors; indeed, the annual budget of the town will not allow of a complete remedy; yet it cannot be doubted that the more direct and decisive intervention of medical authority in these hospitals would correct some of the errors which prevail there.

The *Bon Secours* is appropriated to town patients; those from the department are admitted only when the communes to which they belong pay for their maintenance. A sad necessity! The gates of a hospital ought to open to suffering, from whatever village or market-town it may have dragged its slow length along. Why ask for a certificate of birth and origin from the man who is about to die? Does charity know the limits of departments, stamp its deeds with the seal of a commune, or select its harvest on a privileged soil? Our fathers built the house of the sick by the side of the house of the Lord, and they called it the Hôtel Dieu, consecrating it as a pious and helpful hostelry for all the pains which wend their way, lame and crooked, over this earth. As the gate of the church is open to every devotion, that of the hospital ought to yield to each languid hand which knocks at it. Governors and physicians, suffer all the sick to come to you, as our Divine Master suffered all children: do not the ailing and the weak compose the perpetual childhood of humanity?

A thought occurred to us at Metz which has already been expressed in this journal, and which our pen will never be tired of repeating; a thought which contains in itself a host of reforms and improvements. It is simply this:—let the government take into its hands all the property and income belonging to all the hospitals and *hospices* in France; and let every department have its share in proportion to its general population and its annual contingent of sick poor; and let all these establishments form part of a general system, governed by a uniform statute vivified by a homogeneous administration, and let it be under the parliamentary control of the Chambers.

Once more we announce this wish, which we number among the desiderata of each new year; may it germinate, and fructify; may it one day ascend the tribune, in the person of some honourable man imbued with the real interests of the country; and may a minister, capable of occupying himself with something besides the preservation of his portfolio, embody it in the form of a bill! While waiting for this miracle, let us finish our rapid glance over the medical institutions of Metz.

One of the most useful is the practical school of midwifery of the department. It

is under the direction of Professor Morlanne, who, with two colleagues, bears the burden of teaching. There is a course of midwifery, and a *clinique* for the diseases of children, and of women in childbed; and gratuitous advice is given daily to town and country patients. This advantage seems to be appreciated, for last year no less than 1423 persons applied for advice. The *Hospice de la Maternité* is distinct from the Ecole d'Accouchement. It was founded in 1334, by a citizen of Metz, named Jean La Hungre, for poor pregnant women; and the deed was confirmed in 1337, by the bulls of Pope Clement VI. The sisters of St. Felicity attend in the house, and, which seems strange for nuns, are both midwives and nurses.

Besides these public institutions, Metz affords other relief to the poor. The Sisters of St. Christian, whose chief residence is in this town, and who pass their noviciate here, devote themselves to the sick, and to the instruction of the poor. Their assistance is sought by the sick of every sect, which is the finest panegyric that can be given them.

The municipal board of charity has divided the town into five sections, and gratuitous advice with home visiting are to be found in each. With an honourable solicitude, the physicians of Metz share among them this domain, where science meets religion in order to bring living streams into a withered land, and in the same beings to revivify the body which decays, and the soul which degenerates.

There is an Israelite Hospital, established by a royal ordinance of May 5th, 1832; it contains only eleven beds.

The midwifery school is of secondary rank, and is intended for midwives alone.

For science, there is a military hospital of instruction, and a medical society. The latter participates in the lot of all provincial academical associations; there are alternations of languor and life; and years of silence and of death are followed by a sign of life in the shape of a volume. The Metz society has lately published a volume, and I arrived just in time to receive it from the skilful and learned Dr. Maréchal. Some day I shall return to it, as it contains several interesting papers; in particular, a very good report on the epidemic cholera at Metz. The book has a tendency to usefulness which I will not pass over, and is especially occupied with questions of public hygiene as applied to the city of Metz. This society has been engaged with the useful, while so many others are like the astrologer in the fable.

The hospital of instruction is, without doubt, the true representative of the medical science of Metz, from the scale on which it teaches, from the merit of the men who

have successively directed it, from the extent of the practical theatre which the art possesses there, and from the influence which it exercises over a body of pupils frequently renewed, and many of whom have taken a distinguished rank in civil and military medicine. The hospital of instruction has, moreover, had the honourable privilege of furnishing the town with its favourite practitioners. There is not an inhabitant of Metz who does not pronounce with respect and gratitude the names of Gorsy, Marchand, Rampon, Moizin, and Wullaume; the military hospital has supplied the Institute with an eminent chemist—Serullas; and has given two inspectors to the Army medical board—MM. Moizin and Brault. M. Bégin, now first surgeon at the Val-de-Grâce, belonged to this hard-working school; Boisseau, author of the "*Pyrétologie Physiologique*," died there; it has just sent M. Scoulteten, author of the *méthode ovariaire* to the Strassburg Hospital; and a number of other remarkable persons belonging to the Metz hospital of instruction. The distinguished men who now conduct it are unable to reap all the fruit of this long-enduring reputation, or to maintain the dignity of the institution against the injustice of the military spirit which rules at Metz, and casts its shade over every sort of merit which is not expressed by a big epaulette. Fortunately, science escapes from garrison influence, and moves at ease in a vast and handsome establishment capable of holding 900 patients; and fortunately, the *officiers de santé* of the army, and the body of teachers at their head, have never sought for the springs of their emulation and their zeal anywhere but in themselves*.

CASES FROM THE DANISH MEDICAL REPORTS.

DR. RHODE, of Friedensburg, reports—

1. *An interesting case of scrofula treated with iodine.*—A young lady, of twenty-three, in consequence of a fall on the trachea in her twelfth year, had a swelling there, which was immediately pronounced to be strumous, and had gradually increased until she came under Dr. Rhode's care. It extended from the posterior edge of the sterno-cleido-mastoideus on one side of the neck, to the same situation on the other, and from above downwards from the submental region to the upper paries of the sternum. The treatment consisted in the exhibition of burnt sponge, ointment of the hydriodate of potash, tincture of iodine, and afterwards puncturing the tumor. Great advantage was derived from these measures.

* Abridged from an article by M. Michel Lévy, in the *Gazette Médicale* of February 6, 1841.

2. *Internal abscess opening into the intestinal canal.*—A countrywoman, aged thirty, four days after recovering from an attack of sporadic cholera, was seized with suppression of urine, and pain in the abdomen, which were followed by a swelling between the umbilicus and the symphysis pubis, a little towards the left side. The case was terminated, and a cure effected, by evacuations of foetid yellow pus by stool.

Remarkable case of secondary syphilis.—A man, who had previously suffered from secondary syphilis, for which he had taken a strong decoction of sarsaparilla and hemlock pills, a year after this treatment, (no secondary symptoms having occurred during the interval,) was suddenly seized with strabismus, without any known cause; and it gradually reached such a pitch, that both pupils were strongly turned towards the nose, whenever he wanted to look either at a distant or near object. As he had previously suffered from caries of the external bones of the nose, I was afraid that a similar disease might have arisen in the orbital bones; and that caries or exostosis in the orbit produced the phenomenon by pressure on the nerves of the eye. As the patient refused to go through a course of sarsaparilla again, on account of his occupations, I prescribed pills of corrosive sublimate, beginning with one-tenth of a grain a day; and at the same time I placed an antimonial plaster on the back of his neck. When about three grains of the sublimate had been taken, the strabismus disappeared; it was succeeded by unquenchable thirst, and a total cessation of the secretion of the skin. The pulse became quick, evacuations by stool were difficult, unfrequent, and clayey; the patient lost his appetite, and the quantity of urine increased so much, that thirty ounces were usually evacuated in a night. The urine was as clear as water, and without odour, deposited no sediment, and, as appeared by chemical analysis, contained no sugar. Hence it was a case of diabetes insipidus, which had, perhaps, arisen from taking cold. Sudorifics, and milk of sulphur to keep the body open, soon removed these symptoms, and the patient, who was rather attenuated, recovered entirely, and is now quite well.—*Zeitschrift für die gesammte Medicin*, from the extracts made by Professor Otto, of Copenhagen, from the Official Reports sent to the College of Health.

"A TRADING CORPORATION."

To the Editor of the Medical Gazette.

SIR,

My attention has long been directed to the all-absorbing topic of "Medical Reform,"

and I have been much struck by observing in nearly all the communications on that subject, the just meed of praise that is given to the Society of Apothecaries for the gradual, cautious, but consistent mode in which they have advanced the education of the medical student, and thereby raised the character of the apothecary or general practitioner; yet with all this praise there is uniformly mixed up the dreadful charge of their being a trading corporation, and the disgrace that is incurred by the profession in having the conduct of the education and examination of the apothecary entrusted to a "trading body." Now any indifferent person hearing this accusation would be led to suppose that the Society of Apothecaries was composed of a set of petty dealers, and that the apothecary himself was a person too dignified to sell or buy anything. I have carefully examined the list of the members of the Society, and find among them names of men not altogether unknown to science, and some indeed even distinguished in several departments of medical knowledge. But in this highly aristocratic country, I am well aware that the imputation of "trading" is most offensive; yet it appears to me rather strange that the Society of Apothecaries should be reproached for doing that in their collective capacity, which each apothecary individually is anxious to do; for I conceive that as soon as the tyro has passed his examination, he is most anxious to dispose of as much rhubarb and jalap as he can persuade the public to swallow.

I am old enough to recollect the first agitation of the Apothecaries' Act in 1814: at that time it was first offered to the College of Physicians, but they declined to have any connection with the administration of an Act to regulate the education of the apothecary, conceiving that beneath their dignity; in like manner the College of Surgeons repudiated the measure, being, as they said, the "conservators of pure surgery;" but now that the Society, by their judicious regulations and strict examinations, have created a body of many thousands of efficient practitioners, the members of the two learned Colleges can boldly assert, or slyly insinuate, that it is degrading to entrust the education and examination of the medical practitioner to a "trading body." It is a cry that has been raised by some interested individuals, and to serve their own purposes, and is taken up and echoed by those who never allow themselves to think upon any subject, but follow servilely the track that others have made.

March 24, 1841.

GILES JALAP.

THE COLLEGE OF PHYSICIANS.

To the Editor of the Medical Gazette.

SIR,

I HAVE seen a document in which it is proposed that the Royal College of Physicians of London shall admit indiscriminately to their license, without examination, but on payment of "fees," the medical graduates of the British Universities at large, being twenty-six years of age. Now, conceding as I do, that there might be reasons for the admission of practitioners of known character and professional skill, without being "put through the mill," I beg leave to remark as to the sweeping measure proposed:—

1st, That if practitioners of immoral, unprofessional, or even unknown characters, or imperfectly qualified, be contained among the class of persons contemplated, a voucher from a college, constituted for such purposes as the Royal College of Physicians, and formed of members acting on oath, that such practitioners are trustworthy, would, to say the least, be a most monstrous proceeding.

2dly, That if there has been such a strict performance of duty in the medical department of the English and Scotch universities, as to render their existing degrees sufficient passports to the license of the College of Physicians, without further examination, and to public confidence, there can be no use in the College as a controlling authority. Far less would the legislature be justified in increasing its powers, or extending its jurisdiction, as recommended. Such a measure might benefit the College and its new licentiates, but it would not be serviceable to the public, or just towards future graduates of the immaculate universities in England and Scotland.

The advocates of this (what they are pleased to term) "reform," must choose, then, between the horns of a dilemma. If the after-mentioned universities have failed in their duty, would a college, vouching on oath for its due performance heretofore, be the most eligible institution to entrust with new and extensive powers? If, on the other hand, the universities have acted well, why distrust them hereafter, or do any thing but give them full scope?

There is no analogy, in the Apothecaries' Act of 1815, for the plan. The act saved existing rights, but gave no one what he had not before, or any thing but mere immunity. Existing practitioners were enabled, as previously to the act, to practise without incurring further penalty; but they were not palmed on the public as licentiates of the Apothecaries' Company. If, then, there are to be new penal powers, ex-

empt existing practitioners from the influence of such; but do not buy their co-operation by giving them a false name.

I am, sir,

Your obedient servant,

A PROFESSOR IN THE SCHOOL OF
PHYSIC IN IRELAND.

Dublin, March 1841.

ANSWER TO DR. BUCHANAN'S
LETTER ON HIS COMPOUND
CATHETER.

To the Editor of the Medical Gazette.

SIR,

I WAS not a little surprised on reading in the last number of your publication a letter from Dr. Buchanan, of Glasgow, in which that gentleman boldly claims the honour of inventing twenty long years since the improved catheter which I introduced to the notice of the profession in your journal of the 22d of January last.

Now, after allowing the Doctor all the honour justly due to his age and experience from one so young in the profession as myself, I feel bound to set the public right, and to show them that I never saw the Doctor's instrument, or heard of it from any "catheter-maker," who "called upon him from England." The design originated in my own mind a few months ago, when witnessing the agony which patients endured from the delay arising from the use of the common catheter. The Doctor says my instrument is in all essential respects identical with his own; this I beg leave to affirm is not the case; and to prove this, I entreat your readers to compare the drawings of both instruments.

Dr. Buchanan, in laying claim to my invention, displays a vast deal of erudition, and clearly shows that he has travelled much farther north than I, who have never visited Scotland. The Doctor allows that "it is quite possible that the same principle may occur to the minds of two different individuals without any communication;" and continues still smoothing me over by allowing that the instrument will prove "a highly useful one." All this is done merely with a view of attempting to gull the public into a belief that nothing good or useful in medical science can be invented out of the precincts of Edinburgh or Glasgow. That the Doctor is attempting to make this impression upon your readers is self-evident; but like many of his boasting forerunners of Scotia, he will only excite the risible faculties of all men of sense, whilst "out of his own mouth" he condemns himself and shows his motive. Hear the learned Doctor: "it is quite possible that the same principle may

occur to the minds of two individuals;" but hear him again: he hopes that Mr. Foulkes, and every other man who takes the trouble to invent instruments, will allow what he (the Doctor) is now labouring so hard to establish, "that *I did invent* the said instrument about the year 1822."

I am not going to assert that Dr. Buchanan did not invent a compound catheter in that year: but I am bound to ask, where did he place the record of his useful invention? in the medical or other public journals? No; but, as he tells us in his letter in your last Gazette, in *his Note Book*;" and this, forsooth, not in the year 1822, but in 1831, when he got one of the instruments constructed.

Now, with all due deference to the Doctor, I think, sir, if he really had invented an instrument possessing any of those qualities which would relieve his fellow men labouring under the direful effects of stricture, he should not have been so selfish as to withhold it for years from the profession; but have acted with the same candour as myself, and the moment he proved its utility given publicity to it. Trusting you will insert this explanatory letter,

I remain, sir,

Yours most respectfully,

JOHN CROXTON FOULKES.

7, Great Homer Street, Liverpool,
March 16th, 1841.

LONDON DISPENSARY.

CASE OF CHRONIC LARYNGITIS, WITH PERTUSSIS, IN A PATIENT ÆT. 49 .

To the Editor of the Medical Gazette.

SIR,

THE peculiarity of the following case consists in the occurrence of a distinct and prolonged crowing noise, the whoop of whooping-cough, in a patient aged forty-nine, during violent paroxysms of coughing. So great was the resemblance, in the present instance, to whooping-cough in a child, that one of the medical officers of the dispensary, who heard the cough from another room, thought I was prescribing for a child affected with whooping-cough, instead of an adult.

Your obedient servant,

C. J. C. ALDIS, M.D.

Physician to the London Dispensary.

13, Old Burlington Street,
March 1, 1841.

Elizabeth A., æt. forty-nine , widow, housekeeper. Admitted Feb. 25, 1841. Pulse 72; skin natural; tongue very red at the edges and tip, furred at the base; bowels regular; urine free; catamenia regular.

Hoarseness of the voice, which is some-

times reduced to a whisper; occasionally natural sounds are produced; slight tenderness about the larynx, States that she is subject to violent paroxysms of cough, "like the hoop of a child, only louder." Tightness of the chest, and very scanty expectoration. The "hoop" produces vomiting if it commences after taking food. Is not troubled by the cough at night; sleeps pretty well. Hæmoptysis on Monday last, when she expectorated a dessert-spoonful of dark-coloured blood. There is dulness on percussion at the upper part of both sides of the chest.

Ailing twelve months. The present hoarseness since Tuesday week, but is subject to it "every cold she takes," as well as the "whoop." Has lost much flesh. Has taken "cough pills and opening medicine."

Having heard the paroxysm of coughing, I am able to state that it consists of a succession of expirations, interrupted occasionally by loud whooping, until the patient is nearly exhausted.

Applic. Emp. Canthar. summo sterno.
℞ Hydrarg. Chlorid. gr. j.; Pulv. Antimon. gr. ij. omni mane ad tres vices.
℞ Vini Ipecac. ℥ xv.; Sodæ Sesquicarb. ℥ ss.; Tinct. Opii, ℥ v.; Mist. Camphoræ, f. ʒj. 6tis horis.
℞ Mist. Cathart. cràs et p. r. n.

TESTIMONIAL.

MR. RICHARDSON, the eminent medical practitioner of Harrogate, having, after the arduous services of fifty years, resigned his practice to Dr. Kennion, it was lately resolved; at a meeting of his friends, to commence a subscription, for the purpose of placing a portrait of Mr. Richardson in the Harrogate Bath Hospital, from which portrait an engraving is to be taken for the individual gratification of the subscribers. Mr. Richardson's numerous medical friends throughout the country will learn with pleasure that the subscription amounts to about £200; each subscription being limited to two guineas.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, March 18, 1841.

D. S. Moore, London.—G. J. Eady, Atherston, Warwickshire.—W. A. Maiben, Brighton.—G. Gaskoin, London.—G. Wilkinson, Manchester.

BOOKS RECEIVED FOR REVIEW.

Mr. Skey's Practical Treatise on the Venereal Disease.

Mr. Lay's "Chinese as they are."

Dr. Kane's Elements of Chemistry, Part 2.

Dr. James Arnott's Treatise on Stricture of the Urethra, containing an Account of improved Methods of Treatment, &c.

A TABLE OF MORTALITY FOR THE METROPOLIS,

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 13th March 1841.

Small Pox	42
Measles	2
Scarlatina	11
Hooping Cough.....	53
Croup	12
Thrush	3
Diarrhœa	4
Dysentery	0
Cholera	0
Influenza.....	29
Typhus	21
Erysipelas	6
Syphilis	0
Hydrophobia.....	0
Diseases of the Brain, Nerves, and Senses ..	157
Diseases of the Lungs, and other Organs of Respiration	314
Diseases of the Heart and Blood-vessels	27
Diseases of the Stomach, Liver, and other Organs of Digestion	73
Diseases of the Kidneys, &c.	8
Childbed	7
Ovarian Dropsy	0
Diseases of Uterus, &c.....	2
Rheumatism	3
Diseases of Joints, &c.	4
Ulcer	1
Fistula	1
Diseases of Skin, &c.....	0
Diseases of Uncertain Seat	127
Old Age or Natural Decay.....	114
Deaths by Violence, Privation, or Intemperance	24
Causes not specified	4
Deaths from all Causes	1049

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

March.	THERMOMETER.	BAROMETER.
Wednesday 17	from 36 to 56	30.59 to 30.56
Thursday . 18	38 56	29.41 29.57
Friday . . 19	38 53	29.57 29.67
Saturday . 20	37 56	29.65 29.55
Sunday . . 21	39 53	29.56 29.59
Monday . . 22	47 57	29.33 29.48
Tuesday . 23	37 57	29.72 29.94

Wind, South on the 17th, and two following days; S.E. on the 20th; South on the 21st; S.W. on the 22nd, and following day.

On the 17th, morning cloudy, with rain, otherwise clear. The 18th, morning overcast, with small rain, otherwise clear. The 19th, generally clear, except the morning, when rain fell. The 20th, morning clear, otherwise overcast, raining frequently during the afternoon and evening. The 21st, generally cloudy. The 22nd, morning cloudy, with rain, otherwise clear. The 23rd, generally clear.

Rain fallen, .295 of an inch.

CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, APRIL 3, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXVIII.

Inflammatory and structural diseases of the spinal cord, continued. Treatment.

Apoplexy. Its general symptoms and diagnosis. Different forms of the attack. Predisposition to apoplexy—natural, and accidental. Precursory symptoms.

ALLOW me to repeat that the structural diseases of the spinal cord will most clearly reveal themselves, by their symptoms, to him who most distinctly perceives, and most accurately bears in mind, the *physiology* of that part of the nervous system. But to the best informed, and the most sagacious, they are too frequently obscure and perplexing.

Disease occupying a portion only of the cord, but affecting the *whole thickness* of that portion, from centre to circumference, will be likely to disturb, or suspend, the functions of sensation and voluntary motion in all the parts supplied with motor or sentient nerves by that portion of the cord, and by the portion beyond it. So that a great variety of symptoms depend, when the *amount* of disease is the same, upon the *place* of the disease. A total interruption of the conducting function of the cord, in the neck, above the origin of the phrenic nerves, extinguishes life by stopping the actions of respiration. A similar interruption in the cervical part of the cord, above the origin of the intercostals, but below the origin of the phrenic nerves, destroys life as certainly, but not so rapidly, nor in exactly the same manner. We find the lungs loaded with frothy

serous fluid in such cases; we find the bladder inflamed; and, often, sloughing of the integuments and muscles of the nates and hips. A similar interruption below the dorsal vertebræ is not *necessarily* fatal, even when it is attended with permanent paralysis: but it *usually* is so, sooner or later.

It is commonly believed that disease affecting the *anterior* columns only of the cord, will be likely to disturb, or to suspend, the *power of voluntary motion* in the corresponding parts; to produce *spasm* or *palsy*: and that disease affecting the *posterior* columns alone will be likely to alter or abolish the *faculty of sensation* in the corresponding parts; to cause *pain*, *tingling*, *numbness*, or *complete anæsthesia*. But I have mentioned certain facts which contravene this opinion. Suspend your judgment respecting it. Neither the minute anatomy nor the physiology belonging to the question are yet conclusively settled.

There seems no reason to doubt that disease affecting the *lateral* half only of the cord will be likely to derange *both* the sensibility and the power of movement, in the corresponding parts on the *same side of the body alone*.

If you impress upon your recollection the facts thus summarily stated, you will find in them, I think, a key to many of the phenomena which accompany, and denote, more or less plainly, disease of the spinal marrow.

Inflammation of the *membranes* of the spine is most apt to declare itself by *pain*, increased on motion, of the spine and of the limbs; and by *rigidity and spasm* of the muscles of the neck and back. Inflammation of the *cord* itself, which readily passes into, or rather produces, softening of its substance, is most commonly marked, first, with convulsive movements of some parts of the body; secondly, by palsy of those parts, with or without anæsthesia. The same may be said of *suppuration* when it occurs as an event of inflammation; and the pus may be collected

into an abscess in the nervous matter of the cord, or it may be diffused and mixed with softening.

Now I need not dwell upon the *treatment* proper to be adopted in inflammation of the spinal cord and its membranes. *Mutatis mutandis*, it is the treatment already recommended in inflammation of the *brain* and its membranes. When the inflammation is acute, we must take blood freely; from the arm, or by cupping glasses along the sides of the spine. Blood enough may be taken by cupping along this tract to produce the effect of general bleeding as well as of local. Perfect rest in the horizontal posture must be strictly enjoined. Mercury will generally be proper.

In more chronic forms of inflammation within the spinal canal, we still have a capital remedy in cupping: and counter-irritation in various ways, but more especially by means of issues made on one or both sides of the spinous ridge, is also, in many cases, of most essential and unquestionable service.

Great care must be taken, when there is palsy of the bladder, not only (as I admonished you in the last lecture) that the urine be regularly drawn off, but also that the patient be kept *dry* and *clean*: for if great attention be not paid to this point, sores will form where the urine remains in contact with the skin, to the great increase of his suffering and of his danger. Indeed, take what pains we may, there is always a strong disposition to the formation of sloughs upon the sacrum and hips in cases of paraplegia. They result from the perpetual pressure made upon those projecting points; from the feeble state of the circulation in the palsied parts; and (often) from the irritation of the urine and fæces, which are passed without the patient's consciousness.

When the patient is kept clean and dry, and the surfaces on which the weight of his body has been supported begin to be red or angry, you may protect them by a plaster: or by rubbing them with brandy you may sometimes prevent the skin from breaking: or, what is best of all, you may put your patient upon one of Dr. Arnott's hydrostatic beds; and then the pressure will be equally distributed over all that portion of the body which comes in contact with the waterproof material of the bed.

To bring this outline of the diseased states of the spinal cord up to that point in which we left those of the encephalon, I may state that, like the brain, the spinal marrow may become hardened by chronic inflammation; and, like the brain, it may be encroached upon by tumors; fibrous, scrofulous, or malignant. With respect to these, all that I can now say likely to be of any practical benefit to you, is that the symptoms they occasion are those of slowly increasing paralysis,

without fever or what is called reaction; and that the locality, and extent, and effects of the paralysis, will vary according to the part of the cord in which these changes occur, and the depth to which they affect it.

Apoplexy.—I proceed, in the next place, to a perfectly distinct class of diseases of the brain and spinal cord; to the apoplectic affections: and especially to cerebral hæmorrhage, and spinal hæmorrhage.

When a person falls down suddenly, and lies without sense or motion, except that his pulse goes on beating, and his breathing continues, he is said to have been attacked with *apoplexy*. He appears to be in a deep sleep; but this is not all, for you cannot awaken him by the same measures which would rouse a healthy man. He is not in a state of syncope, for his pulse beats, perhaps with unnatural force; and often his face, instead of being pale, is flushed and turgid; and his respiration goes on, though it may be laboured and stertorous. What I now denominate apoplexy, is the very same state which has so frequently been mentioned already in these lectures: it is *coma* occurring suddenly, or coming on (at least) with rapidity. What is coma? it is that condition in which the functions of animal life are suspended, with the exception of the mixed function of respiration; while the functions of organic life, and especially of the circulation, continue in action. There is neither thought, nor the power of voluntary motion, nor sensation. But the pulmonary branches of the par vagum continue to excite, through the medulla oblongata, the involuntary movements of the thorax. When this upper part of the cranio-spinal axis becomes involved in the disease, and its reflex power ceases, the breathing ceases also, and the patient is presently dead.

It is a common question—how would you distinguish apoplexy from the effects of a narcotic poison? If you were summoned to a person in the state I have been describing, how could you tell whether he was affected with apoplexy, or labouring under the influence of a large dose of opium, or merely dead-drunk? Why, so far as the condition of the cerebral functions is concerned, you cannot discriminate the one from the other. In each case there is profound coma; but the cause of the coma is different in each, and you must seek to ascertain that cause in the history and other circumstances of the patient: you enquire whether he is known to have been drinking, you try whether you can perceive the odour of spirits, or of wine, in his breath; or you endeavour to make out whether he has been low-spirited, or in known difficulties; in short, whether it is likely that he may have swallowed poison. But from the actual condition of his

sensorial functions, you cannot solve the question.

Yet let me say, thus in the outset of our remarks upon apoplexy, that it is often of great importance that the diagnosis should be determined. A man was found lying in Smithfield in a state of total insensibility, except that he still breathed. He was carried into St. Bartholomew's Hospital. The house-surgeon thought he smelt the smell of gin in his mouth; and thereupon very properly made use of the stomach-pump: by means of it he discharged a large quantity of ardent spirit; and in the course of a few minutes the man revived, shook his ears, and walked off. If the gin had been suffered to remain in his stomach, and if the remedies of apoplexy had been vigorously put in force, the absorption of the poison would have been thereby accelerated; and the debauch would probably have had a fatal termination. The same remarks apply still more urgently to the case in which opium, or any other strong narcotic poison, is lying in the stomach. Even when there is no great danger, either in the person's state, or in the remedies used for it, it is not a very pleasant or creditable thing to make a false point of this kind. If we do err, however, we had better err on the safe side. The father of the late Professor James Gregory, of Edinburgh (who used to relate the case in his lectures) was once called out very late in the evening to visit an old gentleman of that place. He found him in a completely comatose condition; his wife crying, and his household all plunged in grief and distress. They told him that the patient, whom he now saw in a fit, had come home, and upon the servant's opening the door to him, had fallen into the passage, on his back, in a state of insensibility. Dr. Gregory learned, however, that he had been at the "Club," and he knew well enough that this Club was composed of jovial spirits, fond of their cups; although the gentleman's wife did not know as much. Therefore he ventured to express his "hopes" to the wife that her husband was drunk: a charitable view of the case, at which she was extremely affronted and indignant. He persisted, however, in his opinion, and not long afterwards the patient began to recover his senses. It turned out that he had partaken more liberally than the rest of the Club, and was the *first* to be drunk. Two of his companions carried him home quite incapable of motion: but not liking to introduce him themselves to his wife in that predicament, they placed him with his back against the door, rang the bell, and walked away. Of course when the servant came to open the door, his master tumbled senseless on the floor. I need not point out to you the ridicule which the physician would have brought upon himself, and the damage he might have

inflicted upon his patient, had he busily applied, in this case, the ordinary remedies of apoplexy.

The state of coma, such as I have described as being characteristic of apoplexy, may terminate in one of three ways. It may cease, more or less rapidly, and leave the patient in perfect health. What is the exact condition of the encephalon during the continuance of the coma, in such cases, no one can positively tell. But the occurrence of temporary coma, under the influence of a narcotic poison, and the perfect disappearance of the coma as the effects of the drug pass off, teach us that the functions of the brain may be almost totally suspended for a time by causes which do not injure its texture. It is possible that the coma may depend upon that presumed disturbance of the balance of the arterial and venous circulation within the cranium, which I spoke of in a former lecture. It may be that the force and rapidity of the circulation in the cerebral vessels undergo some great alteration. It is still more probable (to my mind) that a temporary stress upon the cerebral blood-vessels (produced by a determination of blood towards the head, through the arteries, or by a detention of blood in the obstructed veins) may really exercise pressure enough to cause transient coma. But these are mere conjectures.

In the second place, the apoplectic coma may terminate, more or less quickly, in *death*. And on examining the brain we may find a large quantity of extravasated *blood*; or a considerable effusion of *serous fluid* in its ventricles, or beneath the arachnoid; or we may detect *no* deviation whatever from the healthy structure and natural appearance of the organ. The congestive pressure (if it indeed existed) has left no prints of its action.

The last is comparatively rare. Dr. Abercrombie has given to this form of apoplexy, which destroys life, but leaves no traces behind it, the name of *simple apoplexy*. And this name, for its convenience, I shall retain. Of the other two kinds of quickly fatal apoplexy, that in which *blood* is found extravasated is more common than that in which there is effusion of *serum* only. The one has been called *sanguineous apoplexy*; a better term is *cerebral hæmorrhage*: the other has been called *serous apoplexy*.

Thirdly, the apoplectic coma may terminate in *partial* or *imperfect* recovery. One, or all, of the cerebral functions may be left impaired; the mind enfeebled; the power of motion limited, or lost, in some parts of the frame; the faculty of sensation benumbed or extinguished: the unhappy subject of the attack remaining more or less crippled in body, and more or less maimed in intellect. In these cases, when at length we have an opportunity of examining the brain, we al-

most always find that there has been extravasation of blood, to a small or moderate extent. I say *almost* always, because I have myself, in more than one instance, carefully looked for such appearances, after such a series of symptoms, without finding them. Occasionally, instead of a clot of blood, we meet with circumscribed softening of the brain.

Modes of attack.—The *attack* of apoplexy does not always occur in the same manner: and Dr. Abercrombie has pointed out three several ways in which it is apt to come on. I am confident, from the result of my own observation, that the distinctions laid down by Dr. Abercrombie are just and true; and it is of importance that you should be aware of them. “In the *first* form of the attack, the patient falls down suddenly, deprived of sense and motion, and lies like a person in a deep sleep; his face generally flushed, his breathing stertorous, his pulse full and not frequent, sometimes below the natural standard. In some of these cases convulsions occur; in others rigidity and contraction of the muscles of the limbs, sometimes on one side only.

Now of persons seen in this condition, the immediate prognosis is *uncertain*. Some die in a short time, and much blood is found extravasated within the cranium. Some die after a rather longer interval, and then we often find serous effusion only, and that of no great amount. And in some that die early, no effusion either of blood or of serum can be detected. Some recover altogether, without any ill effect of the attack remaining. Others recover from the coma, but are left paralytic of one side, and with some imperfection of speech, or of one or more of the senses. And this paralysis and imperfection may disappear in a few days, or gradually subside, or remain for life.

In the second form of attack, the coma is not the earliest symptom. The disease generally begins with a sudden attack of pain in the head. The patient becomes pale, faint, and sick, and usually vomits; and sometimes, but not always, falls down in a state of syncope, or resembling syncope, with a bloodless and cold skin, and a feeble pulse. This also is occasionally accompanied by some degree of convulsion. Sometimes he does not fall down, the sudden attack of pain being accompanied only by slight and transient confusion. In either case he commonly recovers in a short time from these symptoms, and is quite sensible, and able to walk; but the headache does not leave him: after a certain interval, which may vary from a few minutes to several hours—and Dr. Abercrombie records cases in which it was even much longer—the patient becomes heavy, forgetful, incoherent, and sinks into coma, from which he

never rises again. In some instances paralysis of one side occurs; but perhaps more often, there is no palsy observed.

The disease, when it comes on in this way, is much more uniform, and of much worse omen, than when it commences after the former fashion. It is of great use to know this; for to an unexperienced eye the cases do not *seem* so terrible as those in which the patient becomes profoundly comatose from the very first. The apparent amendment is fallacious, and apt to lead one into giving a false prognosis. Very few persons come out of the coma, and a large quantity of blood is usually found extravasated in the brain. These cases are not, as Dr. Abercrombie well observes, apoplectic in the outset. They differ remarkably from the first set of cases. If there be at the very beginning some loss of sense or motion, it goes off again in a very few minutes, or perhaps in a few seconds: the prominent symptom, at the commencement, is sudden and violent pain of the head, with faintness, sickness, and often with vomiting. The pain continues, and is sometimes confined to one side of the head; the face is pale and ghastly, the pulse weak, and often frequent or irregular; but the patient is quite conscious, and in full possession of his intellect. At length he recovers his natural temperature, his countenance improves, and the pulse becomes stronger and steadier: then his face gets flushed, he feels oppressed, answers questions slowly, and at last sinks into stupor and fatal coma. The period between the first attack and the commencement of the coma is variable. Sometimes the stupor succeeds the pain and faintness so rapidly, that the case comes greatly to resemble those in which coma is the first symptom, and takes place suddenly; but still a short period of sense, commonly with complaint of great pain, may be observed. But the interval may be a quarter of an hour, or many hours, or even two or three days. “Upon inspection,” says Dr. Abercrombie, “we find none of those varieties and ambiguities, which occur in the apoplectic cases, but uniform and extensive extravasation of blood. [I should state that he calls the first class of cases *apoplectic* cases, the coma being present from the first: and the second class, which we are now considering, he calls cases *not primarily apoplectic*.] The symptoms in this form of attack depend, no doubt, upon the giving way of some one of the cerebral vessels. At the moment when the vessel is ruptured, a shock is given to the brain; a temporary derangement of its functions occurs; but this passes off. The circulation then goes on as before, until such a quantity of blood has escaped from the ruptured vessel as is sufficient to produce coma. There is no part of Dr. Abercrombie’s book more admirable

and clearly put than that which is occupied with these important distinctions, which I give you very much in his own words. He points out the close analogy which exists between this variety of apoplexy, and the result of external injuries, when they occasion extravasation of blood on the surface of the brain. The hurt person recovers from the immediate effects of the accident, walks home perhaps, and after some time becomes stupid, and at last comatose. The surgeon trephines the skull, and discovers blood upon the dura mater; and the blood being removed, the coma goes off. We cannot help *our* patients by a similar expedient; though the opinion has been broached that trepanning the skull will, at some future period, be a common practice in apoplexy. Dr. Abercrombie conjectures that after the rupture has taken place, the hæmorrhage is sometimes stopped by the formation of a clot at the orifice in the vessel, but at length the blood bursts out again, and proves fatal. He relates two cases in which this probably happened; in one of them an interval of three days, and in the other an interval of a fortnight, elapsed between the first attack, and the supervention of coma. The portions of blood extravasated at the distinct periods may sometimes be distinguished by their appearance—their colour and consistence.

The *third* form of attack is characterized by sudden loss of power on one side of the body, and frequently by loss of speech, without loss of consciousness; or at most with a very temporary suspension of consciousness. The patient is sensible, listens to and comprehends your questions, and answers them as well as he is able, either by words, which in most cases he articulates imperfectly, or by gestures. The farther progress of the cases that commence in this way is marked by considerable variety. Sometimes the hemiplegia passes gradually in a short time into apoplexy. Sometimes the patient soon gets well, the palsy leaving him entirely. Or a gradual recovery takes place, which is not complete for some weeks or months. Or the patient recovers up to a certain point, and there the improvement stops; he regains the power of moving his leg, but it drags somewhat after him; or the leg recovers, but the arm remains feeble, or his speech continues to be inarticulate. And in another variety of this form the patient neither recovers on the one hand, nor becomes apoplectic on the other, but is confined to his bed, paralytic, and perhaps speechless, but in possession of his faculties in other respects, and dies at last worn out and exhausted, some weeks, or months it may be, after the attack. In the outset of these cases there is not always complete hemiplegia, sometimes the arm only is affected, sometimes (but much more rarely) the leg

only. Or some other voluntary muscles are the first to lose their power.

Now the appearances on dissection after death, in cases that have thus commenced (Dr. Abercrombie calls them the class of *paralytic* cases) are, as in the *apoplectic* cases, inconstant. Much the most common of all—according to my own experience—is the extravasation of blood, to a moderate or small amount, and definite extent, in the substance of the brain. But sometimes nothing is found upon dissection to account for the symptoms, or slight serous effusion only. The same symptoms attend some cases of softening of the brain also; or inflammation and its consequences. In a vast majority of cases, I repeat, this sudden hemiplegia marks an attack of cerebral hæmorrhage.

You will not find that all cases of apoplexy commence exactly in the one or the other of the three ways which I have been describing. But most attacks range themselves in one of these classes, and by attending to the points of distinction, I make no doubt that you will often derive much assistance from them in regard to diagnosis and prognosis; and that the distinctions themselves will give a higher interest to your study of this complaint, than it would possess if all the forms of attack were lumped together in one common description.

Predisposition.—In treating of this large subject, this multiform disease, the main points will best be made intelligible by my breaking what I have to say into separate heads. I have told you the different ways in which the disease may make its assault. I will next say something of the persons who are most liable, *cæteris paribus*, to attacks of apoplexy: and afterwards of the symptoms which in many cases, though not in all, precede the seizure, and lead us to fear that it may be impending. It is of great importance to attend to these threatenings; for, as you will readily conceive, the chief good that medicine can do in such cases, is in the way of prevention. After the attack has taken place, the effect of our treatment must be very uncertain. A large effusion of blood upon the brain will be fatal in spite of us: and a smaller amount of extravasation we cannot remove; and the best that the patient can expect in too many cases, is long continued or permanent palsy, a weakening of the mental powers, and sometimes a state nearly approaching to idiotcy. Also, when once an apoplectic fit has happened, it is the more likely, on that very account, to happen again. These are quite sufficient reasons why we should not neglect the *warnings*; the symptoms which are apt to precede and herald the attack of apoplexy.

The classes of persons in whom, *cæteris paribus*, attacks of apoplexy are especially to

be apprehended, are those whose *ancestors* have suffered the same disease; those who possess a *particular conformation of body*; and, above all, those who have reached a *certain period of life*. No doubt apoplexy may and does occur in persons whose progenitors have escaped it; in persons of every conceivable conformation; and in persons of all ages. But it is *much more* frequent in the classes I have specified, than it is among persons not comprehended in those classes.

The first and second class sometimes concur, *i. e.* a particular conformation of the body is transmitted from parent to child, and with it is transmitted a proclivity to apoplectic disease. But even when there is nothing peculiar in their bodily form, or in their habits of life, practical men of large experience declare that they who come of an apoplectic stock are themselves more than ordinarily liable to apoplexy.

The pattern of body which is most prone to apoplexy is denoted by a large head and red face, shortness and thickness of the neck, and a short, stout, squat build. This remark is as old as the time of Hippocrates. However, apoplexy is common enough in men and women who are thin, and pale, and tall. *Cæteris paribus*, corpulent people are more in danger of apoplexy than spare people; but it attacks both the one and the other.

Advanced life is certainly a very strong predisposing cause. And the reason of this will be apparent when we come to enquire more particularly into the morbid appearances presented after death by apoplexy. The disease begins to be *common* after 50: but it does sometimes occur even in young children. I am speaking principally of that form of apoplexy which depends upon cerebral hæmorrhage, which is by far the most frequent of all its forms.

Of sixty-three examples of cerebral hæmorrhage collected, and carefully examined by Rochoux, (who has written a very good treatise on this affection), two only happened between the ages of 20 and 30; eight between 30 and 40; seven between 40 and 50; ten between 50 and 60; twenty-three (or more than one-third of the whole) between 60 and 70; twelve between 70 and 80; and one between 80 and 90. To analyze this table a little farther: it appears that of the 63 cases seventeen only took place before 50; forty-six after that age. There are also twice as many victims to the disease between the ages of 60 and 70, as between 70 and 80. And from this fact Rochoux has drawn, I conceive, an erroneous conclusion; and I mention it that, in case his treatise falls in your way, you may not be led by it into what I imagine would be a mistake. There being 23 cases between the ages of 60 and 70, and only 12 between the ages of 70 and 80, Rochoux infers from this

that the disposition to cerebral hæmorrhage *decreases* after the age of 70: which would be a most unaccountable thing, and quite inconsistent with what I believe to be the true pathology of the disease. But I make no doubt that the difference in the actual numbers observed in these two decennial periods depended upon the number of persons alive, at the same time, of the ages of 60 and 70 respectively. There are always more persons living whose age ranges from 60 to 70, than from 70 to 80; and therefore more persons die of apoplexy in the former period. In all probability, if the exact truth could be ascertained, of a *given number* of persons, there are *more* attacked with apoplexy between 70 and 80 than between 60 and 70.

All these three kinds of predisposition are beyond our power. We cannot eradicate the hereditary tendency; nor remodel the plan upon which the body is constructed; nor arrest, or put back, the clockwork of human life: but we may guard and caution persons, thus predisposed by nature towards apoplexy, against many of its exciting causes.

A strong predisposition to apoplexy is, moreover, engendered by certain other diseased conditions; and over some of these conditions our art enables us to exercise more or less control.

Disease of the cerebral blood-vessels is a very common and a very pregnant circumstance of predisposition. I shall revert to this when I describe more particularly the anatomical characters of cerebral hæmorrhage.

Diseases of the chest influence very materially and injuriously the circulation in the head. Without going into detail respecting complaints with which I am obliged to suppose that you are as yet unacquainted, I may state, by anticipation, that impediments to the free transmission of blood through the heart and lungs constitute the mode in which thoracic disorders predispose to apoplexy. The *plethora capitis* produced by such impediments is frequently visible in the turgid and livid features, and in the distended jugular veins.

A very remarkable disease of the kidneys, of which also I must postpone any particular account, renders the system unusually liable to apoplectic affections. I mentioned this renal disease when speaking of the general pathology of dropsy. It is connected, on the one hand, with a morbid state of the urine, which contains albumen, and is deficient in urea; and, on the other, with a morbid state of the blood, which circulates unpurified from the latter excrementitious substance.

The cessation of habitual discharges, of the catamenia, of bleeding piles: he drying up

of old sores; the healing of long established issues and setons; all have an unquestionable tendency, by causing or augmenting plethora, to generate a predisposition to apoplexy.

And large observation of the habits of those who fall victims to this terrible malady, leaves no room for doubting that intemperance often paves the way for its invasion. The continued abuse of ardent spirits, in particular, lays the foundation of many of those morbid conditions of the sanguiferous system, and of the viscera, which constitute the predisposition we are now considering.

Precursory warnings.—Among the *premonitory* symptoms, *headache* is of frequent occurrence: but the same symptom is abundantly common in persons who are in no danger of apoplexy: it derives its minatory character from the concurrent circumstances. Headaches awaken our fears when they *begin* to be troublesome in advanced life. They are, then, still more formidable if they are accompanied by vertigo; or, without any other evidence of gastric derangement, by nausea and retching. Sometimes, as I just now told you, severe headache ushers in, and almost forms a part of, the apoplectic attack.

Vertigo itself, even without headache, is a very common precursor or warning of an approaching seizure. It is sometimes slight and transient; sometimes almost habitual. Although vertigo may depend upon other causes than mischief within the head, we cannot regard it without apprehension when it often occurs in old persons. It should teach us to obviate as entirely as we can all the known exciting causes of apoplexy. The principal of these I shall by-and-by describe to you.

Transient deafness, or transient blindness, blindness or deafness for a few seconds or minutes, is another of these warning symptoms. The late Dr. Gregory, of Edinburgh, used always to mention in his lectures the case of Dr. Adam Ferguson, the celebrated historian, as affording one of the strongest illustrations he ever met with of the benefit that may be derived from timely attention to the avoidance of those circumstances which tend to produce plethora and apoplexy. It is, perhaps, the most striking case of the kind on record. Dr. Ferguson experienced several attacks of temporary blindness some time before he had a stroke of palsy; and he did not take these hints so readily as he should have done. He observed that while he was delivering a lecture, his class, and the papers before him, would disappear, vanish from his sight, and reappear again in a few seconds. He was a man of full habit; at one time corpulent and very ruddy, and, though by no means intemperate, he lived fully. I say he did not attend to these admonitions; and at length, in the sixtieth year of his life, he suffered a decided shock

of paralysis. He recovered, however, and from that period, under the advice of his friend, Dr. Black, became a strict Pythagorean in his diet, eating nothing but vegetables, and drinking only water or milk. He got rid of every paralytic symptom, became even robust and muscular for a man of his time of life, and died in full possession of his mental faculties at the advanced age of ninety-three; upwards of thirty years after his first attack. Sir Walter Scott describes him as having been, “long after his eightieth year, one of the most striking old men it was possible to look at. His firm step and ruddy cheek contrasted agreeably and unexpectedly with his silver locks; and the dress which he usually wore, much resembling that of the Flemish peasant, gave an air of peculiarity to his whole figure. In his conversation, the mixture of original thinking with high moral feeling and extensive learning, his love of country, contempt of luxury, and especially the strong subjection of his passions and feelings to the dominion of his reason, made him, perhaps, the most striking example of the Stoic philosopher which could be seen in modern days.”

This anecdote, which I have made use of as a wrapper for some medical instruction, will not be the less acceptable to you when I add that the remarkable man to whom it relates was the great uncle of my friend and present colleague in this school, Dr. Robert Ferguson.

Very frequently slight and partial paralysis is the forerunner of an attack of apoplexy. Double vision is one form in which such limited palsy is apt to shew itself. It is evidently connected with some degree of squinting; *i. e.* some one or more of the muscles that move the eyeball are paralyzed; the person cannot direct each eye to the same object at the same time. This is a very suspicious symptom. Dr. Gregory was acquainted with a sportsman who one day, when out shooting, disputed with his gamekeeper as to the number of dogs they had in the field. He asked how he came to bring so many as eight dogs with him. The servant assured him there were but four; and then the gentleman became at once aware of his situation; mounted his horse, and rode home. He had not been long in the house when he was attacked with apoplexy, and died.

Sometimes the slight and local paralysis shews itself in a faltering or inarticulate mode of speaking. The rapidity of the movements of the tongue requisite for distinct utterance is so great, that the slightest weakness of any one of its muscles is rendered obvious. We see this in one very common form of what may in truth be considered a kind of apoplexy; *viz.* in drunkenness. In many persons the very first symp-

tom of their becoming intoxicated is their inability to speak plainly. "Clipping the King's English" is the slang expression for it; and the same thing often takes place in respect to the more proper forms of apoplexy.

It is a curious circumstance, by the way, and one which is analogous to what we meet with in disease, that different sets of muscles are chiefly affected by intoxication in different persons; the same sets being always the first affected in the same persons. Thus some persons who are drunk lose (as I have just stated) the proper command over the muscles of the tongue, and falter in speech, while they can walk very well: others reel and stagger, having lost, in a greater or less degree, the power of moving and governing their limbs, and of balancing themselves, who yet can speak quite fluently and distinctly; and in a few cases, drunken persons become delirious, who still retain the power of distinct articulation, and of directing their steps aright. This being so, we need the less wonder at the variety in the nature of the warnings that precede the apoplectic attack.

In many cases there is numbness or debility, or total palsy of one limb, or of a single finger, or even of a solitary muscle, as of the levator palpebræ. The patient cannot grasp your hand with firmness, or sign his name in his usual way, or pick up a pin, or snuff a candle, or manage an obstinate button, or tie a knot in a thread cleverly: or, perhaps, one of his eyelids droops, and the eye is half closed. Sometimes, on the contrary, the patient stares at you, frightfully, with one eye, which he cannot shut.

The numbness also assumes various characters, according to its place and degree. One patient will tell you that he feels as if one limb was always muffled in flannel; another, that he is uncertain whether, in walking, his foot has reached the ground or not. One gentleman, since dead of apoplexy, assured me that, when seated, he did not know how far his breech extended beyond the edge of the chair.

All these symptoms are modifications of the function of voluntary motion; or of the function of sensation. Nor are manifestations wanting, among these precursory circumstances, of a derangement of the other and nobler function, of which the brain and nervous system form the material instrument. I mean the function of *thought*.

Thus one very deplorable warning is the loss of *memory*. All persons, I believe, as they grow older, find that they do not retain so tenaciously in their recollection things which have recently occurred, as things which happened when they were young. This depends upon the degree of *attention* which we pay to different circumstances. Those

events which strongly excite the curiosity, and rivet the attention of the boy, become familiar to the man, and he gives them but little notice, and is very apt to forget them. But the loss of memory that threatens apoplexy is something more than this. It is sometimes partial, and extends to certain sets of things only. For example, some persons entirely forget certain words, while they recollect others perfectly. Common words are often thus forgotten, while unusual or remarkable words are remembered: or a wrong word is chosen. One word is used for another that sounds something like it. Thus one of my patients, meaning to accuse a certain individual of *perjury*, always called it *purging*; and many other words he changed after the same fashion. But in truth the modifications of a partial loss of memory that have been known to precede apoplexy are both odd and endless: some people forget their own names, or the names of their children. Dr. Gregory, who had paid particular attention to these precursory symptoms, and who had a large practice for a great number of years to furnish them, used to mention a case of this kind. After some efforts his patient could recal to his recollection what his christian name was, but he could not think of his surname. About twelve months after his memory began to fail in this strange manner he was found dead in his bed. Another gentleman for some time before his death could never recollect the name of the street in which he lived. Upon one occasion of his visiting Edinburgh, he called on Dr. Gregory, and partook of a hearty breakfast, having forgotten that he had breakfasted before he came out. On the same day he attended, with Dr. Gregory, the funeral of a young lady who had been his ward; the funeral took place in the country; and when they returned together in the carriage, the doctor found that his friend had forgotten all that he had been doing. Next day he met him in the street, and saluted him with all the kindness of an old acquaintance at first meeting; saying he was happy to have fallen in with him now that he was in town, and totally forgetful of their former interviews.

Connected with this failure of memory, there is often an unnatural degree of drowsiness. Sometimes, without any other affection of the memory, there is a temporary confusion or suspension of thought: the patient suddenly loses the train of ideas in which his mind had been occupied; stops short in the middle of a sentence, and endeavours in vain to recover the broken thread of his discourse.

Among the mental conditions that bespeak a tendency to apoplectic disease, I have several times noticed a strange and vague *dread*, of which the person can give no reasonable ex-

planation; a sense of apprehension and insecurity not accounted for by the apparent state of his general powers and functions; a painful degree of indecision and irritability; with a dislike and fear of being left alone. One patient of mine described his "nervousness" of this kind, by telling me that in descending a staircase, especially a winding one, he was obliged to turn round, and come down backwards, as one descends a ladder; or even to sit down, and so slip, stair by stair, from the top to the bottom. Yet with the assurance given him by a friend's arm, or by a convenient bannister, he could walk down stairs without difficulty. He had no actual vertigo.

I say, all these, and many other signs that indicate a disposition to apoplexy, are well worth your study; because a knowledge of them may enable you to ward off the threatened attack, by medicine, by regimen, and by admonition to the patient on the subject of such exciting causes of the disease as are within his own control. They show that, even before the stroke descends, there is some morbid process going on within the head.

LECTURES

ON THE

FUNCTIONS OF THE NERVOUS SYSTEM.

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LECTURE IV.

Nervous System of Vertebrata.

WHEN we direct our attention to the nervous system of the Vertebrated classes, we are immediately struck by two remarkable differences which its condition presents, from that under which we have seen it to exist in the Invertebrata. In the latter it has seemed but a mere appendage to the rest of the organism,—a mechanism super-added for the purpose of bringing its various parts into more advantageous relation. On the other hand, in the Vertebrata the whole structure appears subservient to it, and designed but to carry its purposes into operation. This is more evident in the higher classes of this group than in the lower; these last bordering, as we shall presently see, upon the classes below. But this gradual weakening of the typical character is common to all natural groups, and indeed marks them to be such; since no group can be regarded as complete, which does not contain *aberrant* forms, by which it is connected with the groups in nearest proximity with it. Between the simple nervous system of the lowest Fishes, and the

highly developed apparatus which is peculiar to Man, we find a most gradual series of transitions, in which no link is wanting.

The second of the conditions above alluded to, is one which gives additional force to the remark first made. In the Invertebrata, we do not find any special adaptation of the organs of support for the protection of the nervous system. It is either inclosed, with the other soft parts of the body, in one general hard tegumentary envelope, as in the Echinodermata and Articulata; or it receives a still more imperfect protection, as in the Mollusca. In the latter, the naked species are destitute of any means of passive resistance, and the nervous system shares the general exposed condition of the whole body; and it is not a little remarkable that, in the testaceous kinds, the portion of the body containing the nervous centres should be protruded beyond the shell, whilst the principal viscera are retained within it. Now, in the Vertebrata, we find a special and complex bony apparatus, adapted in the most perfect manner for the protection of the nervous system; and it is, in fact, the possession of a jointed spinal column which is characteristic of the group. Here, therefore, we exchange the *external* skeleton, formed by the condensation of the integument—the *dermo-skeleton*—for the internal structure composed of true bone, and thus a part of the living tissues, which may be termed the *neuro-skeleton*. The cranium itself may be regarded as only an assemblage of vertebræ, possessing the same constituent parts as those of the back, but having those parts developed in a different proportion. This idea, which, when first propounded, was received with great ridicule, is now entertained by all philosophic anatomists; and it is necessary to keep it in view, when we speak of the arrangement of nerves proceeding from the shell. It is not in the highly developed cranium of Man that we must look for its verification; but in the lower forms of that structure, such as those presented by Fishes and Reptiles.

Although it may be stated as a general proposition that no *neuro-skeleton* exists in the Invertebrata, and that a jointed vertebral column is characteristic of the Vertebrated classes, some exceptions must be admitted, which occur on each side, and go to establish the connection between these divisions of the animal kingdom. Thus, in some Crustacea, we find the nervous cord, which runs along the ventral* aspect of the body, enclosed in a complete ring, formed by two projections from the external shell;

* In all the Articulata, the *ventral* aspect of the body corresponds with the dorsal surface in higher Animals. This is proved by the position of the umbilicus, and the general reversed position of the viscera.

these pass inwards, and meet on the median line. Even where the ring is not complete, these two projections frequently exist, and are evidently the rudiments of it. Again, in the Cephalopoda, a cartilaginous envelope surrounds the nervous centres; and this is but little inferior in organization to the simplest form which the neuro-skeleton presents in the Cyclostome Fishes. Thus, in the Myxine, there is no jointed vertebral column, but only a continuous tube of soft cartilage, exhibiting scarcely a trace of division into rings, on the outside of which the spinal cord runs. This tube contains a gelatinous substance, resembling that which is included in the intervertebral bodies in higher Fishes; so that it may be regarded in the light of a continuous intervertebral substance. In the Lamprey, there is a more distinct division into rings, but still the canal is continuous through all; and it is not until we advance much higher, that the division becomes complete between the solid bodies of the vertebræ, and the capsules of intervertebral substance which separate them.

When we look more particularly at the nervous centres themselves, we perceive that they combine the general characters of those of the Articulata with those of the Mollusca. In the former, the power of active locomotion seems the chief object to be attained; and the predominant part of the apparatus is evidently the series of ganglia connected with the locomotive organs. The sensory ganglia appear subservient to these, both in size and function. On the other hand, in the Mollusca the sensory ganglia predominate; and under their function, which is to direct these walking stomachs to their food, the control of the locomotive apparatus seems to be placed. Now, in the Vertebrata we have the locomotive powers of the Articulata (reduced, however, in activity), united with the complex nutritive system of the Mollusca; and we find this combination manifested, not only in the organs themselves, but in the nervous system which stands in so close a relation with them. The spinal cord of Vertebrata is evidently the analogue of the ventral columns of Articulata. It is a continuous ganglion, containing two portions as distinct as the two tracts in the Articulata;—a fibrous structure, which is continuous between the brain and the spinal nerves, and thus resembles the white tract in Insects;—and a ganglionic portion, principally composed of grey matter, or of a substance equivalent to it in character. In this grey matter, as in the ventral ganglia of Insects, a part of the roots of the spinal nerves are lost; whilst others may be traced to the brain. At the upper extremity of the spinal cord (commonly termed the *medulla oblongata*), we find the ganglia and nerves of special sensation; and the organs

which these supply are placed in immediate proximity with the entrance to the alimentary canal, and hold the same relation to it as in the Mollusca. But in addition to these, we find two ganglionic masses in all Vertebrata, to which we have no distinct analogue in the lower classes—the cerebral ganglia, and the cerebellum. With the development of the former of these, the perfection of the reasoning powers appears to hold a close relation; that of the latter seems connected with the necessity which exists for the adjustment and combination of the locomotive powers, when the variety of movements performed by the animal is great, and the harmony required among them is more perfect. Upon this latter point, however, much doubt still exists.

The visceral system of nerves now assumes a more distinct form. It does not share the protection of the spinal column; but its ganglia lie for the most part in the general cavity of the trunk. Microscopic examination of the structure of the visceral nerves and ganglia exhibits a considerable difference in their character from that of the cerebro-spinal system; as was stated in the first lecture.

The connections of the cerebro-spinal and sympathetic systems may be best studied in the trunk; since the regularity of the distribution of the spinal nerves prevents the existence of that doubt regarding the nature of the communication, which obscures the relations of the cranial nerves with the sympathetic. The visceral ganglia—namely, the cardiac and semilunar—may probably be regarded as the true centres of the nervous system of organic life; whilst the chain of ganglia, which lie along the spine, are intermediate between these and the cerebro-spinal system. When the filaments connecting these ganglia with the roots of the spinal nerves are closely examined, they are found to contain both kinds of fibres; and they can no more, therefore, be regarded as the *roots* by which the sympathetic system arises from the cerebro-spinal (as they were formerly described), than as the roots by which the latter originates from the former. The white tubular fibres which these filaments contain, are found, upon close examination, to be derived from both roots of the spinal nerves, and not from the posterior only, as some have supposed. The quantity of white fibres in the nerves proceeding from this lateral chain of ganglia is much greater than that contained in the nerves of the solar plexus; and it is confirmatory of the idea just stated—that the visceral ganglia are the true centres of the sympathetic system—to find that the nerves proceeding from them are almost entirely composed of the fibres characteristic of this system. A few grey fibres may be found in almost all the cerebro-

spinal nerves; they are particularly abundant, however, in the first division of the fifth pair. It would seem that only a part of these are derived immediately from the sympathetic nerve; and that the remainder may be traced into the grey matter of the ganglia formed on the posterior roots of the spinal nerves and on the larger root of the fifth pair. As this grey matter consists of nucleated globules, like those which are found in the centres of the sympathetic system, it may be surmised that this series of ganglia also may be regarded as belonging to the sympathetic system, and as having an influence on the grey fibres contained in the cerebro-spinal nerves, like that which the semilunar ganglia possesses over the nerves of the solar plexus. This is, at any rate, a much more probable account of the functions of these ganglia, than any which had been offered before their connection with the system of grey fibres was discovered by Remak. The notion once entertained that these intervertebral ganglia had some connection with sensation, because found on the posterior or sensory roots of the nerves, and that the ganglionic portion of the column of the Articulata must therefore be sensory, has, therefore, no adequate foundation. If anything be wanting to prove the want of real analogy between these parts, it is the fact that small ganglionic enlargements may be detected upon the nervous trunks proceeding from the ganglionic column of the larger Crustacea, at a short distance from their roots. These are evidently the analogues of the invertebral ganglia of the Vertebrata.

The branches proceeding from the semilunar ganglia are distributed upon the abdominal viscera; and those of the cardiac ganglion upon the heart and vessels proceeding from it. The latter seem to accompany the arterial trunks through their whole course, ramifying minutely upon their surface; and it can scarcely be doubted that they exercise an important influence over their functions. What the nature of that influence is, however, will be a subject for future inquiry. It is so evidently connected with the operations of nutrition, secretion, &c. that the designation of "nervous system of organic life," as applied to this system, does not seem objectionable, provided that we do not understand it as denoting the *dependence* of these functions upon it. Even in Vertebrata, however, we do not always find the visceral system distinct from the cerebro-spinal. In the Cyclostome Fishes, the par vagum supplies the intestinal canal along its whole length, as well as the heart; and no appearance of a distinct sympathetic can be discovered. In serpents, again, the lower part of the alimentary canal is supplied from the spinal cord, and the upper part by the par vagum; and though the lateral cords of the

sympathetic may be traced, they are almost destitute of ganglia. Even in the highest Vertebrata, some of the glands, of which the secretion is most directly influenced by the condition of the mind, are supplied with most of their nerves from the cerebro-spinal system; thus, the lachrymal and sublingual glands receive large branches from the fifth pair, and the mammary glands from the intercostal nerves. It may therefore be regarded as not improbable that the organic fibres contained in these nerves, and principally derived from the ganglia at their roots, are the most direct channels through which the processes of nutrition and secretion are influenced by mental emotions; and that the office of the distinct visceral system is rather to bring these functional changes into harmony with each other.

The spinal cord, with its encephalic continuation—the medulla oblongata, may be regarded as constituting the essential part of the nervous system of Vertebrata. Although the cerebral hemispheres in man bear so large a proportion to it in size, that the spinal cord seems but a mere appendage to them, the case is reversed when we look at the other extremity of the scale;—the cerebral hemispheres in fishes being but ganglionic protuberances from the medulla oblongata. Moreover, the fact that animals are capable of living without the brain, whilst they at once die if deprived of the spinal cord, sufficiently demonstrates this. The spinal cord, then, when viewed in relation to the nervous system of the Invertebrata, may be regarded as including their respiratory, stomato-gastric, and pedal ganglia. That these should be associated together can scarcely be considered remarkable. It is obviously convenient that they should all be enclosed in the bony sheath provided for their protection; and their closer relation favours that sympathy of action, which is so important in animals of such complex structure and mutually dependent functions, as the higher Vertebrata. An animal either congenitally or experimentally deprived of its cerebral hemispheres, is very much in the condition of one of the acephalous Mollusca. It can perform those respiratory movements on which the maintenance of its circulation, and consequently its whole organic life depend; it can swallow food brought within its reach, and it can, in some degree, exert its locomotive powers to obtain it; but it is unconscious of the direction in which these can be best employed, and is dependent upon the supplies of food that come within its grasp. The acephalous Mollusca are so organised that they find support from the particles brought in by their respiratory current; but the more highly organised Vertebrata are not capable of so existing, and they must have their food provided for them

by an exertion of the mental powers. So long as an acephalous vertebrate animal is duly supplied with its requisite food, so long may it continue to exist; and thus it is seen that the operations of the brain are rather connected with the *intelligence* than with the blind undesigning *instinct* of the animal.

It is only in the Vertebrata that the difference between the *afferent* and *efferent* fibres of the nerves has been satisfactorily determined. The merit of this discovery is almost entirely due to Sir C. Bell. He was led to it by a chain of reasoning of a highly philosophical character; and though his first experiments on the spinal nerves were not satisfactory, he virtually determined the respective functions of their two roots, by experiments and pathological observations upon the cranial nerves, before any other physiologist came into the field. Subsequently his general views were confirmed by the very decided experiments of Müller; but until very recently some obscurity hung over a portion of the phenomena. It was from the first maintained by Magendie, and has been subsequently asserted by other physiologists, that the anterior and posterior roots of the nerves were both concerned in the reception of sensations and in the production of motions; for that, when the anterior roots were touched, the animal gave signs of pain, at the same time that convulsive movements were excited; and that, on touching the posterior roots, not only the sensibility of the animal seemed to be affected, but muscular motions were excited. These physiologists were not willing, therefore, to admit more than that the anterior roots were especially motor, and the posterior especially sensory. But the recently-attained knowledge of the reflex function of the spinal cord enables the latter portion of these phenomena to be easily explained. The motions excited by irritating the posterior root are entirely dependent upon its connection with the spinal cord, and the integrity of the anterior roots and of the trunks into which they enter; whilst they are not checked by the separation of the posterior roots from the peripheral portion of the trunk. It is evident, therefore, that excitation of the posterior roots does not act immediately upon the muscles through the trunk of the nerve, which they contribute to form; but that it excites a motor impulse in the spinal cord, which is propagated through the anterior roots to the periphery of the system. The converse phenomenon, the apparent sensibility of the anterior roots, has been still more recently explained by the experiments of Dr. Kronenberg, who has satisfactorily proved that it is dependent upon a branch of the posterior root passing into the anterior root at their point of inosculation, and then directing itself towards the centre; for it was found

by Dr. Kronenberg that the sensibility of the anterior roots was not diminished by the separation of them from the spinal cord, whilst it was entirely destroyed by division of the posterior roots; thus proving that the latter constituted the channel through which the impression was conveyed.

On the other hand, the distinctness of the system of nerves in the simply-reflected actions, from those which minister to sensation and volition by their connection with the brain, is by no means so obvious as in the invertebrated classes. When first pointed out by Dr. Marshall Hall, who had grounded his opinion more upon physiological phenomena than upon anatomical facts, the statement did not command general assent; since, while the phenomena were admitted, the inferences drawn from them by him were not regarded as necessary results. When, however, the anatomy of the nervous centres in Vertebrata was more closely inquired into, (by Mr. Grainger, who had been partly anticipated by Bellingeri), it was found to present certain phenomena which might be regarded as supporting Dr. M. Hall's views; and when the inquiry was extended to the Invertebrated classes, the confirmation was found to be still more decisive. In our previous sketch these doctrines have been treated as established; since they were found not only to correspond with the facts disclosed by anatomical research, but to be required by them. We shall now apply them to the nervous apparatus of the Vertebrata.

The spinal cord consists of two lateral halves; these are partially separated, in the higher classes, by the superficial anterior and posterior fissures; and in fishes by an internal canal, which is continuous with the fourth ventricle. This canal is evidently the indication of that complete separation of the two columns which exists in the lower Articulata; and the fourth ventricle, which, in many fishes, remains unclosed (the cerebellum not being sufficiently developed to overlap it), corresponds with the passage between the cords uniting the cephalic ganglia with the first suboesophageal, through which the oesophagus passed in all the Invertebrata. The two lateral halves have little connection with each other in fishes, and the pyramidal bodies at their apex scarcely decussate; but, in ascending towards the higher classes, the communication between the two sides is more intimate, and a larger proportion of the pyramidal fibres crosses to the opposite sides. In all the Vertebrata, the true spinal cord contains grey substance, or something equivalent to it; thus possessing the character of a continuous ganglion. The proportion of the vertebral column which this ganglionic portion occupies is, however, extremely variable; depending principally on the posi-

tion of the chief organs of locomotion. Thus, in the Eel, and other vermiform fishes, it is continued through the whole spinal canal; whilst in the Lophius and Tetraodon, whose body is less prolonged, and more dependent for its movements upon the anterior extremities, the true spinal cord scarcely passes out of the cranium. The quantity of grey matter is nearly uniform in every part of the cord, where there is no great diversity in the functions of the nerves which originate from each portion. In most fishes, for example, the body is propelled through the water more by the lateral action of the flattened trunk, whose surface is extended by the dorsal and caudal fins erected upon prolongations of its vertebræ, than by the movements of its extremities, which serve principally to guide it. Hence we usually find the amount of grey matter varying but little in various parts of the cord. But in the Flying-fish, and others whose pectoral fins are unusually powerful, a distinct ganglionic enlargement of the cord takes place where the nerves are given off. In Serpents, again, the spinal cord is nearly uniform throughout its entire length; whilst in Amphibia it is so during the tadpole condition (as in most fishes), but presents enlargements corresponding to the anterior and posterior extremities, when these are developed, at the same time becoming much shortened as the tail is less important to locomotion, or is altogether atrophied. In Birds, the ganglionic enlargements are generally very perceptible, and bear a close relation in size with the development of the locomotive organs with which they are connected. Thus, in birds of active flight, and short powerless legs, the anterior enlargement is the principal; but in those which are more adapted to run on land than to wing their way through the air, such as the whole tribe of struthious birds, the size of the posterior enlargement is very remarkable. Hence we have a right to infer that the increase in the quantity of grey matter in the cord has some connection with the amount of power to be supplied; and this exactly corresponds with what has been observed in the articulated classes, and especially in watching the metamorphosis of insects. In birds and mammalia, however, the whole amount of the grey matter in the spinal cord does not bear so large a proportion to the bulk of the nerves proceeding from it as in the lower Vertebrata; and the reason of this seems obvious. The actions of the locomotive organs are less and less of a reflex character, and are more directly excited by the will, and consequently by the brain, than in the inferior tribes; and just in proportion, therefore, to the development of the brain, will it become the centre of all the actions performed by the animal, and the spinal cord a mere appendage to it.

Still, in all the Mammalia, and in man, do we find these ganglionic enlargements of the spinal cord; and in man it is the posterior one which is most considerable. In the cord of this class, too, the lateral halves are much more intimately united than in the classes below; for not only is the central canal for the most part absent, but the two crescent-shaped plates of grey matter are united by a transverse lamella, which connects their centres like a commissure.

The cord is traversed not only by the anterior and posterior fissures, but by two furrows on each side, marking out three columns upon it. We have, therefore, on each half of the cord, an anterior, middle or lateral, and posterior column. The points of the crescentic lamellæ of grey matter approach these furrows pretty closely; but elsewhere the grey matter is covered deeply by the fibrous columns. We are speaking here of the spinal cord of the higher Vertebrata, the minute structure of which has been more closely examined than that of fishes. Each spinal nerve arises from two sets of roots. The anterior roots join the spinal cord near the anterior furrow; and the posterior near the posterior furrow. Regarding their intimate connection with the principal divisions of the cord, however, a considerable diversity exists among the statements of anatomists. In the first place, with regard to their connection with the fibrous and cineritious structures, it may be stated that a portion of the posterior set of roots unquestionably loses itself in the grey matter; whilst of another portion the fibres are continuous with those of the white columns of the spinal cord. There is more difficulty, however, in tracing the anterior roots into the grey substance. Still, however, the connection has been made out, although not in the human subject, by Bellingieri and Grainger; and there is reason to believe, therefore, that it is constant, although not readily perceptible. Of the portions of the roots which are continuous with the fibrous columns, it is stated by Sir C. Bell that the anterior fasciculi pass to the anterior columns only, and that the posterior are restricted to the lateral columns. On the other hand, Mr. Grainger and Mr. Swan maintain that both sets are connected with the lateral columns only; the anterior and posterior lateral fissures definitely limiting the two roots. Perhaps both these statements are rather too exclusive. The anterior roots would seem to have a connection with both the anterior and lateral columns; and the posterior cannot be said to be restricted to the lateral column, some of their fibres entering the posterior division of the cord.

As the white or fibrous portion of the spinal cord is continuous with that of the brain, it follows that the roots of the nerves which enter it are in reality thus brought

into connection with the cerebral hemispheres and cerebellum; and the posterior division of these may, therefore, be regarded as conducting to the brain the impressions which there become sensations; whilst the anterior roots convey the motive influence, which has been propagated, by a voluntary impulse, down the tract of the spinal cord with which they are continuous. On the other hand, the termination of one portion of each set of roots in the grey matter of the cord, completes the nervous circle required for the performance of reflex actions; and by this they would seem to take place in Vertebrated animals, just as through one of the ganglia in the Articulata. It follows, then, on this view of the character of the spinal cord, that the continuity of the fibrous tracts is all that is required to convey the influence of the will to the parts below; whilst the completeness of the nervous circle is all that is necessary for the performance of reflex actions excited through it. This is found to be strictly true; the former having been observed in cases of disease, and the latter having been proved by experiment. As far as simple reflex actions are concerned, there is as much segmental independence in Vertebrata as in the Articulata; but these actions seldom have so completely the character of adaptation, and are of a more irregular and convulsive nature. Still, however, there is an essential correspondence between them; and we may, therefore, regard the distinction between the reflex and voluntary movements as the same in each group; the former predominating in Articulata; the latter in Vertebrata. On this view, then, each spinal nerve contains at least four sets of fibres.

1. A sensory bundle passing upwards to the brain.

2. A motor set, conveying the influence of volition downwards from the brain.

3. Excitor or centripetal fibres, terminating in the true spinal cord or ganglion, and conveying impressions to it.

4. A motor or centrifugal set, arising from the same ganglionic centre, and conveying the motor impulse reflected from it to the muscles.

Of these the first and third are united in the posterior or *afferent* roots; the second and fourth in the anterior or *efferent* roots.

It is difficult to trace the course of the fibres within the spinal cord; and some recent experiments by Valentin appear to prove that Sir C. Bell was not correct in his idea that the functions of the columns of the cord are similar to those of the roots which are connected with them. Cases, indeed, are of no unfrequent occurrence, in which a portion of one of the columns has been almost entirely destroyed by injury or disease, without any corresponding loss of

the function attributed to it. Such cases have kept alive, in the minds of many eminent practical men, a considerable distrust of the accuracy of Sir C. Bell's conclusions. We have seen that, in regard to the roots of the nerves, his first statements have been confirmed, and rendered more precise, by subsequent researches; but it is not so in regard to the functions of the anterior and posterior divisions of the spinal cord. Bellingeri was led, by experiments on the spinal cord, to the conclusion, that the anterior roots of the nerves were for the flexion of the various articulations, and the posterior for their extension. He also was wrong, in extending an inference founded on experiments on the cord to the roots of the nerves. The recent experiments of Valentin, whilst they fully confirmed Sir C. Bell's determination of the functions of the roots of the nerves, coincide, to no small degree, with Bellingeri's opinion in regard to the offices of the anterior and posterior divisions of the cord. He obtained reason to believe that, in the frog, neither the superior nor inferior strands of the cord (posterior and anterior columns in man) solely possesses motor functions; but he found that, when the former were irritated, sensations predominated; and when the latter, motions were chiefly excited. He further states that, if the superior strand (*posterior* column) be irritated at the point at which the nerves of either extremity are given off, that extremity is *extended*; and that if the inferior strand (*anterior* column) be irritated, the extremity is *flexed*. At their entrance into the spinal cord, therefore, it would appear that the motor fibres of the extensors pass towards the superior stratum (posterior column in man), whilst those of the flexors are continuous with the inferior stratum (anterior column); their course being more altered, however, when they are examined far from the point of issue. This doctrine was confirmed by experiments on Mammalia; and is borne out (according to Valentin) by pathological phenomena observed in man. According to this eminent physiologist, also, relaxation of the sphincters is analogous to the extended state of the extremities; and he has noticed a manifest relaxation of the sphincter ani in the frog, when the superior part of the spinal cord was irritated, so as to produce extension of the limbs. These statements are entitled to considerable weight, on account of the quarter from which they come; but they are not, perhaps, to be received altogether without hesitation, until confirmed by other physiologists, especially whilst the phenomena of reflex action are still so imperfectly known. For it is quite possible that, whilst stimulation of the anterior part of the cord may directly excite the motions of flexion in preference to those of extension, the movements of extension

produced by stimulating the posterior column may be of a reflex character.

There is no reason to believe that the functions of the spinal cord are essentially different along its whole length. Every where it appears to consist of a ganglionic centre, supplying nerves to its particular segment; and of connecting fibres, by which the nerves proceeding from any one division are brought into relation with distant portions of the organ, and with the large ganglionic masses at its anterior extremity. In this respect, then, it corresponds precisely with the double nervous cord of the *Articulata*; the only prominent difference between the two being, that in the former the ganglionic matter is continuous from one extremity of the organ to the other, whilst in the latter it is interrupted at intervals; and in the *Mollusca* the centres are still further separated from each other. The connection of the spinal cord with the large ganglia contained within the cavity of the cranium is effected by means of processes from its superior extremity, the arrangement of which is somewhat complex. This portion of the cord, which also lies within the cavity of the cranium, has been termed the *medulla oblongata*. It has been supposed to be the peculiar seat of vitality; but the only real foundation of this idea is that it is the great centre of the respiratory actions, on the continuance of which all the other functions are dependent. The brain may be removed from above, and nearly the whole spinal cord from below, without an immediate check being put upon the nervous phenomena of life. In the *medulla oblongata* four different parts may be distinguished on each side; the anterior pyramids, or *corpora pyramidalia*; the olivary bodies, or *corpora olivaria*; the restiform bodies, or *corpora restiformia*, and the posterior pyramids, or *processus a cerebello ad medullam*. These we shall now trace downwards into the spinal cord, and upwards into the encephalon; and the description I shall employ is chiefly that recently given by Dr. John Reid, which appears to me the most satisfactory of the many which have been put forth.

The *anterior pyramids* decussate, as is well known, at their lower extremity; the principal part of the fibres on each side passing over to the other. The decussating fibres pass backwards and downwards, and enter, not the anterior column of the spinal cord, but the middle column, uniting with the fibres from the restiform bodies. The smaller bundle of fibres, which do not decussate, passes downwards along with those from the olivary bodies, to form the anterior column. A small band, however, proceeds backwards to join the cerebellar column: this is the arciform band of Mr. Solly; but was previously described by Rosenthal and Rolando.

The fibres descending from the olivary bodies converge, as those of the pyramids pass backwards from between them, until they meet on the median line, forming the greater part of the anterior column. The greater part of the restiform body descends into the middle column of the spinal cord, but the posterior part of it unites with the posterior pyramids to form the posterior column. Thus the *anterior* column of the spinal cord may be said to pass upwards into the olivary body, and the non-decussating portion of the anterior pyramid,—a small portion also communicating with the cerebellum; of the *middle* column, a part decussates, and passes forwards into the anterior pyramid, whilst another part ascends into the restiform body of the same side; and of the *posterior* column, a part ascends into the restiform body, and the remainder into the posterior pyramids, which do not decussate in man, but which are stated by Valentin to do so in the frog.

On tracing upwards the fibres of the *anterior pyramids*, they are found to enter the pons varolii, through which they pass, traversing the optic thalami, into the crus cerebri; they then diverge from each other, and become intermingled with grey matter, forming the corpora striata. The fibres of the *olivary body* also pass into the pons varolii, and there divide into two bands, of which one proceeds upwards and forwards to join the crus cerebri, whilst the other proceeds upwards and backwards to reach the corpora quadrigemina. Of the *restiform* body, also, a part of the fibres ascend into the crus cerebri, and pass on to the optic thalami, where they diverge and become intermixed with grey matter; whilst the remainder ascend, with those of the posterior pyramids, into the cerebellum.

Thus the anterior column principally terminates in the corpus striatum and corpora quadrigemina; the middle in the thalamus and corpus striatum; and the posterior in the cerebellum, which communicates, however, with the anterior also.

OBSERVATIONS

ON

THE TREATMENT OF ANEURISMS.

To the Editor of the Medical Gazette.

SIR,

SHOULD you deem the following remarks worthy of a place in your journal, you will oblige me by inserting them.—I am, sir,

Your obedient servant,

ROBERT DICKSON, M.D.

47, Finsbury Square,
March 19, 1841.

In the MEDICAL GAZETTE of the 26th of February, it is stated that, "On Saturday, the 20th instant, Mr. Partridge, at King's College Hospital, tied the subclavian artery in the first part of its course, for an aneurism which had existed in the vessel for about twelve months. After the operation pulsation was no longer perceptible in the tumor; and for the first two days the patient, who bore the operation very well, went on as favourably as could be expected. We believe that this is the sixth time that the operation has been performed, and in no case has the individual ultimately recovered*; for we regret to say that Mr. Partridge's patient died on the 24th."

The unfavourable issue of so many operations for aneurism alluded to in the concluding sentence, recalled to my mind some reflections I had indulged in on this subject many years ago.

In the year 1825, a patient in the Edinburgh Infirmary was operated upon by Mr. Allan for an aneurism of the femoral artery, by tying the external iliac. The patient died a few days after the operation, and, on inspection of the body, no inflammation of the neighbouring parts, or any other cause, deemed adequate to account for death, was found. It occurred to me that death was in all probability occasioned by suddenly throwing back upon the heart a large quantity of blood, not only disturbing its own action, but inducing a state of brain similar to what happens in certain cases of apoplexy. Determining to inquire further into this matter, I consulted the works of many writers on surgery, to ascertain if such a cause of death, and the means of obviating it, had been mentioned. I found no one had directly noticed it, but Richerand, in his "*Nosographie Chirurgicale*," had recorded a case (to be quoted in the sequel), which bore very closely upon it. I reflected, that if individuals who had submitted to amputation of any limb, in the course of the operation for which a considerable quantity of blood is lost, yet suffer from plethora, and the diseases consequent upon it, and apoplexy among others; and if even the slow suppres-

sion of many habitual discharges induced congestion of the vascular system, and oppression of the brain, how much more must this be the case with those who are the subjects of operation for aneurism, in which the blood which would have been diffused over perhaps a fourth part of the body, is confined to the other three-fourths.

In most of the operations for aneurism scarcely an ounce of blood is lost, as in Mr. Allan's case, where not a teaspoonful of blood flowed from the wound. This alone must disturb the rest of the system, not to mention the circumstance of a ligature being permanently around a large artery, which, like any other mechanical obstruction, always rouses the heart to increased efforts to overcome it, by which more blood is sent to the brain. A perusal of the fatal cases after operations for aneurism, which may be found in the pages of Cooper's Surgical Dictionary, will convince any one how many of the symptoms which preceded death are referable to this condition of the circulation. The great alteration in the heart's action, and, by consequence, in the state of the brain and nervous system generally, which immediately follows the application of a ligature around a large limb, or of pressure directly over a considerable artery, may be understood from the following facts:—

In Duncan's Medical Commentaries for the year 1795, vol. xix. p. 271, is a communication from Dr. Kellie, respecting an expeditious, simple, and effectual method of stopping the cold stage of an intermittent, and inducing the hot stage. It consisted in applying a tourniquet to the humeral artery in the right arm, and another to the femoral artery in the left leg. In the first case mentioned by Dr. Kellie, that of John Humphries, a second paroxysm of a tertian ague began at eleven o'clock in the morning. "When I saw him he was shaking very violently, and complained of headache and pain of the loins. Before screwing the tourniquets I counted his pulse; it was small and hard, beating just 100 times in 60 seconds. I now, as before, stopped the circulation in both extremities. I kept a watch with a stop and second before me, and found that, in three minutes from the obstruction of the circulation in the extremities, the cold stage entirely

* We believe that our reporter was not quite correct in this part of his statement. — ED. GAZ.

ceased, the headache was easier, and the pain of the loins entirely gone. I kept the tourniquets on ten minutes, and now counted the pulse, which I found soft and full, beating 120 in one minute. He was thirsty, but had no other complaint."

The same effects followed the application of the tourniquets in several other cases of intermittent; and Dr. Kellie resolved to try the effect of applying them to a healthy body; and from experiments on himself his report is—"I have thus determined their effects on the healthy body, and find that they occasion:—

1st. Great increase in the velocity of the circulation, as judged from the pulsation of the heart and arteries.

2d. Increase of heat and flushing of the face.

3d. Anxiety and more frequent respiration.

4th. The tourniquets being kept on more than six minutes, induce a tendency to syncope.

5th. The tourniquets being removed, and the circulation restored, the velocity of the circulation soon falls to its natural standard, and frequently considerably below it."

A part of Dr. Kellie's tabular view may be given:—

"Pulse 70, small, and rather hard, before the application of the tourniquet. After application, 90, full and bounding. The circulation being obstructed in two extremities for four minutes, heat and anxiety: I removed the tourniquets. 84, full and soft, the tourniquets having been removed."

In Mr. Wardrop's *Treatise on Diseases of the Heart*, published in 1837, appendix F, an account is given, by the late Mr. Hyslop, surgeon, of the recovery of a lady from severe syncope, after copious venesection, by the accidental compression of the brachial arteries in both arms, and a proposition by him, laid before the Royal Humane Society, to employ pressure by a tourniquet, or any substitute for one, to the humeral artery, for restoring the powers of life in persons apparently drowned, or in syncope. Mr. Hyslop was quite ignorant of Dr. Kellie's previous discovery, and his application of it was different; still the principle is the same, that of taking advantage of the obstruction by the mechanical compression of any consi-

derable artery, to increase the heart's action. In these cases the effect was beneficial: not so in aneurism, where, for the object of the operation, the ligature must remain, and the obstruction be permanent. Other means must be had recourse to, to obviate the evils resulting from this state of things. The simple expedient which I venture to propose is, to relieve the system of some of the superfluous blood by repeated venesections.

The history of a few cases of aneurism will, I trust, be sufficient to shew the soundness of this recommendation. I will begin with a successful case of operation, that of Mr. Liston's case of ligature of the subclavian artery, reported in the *Edinburgh Medical and Surgical Journal*, vol. xvi. p. 348, operated upon 3d April, 1820:—

"The patient for some time had been confined to the horizontal posture, bled repeatedly, purged, and religiously starved, according to the plan of Valsalva." Of the operation it is not necessary to say any thing, except that the external jugular vein was divided, and the lower end only secured by ligature, the upper being left untied; a circumstance to which, on attentive consideration of the case, it will, I think, be obvious that the man owed his safety. "The constitutional derangement consequent to the operation was slight. The pulse, in any part of the body, never was much above 100; nor did the action of the heart or large vessels seem at all disturbed. On the fourth day I dressed the wound, and found it adhering completely; but a little matter oozed along the ligature, with some small clots of blood. Next morning, betwixt twelve and one o'clock, I was sent for on account of violent hæmorrhage. On my arrival I found him a good deal exhausted, and surrounded with blood, apparently venous. On opening the wound the stream was perceived to issue from the upper orifice of the external jugular vein, which had given way on some slight exertion, after the clots had been removed by the suppuration. No other unfavourable occurrence took place."

So far from considering this hæmorrhage as an "untoward event," I conceive it was a most fortunate occurrence—a salutary discharge, which relieved the brain and system generally from a superfluity of blood, which

would have produced mischief by its retention.

An unsuccessful case, by the same operator, seems to confirm this view. It is the case of John M'Intyre, operated upon for subclavian aneurism; and recorded in the same journal, vol. xxvii. p. 4.

"Every thing went on well for some days. On the fifth evening after the operation, a good deal of excitement took place: the pulse, which had not risen above 90, beat 120, full and strong, and accompanied with an increase of pain in the tumor and arm. Eight ounces of blood were taken from the arm, and an anodyne afterwards exhibited, with great relief. Excepting the occasional exhibition of a laxative, no other remedies were had recourse to. On the morning of the thirteenth day, Dr. Sanders, who was so kind as to attend the patient during my indisposition, was sent for on account of slight oozing from the wound. In the evening a little more blood escaped, *with great relief of the pain and throbbing at the root of the neck, which had been all along very considerable, but which, for some days, had been remarked to be very violent.* The tumor also collapsed a good deal after the escape of the blood. Next morning (the 14th after the operation) a slight oozing of very dark and putrid blood again took place, and in the evening recurred to a considerable extent. It was arrested by compress, &c. but the patient did not again rally.

On reviewing the circumstances of the preceding case, I am perfectly satisfied that the fatal result is to be attributed to the unfavourable alteration in the coats of the arteries, and to the great size which, from its long duration, the tumor had attained."

I am far from denying that these may have had a share in producing the fatal result; but I think it clear that the excess of blood in the system was the main cause. We see vain efforts made to get rid of the superfluity, in each instance with relief, and I have marked with italics a passage indicative of the excited state of the heart and great arteries. Had venesection been practised then, and still more, on the first rising of the pulse, to the extent of thirty or forty ounces of blood, the result might have been different.

I will add a short notice of a case by

Mr. Syme, recorded in the same journal, vol. l. p. 372, of ligature of the common iliac artery. "In the course of the day (of operation) the tumor became smaller and softer. The coldness and discoloration extended above the knee, and the patient complained of inability to keep any thing on his stomach. On the 9th he was much in the same state. On the 12th he died. The peritoneum shewed traces of much inflammation, but not general or very extensive."

In this case the coldness and discoloration of the limb shewed how completely the circulation through it had been obstructed, and the inability of the stomach to retain any thing, shewed that the brain had already begun to suffer compression by the recoil of the blood from the obstructed artery. The limited inflammation of the peritoneum can scarcely be held to have caused death, though it may have aided in bringing about the event. But the case already alluded to by Professor Richerand is the most decisive, and the candour with which it is narrated is as instructive and worthy of imitation as the casualty to which it refers deserves to be avoided. "A man, strong and robust, entered the Hôpital St. Louis, for a popliteal aneurism. After preparing him by some laxative drinks, followed by a purgative, I operated upon him by Hunter's process, and during the operation the patient did not lose an ounce of blood. The patient, a man from forty to forty-five years of age, was muscular, and of full habit, the face red, and I should have caused him to be bled had I not counted on the operation that it would deprive him of a certain quantity of blood. I could have repaired that omission, by causing him to be bled after the operation. *I omitted this precaution, and my negligence, I confess it with pain, was, without doubt, the cause of his death.* In a word, every thing went on according to my wishes, till one evening of a hot and stormy day, for it was the month of July, and the thermometer (Reaumur) marked 24 degrees, the patient experienced a frightful apoplexy (*apoplexie foudroyante*): in a moment the face, the hairy scalp, the skin of the neck, became of a violet red colour; the eyes filled with tears; the mouth foaming; and in spite of six successive bleedings, the patient never recovered his intelligence, nor power of articulation. Next

day he was no more."—*Nosographie Chirurgicale*, p. 220, tome 4ieme, edit. 5ieme, Paris, 1821.

I submit these cases, and the practice recommended, with great deference, to the consideration of hospital and other surgeons, as they alone are competent to decide upon its propriety, or have the opportunity of carrying it into effect, should they approve.

TWO CASES

ILLUSTRATING THE EFFECTS OF CONTRE-COUP ON THE BRAIN.

To the Editor of the Medical Gazette.

SIR,

THE two following cases of injury of the head, which occurred within a short interval of each other, appear deserving of being placed on record, chiefly on account of what the *post-mortem* examinations disclosed. The appearances found on dissection in both cases, show how the same impulse, which, on the principle of *contre coup*, frequently causes fracture of the skull, in severe injuries of the head, to take place at the point opposite to where the blow was received, may produce a fatal bruising of the substance of the brain at the part opposite to the seat of the blow, even when the injury has not been so great as to cause fracture of the bone at the situation referred to; or, as one of the cases shows, when there has been no fracture of the skull at all. Some writers, in treating of *contre-coup*, and showing the application of that principle to "concussion" of the brain,* have adverted to the effects thus produced on distant parts of the brain by blows on the skull, independently of fracture: but I have not, in my reading, met with any cases that illustrate the results of this kind of injury in so striking a manner as those which I now beg to send you.

I am, sir,

Your obedient servant,
ALEXANDER SHAW.

Henrietta Street, Cavendish Square.

CASE I.—A. B. æt. 28. This man was admitted into the Middlesex Hospital, Feb. 8th, in a comatose state,

having two bruised and open wounds of the scalp, and it being reported that he had fallen, on the previous evening, from a considerable height. The wounds, which were near each other, were situated on the right side of the head, over the prominence of the parietal bone, and between that part and the sagittal suture. In one of the wounds the bone was denuded to an extent about the size of a sixpence. The patient could be easily roused from his stupor; and was then fretful. He was frequently sick and vomited. His pupils dilated and contracted in obedience to the light. His breathing was tranquil.

For the first six days the treatment corresponded with what is usually adopted in cases of supposed concussion of the brain: he was first freely purged with calomel and jalap; was bled from the arm and afterwards cupped from the temples; had cold lotions applied to his head; and took calomel and antimony in small repeated doses, to the extent of making his gums sore. During this period he appeared to be progressively amending; he became more sensible, lying for a considerable part of the day awake, as if observing what occurred in the ward; answering questions at times to his friends or the nurse; and getting out of bed to void his urine or use the close-stool. The wounds of the scalp assumed an improved appearance and discharged healthy pus.

On the evening of the 14th, shortly after being so well as to answer a question put to him by the nurse, he was seized with a fit resembling an epileptic attack. For this he was bled from the arm by the house-surgeon; and a full dose of calomel and jalap was administered.

15th.—He has had several more fits. They consist of a spasm and convulsion confined to the right side of the body. Each fit commences by the head being drawn to the right side, the eyes being directed to the same side, and remaining fixed, as long as the fit lasts, in that position; the muscles of the right half of the body now become rigid, and presently the arm is alternately bent and extended, while the patient draws his breath in a succession of short rapid inspirations, accompanied with a crying sound. His stupor is increased; yet the breathing

* See a Clinical Lecture by Sir Charles Bell, *MEDICAL GAZETTE*, vol. xiii.

remains soft and tranquil, and both pupils obey the stimulus of light in an equal manner. The pulse varies in rapidity and force at different times.

16th.—There is not much alteration since yesterday. At first the fits were severe, but occurred only about once in every six hours; now they return frequently, and are of short duration. The calomel and jalap have been repeated, and a large blister has been applied to the nape of his neck.

Evening.—It is now observed that the right side of the body, besides being affected with the spasms above described, is paralysed. Yet he preserves sensation on that side. When pinched on the right side, he manifests distinct signs of pain; but it is by moving the left arm, not the arm that is pinched. His face being flushed, and the skin hot and dry, and the pulse hard, he was bled from the temporal artery to the extent of sixteen ounces.

17th, morning.—He is no better. A consultation having been called, it was resolved to remove with the trephine the portion of bone denuded in the upper part of the wound. This was done; but the dura mater underneath presented no unnatural appearance. The fits continued during the day as frequent and as severe as before. He was ordered a quarter of a grain of tartar emetic in water, every two hours.

18th.—There is no observable change in regard to the convulsions, except that they perhaps succeed each other more frequently than before, being sometimes three in the hour.

19th.—Early this morning he died.

Post-mortem examination.—Upon removing the skull-cap, the dura mater presented a natural appearance, except at the point corresponding with the trephined hole in the skull, where some granulations had risen. When the dura mater was reflected, so as to expose the brain, an effusion of blood was found extending over the lateral part of the left hemisphere: but upon the right hemisphere there was no similar effusion, or any morbid change. The effusion of blood on the left hemisphere formed a thin layer, spread equally over the surface, except at the temple, where the clot was more abundant. On examining this part (which, following an oblique line from above downwards, was to be accounted the point opposite to where the blow was re-

ceived) a considerable portion of the brain was found to be broken down to a soft pulp: and from this part it was obvious the blood effused around, and which extended likewise towards the base of the brain, had proceeded. The particular portions of the brain which had suffered this bruising of their texture to the greatest degree, were the anterior part of the middle lobe, which lies in the sulcus formed by the greater *ala* of the sphenoid bone; and the posterior edge of the anterior lobe, at the most external parts. No fracture could be perceived in any part of the skull.

CASE II.—A strongly built, muscular man, was admitted, at one o'clock P.M., March 21st, into the hospital, being in a state of insensibility, consequent, as it was reported, on a fall from a ladder. It was stated by his friends, that on the previous evening, while standing on a ladder about eleven feet from the ground, he fell backwards and lighted on his head. He immediately became insensible, and has remained in nearly the same condition till the present time. Last night he was visited by a surgeon, who bled him at the arm.

Upon examining the back of his head, a bruised wound of the scalp was found, situated over the tubercle of the occipital bone. The patient, although comatose, could be roused, when stirred and loudly spoken to, so as to open his eyes and put out his tongue. He slightly resisted, with both eyes, having the eyelids opened with the fingers. It was ascertained that the pupils were contracted. He was observed to move his arms and legs. His pulse was soft, and at 65: his breathing was noiseless and without labour. He was ordered a powder consisting of

Jalap, grs. xx., and Calomel, grs. v. To have his head shaved, and a cold lotion applied.

Upon being visited in the evening, his face was found flushed, with drops of perspiration on it; and the pulse was above 100, hard and full. He was ordered

V. S. ad ζ xvi.; and to commence taking Calomel, grs. ii., with Tartar Emetic, gr. $\frac{1}{4}$. every four hours.

22d, 3 o'clock A.M.—I was summoned to the hospital in the night, under an alarm that the patient was dying. It

appeared at the time that the attack had proceeded from mucus accumulated in the air-passages, and the patient had not strength to expectorate. He recovered after the house-surgeon had swept with his finger some thick mucus from the back of his throat. It was subsequently noticed that in his breathing, although it appeared easy, the thorax was remarkably fixed, so that its motion could scarcely be observed; and the attempts that he made to cough were feeble and imperfect. During this day his stupor appeared more profound. He allowed his arms to fall when raised, as if they were paralytic. His legs are never moved. His urine and fæces pass from him involuntarily. The pupils are still contracted. As his head was hot, and his pulse rather full, twelve leeches were ordered to be applied to his temples, and afterwards a bladder of ice to be kept to his head. The calomel and antimony are continued.

23d.—At five this morning he died.

Dissection.—Some ecchymosis was found at the back part of his head, where he fell. Upon removing the skull-cap, the dura mater presented its natural appearance. When this membrane was reflected, a layer of blood was found to be effused over the anterior parts of the right and the left hemispheres of the brain, being contained between the dura mater and arachnoid membrane. This blood was most abundant where the anterior lobes lodge on the orbital plates of the frontal bone. Here the brain was extensively bruised, and converted into a substance consisting of coagulated blood mixed with brain. The anterior lobes being sliced down, the injury was found, especially on the right side, to have reached very deeply; so that a communication existed between the broken-down part and the anterior cornu of the right lateral ventricle; and a considerable quantity of blood, obviously derived from the ruptured vessels of the bruised part, was extravasated into this ventricle, and extended likewise into the left ventricle. At the posterior part, where the blow had been received, no marks of injury to the substance of the brain could be perceived; on the contrary, both cerebrum and cerebellum presented a sound appearance at this part. Nevertheless, a fissure of the skull existed here. This fissure

began below the tubercle of the occipital bone, and was traced on the left side of the perpendicular ridge, to the foramen magnum. Although a careful examination was made, no injury of the skull could be detected in front, where the brain was so extensively destroyed.

FUNCTIONS OF THE IRIS.

To the Editor of the Medical Gazette.

SIR,

IF in the following view of the functions of the iris there is any thing new, and at the same time plausible enough to deserve consideration, you will perhaps have the goodness to submit it to the criticism of such of your readers as may take an interest in the inquiry.

The iris is subject to contraction from two different sources; the one vital, the other animal. The former kind of action is intended to mitigate excess of light, and thereby prevent injury to the retina; an arrangement which would seem indispensable, when we reflect upon the immense difference in the quantity of light impinging upon the eye at different times, from the first break of dawn to the full blaze of a meridian sun. For if the limits of sensibility in the optic nerve may be compared with that of the nervous system in general, it is evident that the retina could not react sensibly upon the lesser quantity of light, without suffering a destructive excitation by the greater. In modern terms this kind of contraction is said to be a result of the reflex function. The other action of the iris is voluntary, and intended not only to adapt the focal distance of the retina to the external distance of its object, but likewise to enable us to judge of that distance by the accompanying *sensation of effort*; the degree of convergence of the eyes and other inferences being auxiliary means to the same end.

It is generally admitted that a varying convexity of the cornea is the means by which correct impressions of objects at different distances are attained. But how it is effected is not satisfactorily explained. Some refer the phenomena to an inherent activity of the cornea, and others to the acti-

vity of the external muscles of the eye.

In the present state of physiology, perhaps no very cogent argument can be advanced, either to support or refute the former opinion; and it appears to the writer that the advocates of the latter are satisfied with a very clumsy mechanism for enlarging or contracting the segment of a sphere, so nicely adjusted as the necessity of the case here requires. On the contrary, a circular muscle attached to the circumference of the cornea, a bow-string apparatus in the interior of the eye, namely the iris, would seem just the kind of mechanism required. For it may be presumed to act equally upon every point of its attachment, and thus, by a kind of hour-glass contraction, elongate the diameter of the eye in the direction required; whilst in its laminated texture, resembling the springs of a coach, we find in the cornea a suitable provision for admitting alterations of convexity. Nor is this a singular instance, as every physiologist knows, of an organ presiding over a double function, the one vital, the other animal. It must, however, be observed, we cannot assign these two functions to the iris, without also recognizing two *points d'appui*—the external and the pupillar edges—and that neither of them is absolutely fixed. In graduating the light to the sensibility of the optic nerve, it is the outer edge that is chiefly fixed, whilst the pupillar edge is moveable, but in making a focal adjustment the interior edge becomes the *point d'appui*, and the outer one is moveable: for here, as in other sphincters which have a tendency to contract *suâ sponte*, we can by a voluntary effort either overcome or augment the contraction. The action of the musc. orbic. palpebr. may serve as an illustration. For in nictation, which may be compared to contraction of the iris through increase of light, (both being effects of the reflex function), the motion produced scarcely extends to the circumjacent parts, whilst a forcible closure of the eye-lids is attended by a depression of the eye-brows and a corrugation of the surrounding parts, evincing a centripetal action of the whole muscle. If the reflex action has a natural disposition to close the sphincters, we may surmise the reason, why, in foetal life, they are imperforate or

adhesive, and why also the pupil is closed in sleep; since it does not imply a relaxation of the muscle, but a predominance of the vital over the animal powers; of a constraint over a periodic energy. During our waking hours the pupil is kept dilated by the desire and wish to see, and the reflex action which modulates the light is in so far antagonistic to the voluntary; but there is another antagonism between the voluntary expansion and contraction of the iris when we accommodate our vision to the distance of the object. If this view be correct it will assist in explaining the phenomenon that, in transfixing our sight from a distant to a near object in the same line, as from a remote object in the landscape to a speck on the window, there is a sensible diminution of the pupil, which would not take place were the iris designed for no other purpose than the exclusion of light, since the eye in this experiment is exposed to the same volume of light in both cases. The motion observed in the iris of the parrot, and of birds in general, and which is described as voluntary, may perhaps admit of a similar explanation. The eyes are placed on each side of the head, so that they cannot both be brought to bear distinctly upon the same object. When no particular object is present to excite especially the animal's attention, both eyes are equally susceptible of impressions, but when the attention is arrested on either side, the power of vision is transferred principally to that side, and withdrawn in the same proportion from the other. It is well known that when we close one of our eyes the pupil of the other is proportionally dilated; the probable reason of which is, that it requires a stimulus twice as great from one organ as from two to effect the same nervous reaction. To the human species, however, it is not natural to see with one eye in preference to the other, nor can a simple effort of the will accomplish it. The enlarged pupil is therefore consequent and involuntary, whilst in the animals spoken of, which can only see attentively with one eye at a time, it may not seem irrational to conclude that it is a voluntary dilatation which confers the preference. Whether any analogy be traceable in the distended eyelids, as depicted in surprise, admiration,

horror, or any emotion implying a powerful effort of attention, I leave to the opinion of the reader.—I am, sir,

Your obedient servant,
ZETA.

MEDICAL GAZETTE.

Friday, April 2, 1841.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”

CICERO.

ON MEDICAL COLLEGES.

WE return to Mr. Green's pamphlet on Medical Reform, the topics introduced into which were too numerous and important to be treated of in a single paper; and the portion we shall now choose for comment will probably come best at the present time, when the subject of improvements in one of our oldest and *model* universities has been so recently under consideration.

One of the positions on which Mr. Green most confidently rests is briefly this:—“That it is only in universities and colleges that we can expect to find the conditions under which an adequate medical education, and a suitable professional training, can be accomplished;” and he therefore trusts that we may yet see all the medical schools of the metropolis connected with colleges, in each of which we might find a school for elementary instruction, a senior department for instruction in those knowledges which are common to all the professions (the literary arts and sciences), and a medical department for the studies properly medical.

In an article recently published we shewed that a university education in the arts and sciences, bearing in any respect the characters, and insuring any reasonable portion of the advantages, of that pursued at (for example) Cambridge or Oxford, cannot be de-

manded of the majority of those who desire to be medical practitioners, for the irresistible reason, that they cannot afford it. But this objection applies only to the general instructional part of a university system. It has no reference to the disciplinal portion of it, nor to the mode of study adopted in colleges; and we cannot but admit that much benefit would accrue from the adoption of a certain degree of college-discipline and mode of tuition in medical schools.

Medical students commonly come to the London schools at the same age as others commence their graduation at a university; but under what different circumstances do the members of each class find themselves placed? We need not imagine, for every one must have known, instances in which two companions and fellow students have parted—the one to graduate at Oxford, or at Cambridge; the other to *walk* some hospital. The first is consigned to the charge of a tutor who is responsible for his intellectual and moral culture; his whole course of conduct and management is laid down for him; he is made amenable to well-planned laws of discipline; he is entered a member of a society whose rules he is compelled to obey, to belong to which is deemed an honour, and to disgrace which is punished with the hundred-fold disgrace of the delinquent; every inducement to diligence and propriety that ambition, interest, self-respect, and social order can suggest, is held out to him; as far as possible his domestic comfort is provided for; and he is placed in a position, of all that can be imagined the most enviable for one with his objects. Of all men under discipline the graduate is the least sensibly, though in reality very strictly, restrained; and of all possible circumstances, those in which he is placed have always appeared to

us the most nearly to perfection adapted for the cultivation of the intellectual and moral character. That in many cases even these circumstances fail to produce their intended effect, is only a proof that they partake of the imperfection common to all things; but, on the other hand, the brilliant results that they have fostered, and which are evidenced in the matchless learning and eminent morality which, issuing from our universities, have spread their influence over all the land, and have so much contributed to place England highest in the scale of civilized nations—these are fully sufficient to prove that the system could not be generally imitated without producing the most desirable consequences.

Now, let us contrast with the circumstances of the university graduate, those of the medical student, who, up to this period of his life, has been placed under the same or very similar circumstances. On his arrival in London he is left, in ordinary cases, entirely to his own guidance; often, the choice even of the school at which he shall study is left to his own discretion; and in the best of cases he is only furnished with letters of introduction to one or more of his future teachers or hospital surgeons, from whom all that he can expect to receive is a certain amount of advice as to what courses of lectures he must enter to, and what general scheme of instruction he must follow. Beyond this he receives no counsel, and is entirely unguided; his choice of residence, of associates, and of mode of life, are left entirely to himself; and he is thus abruptly, and unguarded, placed in circumstances, of which, for one that will lead him to steadiness and application, a hundred will tempt him to idleness, and to the waste of the brief time that is allowed him for acquiring the knowledge that must serve him for his life. The circumstances of the

young medical student in London are in a word, almost as utterly hostile to his progress in professional knowledge, as those of the university graduate are favourable to the culture of the intellect; and it is a matter of surprise to us, not that so many, but that so few of the former, lose all the advantages that they ought to obtain from their attendance at the schools.

Of course all these things can only be said *in general*. In numerous instances, talent, and the love of exercising it, ambition, necessity, a naturally steady disposition—one or all of these, are sufficient to resist the pressure of circumstances, and the medical student, in spite of all the disadvantages of his position, works on, and attains an admirable result of professional knowledge. But if he does this, it is in spite of his circumstances; whereas, if a university graduate of the same disposition and talent does not obtain a similar result, it is in spite of his circumstances that he fails; on himself alone can the blame rest.

If these things be only generally true, it must be evident that advantage would result from the introduction of a certain amount of College system into our medical schools. The mode in which this might be effected it is certainly difficult to suggest; yet we do not think the obstacles to it are insuperable; and indeed, as far as the projection of a scheme goes, we believe they have already been surmounted at Guy's Hospital. It has, we understand, been long in contemplation there to have rooms provided in the precincts of the Hospital for all the medical students, with a common Hall and *Commons*, and to appoint tutors, who should both guide the studies, and exercise a certain control over the conduct, of their students; to form, in short, as far as possible, a medical college on the plan of the Colleges of our

Universities. But we fear that it is in Guy's Hospital alone that the main difficulty, resulting from the opposition of the Hospital Governors, is already, by the energy and good sense of the Treasurer, overcome; in others, a scheme of the same kind would probably, for a time, be thought inadmissible. However, there can be little doubt, if the advantages which a scheme of this kind would offer to the students were fairly set forth, this opposition from the Governors of hospitals would not long continue. The same common-sense which induced their predecessors slowly to open their wards for clinical study, and then slowly to permit the erection of theatres, dissecting-rooms, and laboratories in the hospital precincts, and which has now, at length, induced the hospital authorities, in nearly every instance, to afford to the interests of the medical schools much of their encouragement, and sometimes even a portion of their funds, would doubtless be sufficient, in time, to lead them to admit the propriety of having a regular establishment for those whom they now recognize as scarcely more than permitted visitors.

Supposing this difficulty (which, moreover, would exist only in those schools that are attached to the large hospitals) to be overcome, the following may be mentioned as the main objects to be had in view in the conversion of what are now mere day-schools into colleges. A home should be provided for as many as possible of the students, by providing furnished rooms for them in one or more buildings in the immediate vicinity of the hospital and school; these buildings being kept exclusively for the students and their tutors, of whom one or more should be resident. The first year's students, especially, should be thus provided with rooms, and if the number of

students were greater than could be accommodated, the seniors among them should be those obliged to reside without the College walls. There should be a common hall for dinner, for which each student should pay a certain moderate sum during his residence; breakfast commons should be provided at some reasonable rate, for all residing in the College; and servants should be appointed and paid by the College authorities, to attend each on a certain number of the resident students. In all these and similar things of domestic management, the system now prevalent in Cambridge, or Oxford, (and which, as it is the result of some centuries of experience, is probably more nearly perfect than any new one that can be suggested), might be followed; and we are quite sure that if a College system were adopted, even no further than this, it would be sufficient to change the mode of life of the medical student, which is now most dangerous or most uncomfortable, for one which would afford a high degree of social comfort and security.

But the system should not rest here. It should be the duty of the tutors to have the superintendence not only of the domestic arrangement of the Colleges, but of the studies of the pupils. From no small experience we are convinced that one of the greatest difficulties the student has to contend with is his ignorance of how and what he is to study—a difficulty so great, that the majority even of those best disposed to work utterly lose their first three or four months in trying to find out what they should study, or in a vain attempt to study every thing, or in learning that which they had far better postpone to a later period. And even when one does hit upon the right object of pursuit, it is no easy matter for him, if his mind have not been more than commonly disci-

plined, to determine how he should study, or to decide from time to time how much he knows, and, judging from his past progress, to tell what his further course should be. At present, the only guidance a student can find in these difficulties is the advice of one or more of his teachers; but to any one who is cognizant of medical schools we need not say that this is quite insufficient. Besides, the difficulty is commonly one which, though important, is not felt till it is too late; till, at some examination, the student finds himself deficient in a subject at which he had perhaps worked hard, and with which he had thought himself familiar.

To remedy all this, the only sufficient plan that we can imagine is one that would realize a more personal and individual instruction; one in which each student separately should be under guidance, and in which the mind of each should have that direction given to it for which it is best adapted, or which is most necessary. Nor do we know any system by which this can be carried on except that of tutors, who, not being lecturers, or in addition to their lectureships, should be constantly examining every student of the school, and appointing to each his appropriate work.

ROYAL MEDICAL & CHIRURGICAL SOCIETY.

March 23, 1841.

DR. WILLIAMS in the Chair.

Observations on the Blood Discs and their contents. By JOHN QUEKETT, Student of Human and Comparative Anatomy at the Royal College of Surgeons. [Communicated by Dr. Pereira.]

THE author's observations in the paper whose title has just been given, lead him to the following conclusions:—That each red particle of human blood is a flattened circular disc, consisting of an outer membrane or envelope, with a gelatiniform fluid in its

interior, which, under certain circumstances, is capable of becoming granular, and of escaping from the envelope in the form of small globules, the general number from each disc being about six or seven; and that the discs may present either a bi-convex or bi-concave surface: the latter form being by far the most common.

The author has hitherto failed in making out the existence of a central spot or nucleus, as usually described. He declines stating for the present what he has ascertained the contained globules to be, intending to lay before the society, at some future period, not only this but also an account of the important part played by them in some of the effects of inflammation.

The author describes at considerable length the appearances presented by the granules, both in the act of escaping from the discs and afterwards, pointing out the confirmation furnished by his observations of the correctness of the description given by Leuwenhoeck of the appearance presented by the globules of blood; that distinguished observer having described each disc as composed of six smaller ones. The author's description was illustrated by a series of drawings of the discs and granules, as seen in the microscope.

A case of partial Softening of the right Corpus Striatum and left Lobe of the Cerebellum, with imperfect Paralysis of the left Arm and right Leg. By Dr. BARKER.

THE patient is described as having suddenly lost all sensation, and almost all power of motion, in the left arm and right leg; neither the intellect, nor any of the organs of sense, having been affected before, or being affected after the attack. The only preceding symptom had been muscular weakness.

She appeared to have undergone a fit, which lasted half an hour, about five months before.

Between her admission into St. Thomas's Hospital, Sept. 15, 1840, and her death, she regained the complete power of retaining her evacuations, which had been lessened, though not destroyed by the last attack; and up to the 3d of November her other paralytic symptoms had diminished. On that day a fit occurred, and was repeated on January the 6th, 13th, and 23d. In a few hours after the last fit she died.

Her pulse had been uniformly slow and feeble. Her strength had been supported, and no active treatment adopted.

Serum was found on examination under the arachnoid of the brain; otherwise it was healthy, except in two points, in which there was a softened portion of a medullary substance, namely, in the right corpus striatum, and the left lobe of the cerebellum.

The cause of death in this case the author presumes to have existed in the softened points, which he considers as not of the nature of apoplectic clots, or of ramollissement, and he considers the mode in which this lesion produced death as different in kind from what is usually observed where organic disease after long quiescence becomes the cause of death. He presumes, therefore, that this event was attributable to disease distant from the part primarily affected.

The author in conclusion points out the discrepancies between the phenomena of this case and the general rules laid down in regard to the relation between affected portions of brain and the paralysed parts of the body, and expresses his opinion that these rules at present rest on an imperfect foundation.

No discussion took place of sufficient interest to report.

[The next meeting of the society will be on Tuesday, the 13th instant.]

MR. BATTLE'S LABORATORY.

To Sir Henry Hallford, Bart.

SIR,

IN addressing you on the subject of Pharmaceutical Medicine, I must, in the first instance, beg to be allowed to thank you for the numerous opportunities you have afforded me at the College of Physicians of submitting to the profession this class of medicines, and for the permission you have been kind enough to grant me at this season again to cover the College tables with the best samples which the market and the laboratory can furnish.

The object of my addressing you at the present time is to announce the approach of the season for preparing the narcotics. To this class of medicines I have for many years paid especial attention; and I am now convinced that their efficacy depends, in a very great degree, on the mode of their preparation. To improve this, as far as possible, to ascertain the principles and compounds which are essential to the production of their several effects, to retain the useful and reject the useless, has been my aim; and I believe that the constituents of these medicines are *still* imperfectly understood. Feeling most anxious for the improvement of pharmaceutical medicine, I trust I may, with your sanction, submit to the profession the experience I have gained. I purpose at a future time to communicate through the press such part of it as can be committed to writing: but nothing short of *actual inspection* can convey a clear and satisfactory idea of the process which is adopted. I beg permission, therefore, to ask the attendance of any members of the College and of the

profession generally, who may feel disposed to favour me with a visit at my laboratory, to witness the dissection and preparations of the principal remedial agents. The first, in order of time, will be Conium, on which, should the weather continue favourable, I shall commence operations on Wednesday, the 7th of April. The next article will be Hyoscyamus, and the next Belladonna. The remaining recent plants, as Aconite, Digitalis, &c. will be operated on as they come into season; and the other articles of the Materia Medica, as Aloes, Cinchona, Opium, &c. in alphabetical order. All my previous operations on these and other drugs are recorded at length in the Laboratory Journal, which is at all times open for inspection.

I am, sir,

Your obedient servant,

RICHARD BATTLE.

Laboratory, Cripplegate,
March 29, 1841.

MEDICAL REFORM.

To the Editor of the Medical Gazette.

SIR,

DURING the present time the question of medical reform has excited the attention of the profession from one end of the kingdom to the other; and however secluded the place may be, and however antiquated the notions and customs of the town or neighbourhood in which a member of the profession may pursue his calling, yet in some way or other the cry of reform reaches him. This has been the case with myself; and I have been led to reflect somewhat on the various measures proposed.

The grand principle advocated, if I may judge from certain articles in recent numbers of a contemporary journal, appears to be the amalgamation of all classes and orders of the profession! Now, taking this proposition for granted, let us just consider how far it would be conformable with justice towards a large portion of the profession, as at present constituted; and also how far the public would benefit by such a change.

And first, how far would it be just? Let me be allowed to put forward my own case by way of example. After spending sixteen years at home and abroad (during which period I enjoyed advantages not attainable by all) in the acquirement of medical knowledge, and having, during that time, passed the examinations at Apothecaries' Hall, and the College of Surgeons in London, as also a medical board abroad, and obtained the degree of Doctor in Medicine after three years' residence at the University of Edinburgh, and my license from the College of Physicians in London, I sat myself down to endeavour to reap some remuneration for the large sums expended, and time occupied,

in the acquisition of a knowledge of my profession. The rest of my professional brethren, holding the same rank as myself, have, in general, been similarly circumstanced: a certain proportion, it is true, have perhaps devoted more time to literary attainments; but all have been obliged, by existing regulations, to devote a large proportion of their time to the acquisition of medical knowledge.

Let us now take another case. A young man is apprenticed to an apothecary or surgeon for a period of five years; at the expiration of that term he goes to London, and in less than three years he may obtain his certificate from Apothecaries' Hall, and diploma from the College of Surgeons. Now if he has "worked hard," and has talent, he will usually be found a tolerably well-informed young man, and a few years in subordinate practice will fit him to be a most useful and intelligent general practitioner; but can he have the knowledge of medicine, and its collateral branches, possessed by the person who has devoted perhaps one-third of the period of his existence in its attainment? Again, if instead of having been an attentive and hard-working student, he has employed a "grinder," in order to gain a sufficiency of information to enable him to pass his examinations, what, let me ask, is the amount of knowledge possessed at the end of a year or two by this aspirant to public confidence? He may have got through his examinations with seeming credit, especially if he be possessed of iron nerve and ready impudence; yet ought such an one (and there are many such) to be placed on an equality with the studious and well-educated physician? The same confidence and impudence which have enabled him to pass his examinations will prove his friend throughout his professional career, and he will indeed be the "licensed quack." It is true he may meet with rebuffs, and experience difficulties, which would bow down the spirit of the conscientious man, but the mind of the impudent quack is too callous to be affected by any rebuke either of conscience or his fellow practitioners; and yet, should the amalgamation system be adopted, this individual would be admitted to the same rank and privileges as those enjoyed by men who have passed their best years, and perhaps expended a small patrimony, in attaining rank and honourable distinction in their profession! Now, would this be just? The public generally cannot understand all the intricacies of the subject; they are led to imagine a person who practises as a physician is one who has a certain claim to their confidence from superior information and experience, and they are willing to reward him accordingly; nor ought they to be deceived. It will, perhaps, be answered that there is

to be a certain standard of information demanded and ascertained by rigid examination for all classes of practitioners: and this will lead me, secondly, to consider what benefit will accrue to the public by the proposed change. What, let me ask, are the great mass of persons constituting the medical profession? They belong usually to the better sort of the middling classes, whose means are too limited in general to allow of the time expended, and expense necessarily incurred, in order to pass such an examination as that implied as the standard. I am willing to allow that we should still have a large body of well-educated physicians; but where would be the useful and intelligent general practitioner? If no grades are to be observed, or no distinction held forth as a stimulus, who will toil for years, and expend his substance, to become——what? a general country practitioner! And by the way, do not for an instant imagine I intend to cast a reflection on that most useful, intelligent, and ill-requited class: far from it, I respect and honour them, and believe them, though inobtrusive, to be, as to their merits, one of the most efficient and useful bodies in this or any other country. But their duties are arduous, and they are generally ill paid, and, as I have before remarked, but few, if any, would be willing to expend the capital and time required, in order to take upon themselves, under the amalgamating system, the toil and responsibility of the country practitioner. Too many fields for enterprise are open to countenance an opposite belief, and I need only revert to the comparatively few parents who are now willing to place their children in our noble profession. Who are to take the place of the apothecaries under the proposed system? Perchance the present race of chemists and druggists. To use a quaint saying, "If all are to be captains who are to pull in the boat?" Who would there be to minister to the wants of the extensive class of the better order of poor? If we make the standard of knowledge too high, a great proportion of highly useful members must be lost to the profession, and the public must be the sufferers; on the other hand, if too low, we degrade and injure instead of exalting the profession. Merit combined with talent, like oil, sooner or later, always rises to the surface, and exacts that deference from society which superior intelligence always commands. The general practitioner has always the opportunity of becoming a physician, should his claims to the distinction be founded in truth and a just estimate of his own attainments. This is a country in which no bounds are set to a man's rise: the highest offices in the realm are alike open to the mean and poor as to the proud and rich, should the genius and intellect command so exalted a station.

A summary of the foregoing would lead us to the conclusion that the standard of education, under the system proposed, would be too high for all classes, in consequence of which the important labours of a large body of highly useful persons would be lost to society; or, that it would be placed so near mediocrity that those endowed with higher powers and intellectual attainments would seek distinction elsewhere. In any way a system of amalgamation must prove injurious both to the public and to the best interests of the medical profession: as well place upon an equality the barrister and the country attorney:—the idea is preposterous! If a man has no talent he will never rise. The man who is placed by the public choice in an elevated station above his fellows may be deficient in some points, but be sure to excel in others; and perhaps, though his acquirements are not brilliant, they may be solid, and highly useful to his fellow men. How many are there whose knowledge of medicine in its various branches is of the highest order, and yet who have not the faculty of applying that knowledge. Certain reforms would undoubtedly be beneficial both to the profession and the public; but let them proceed from a legitimate source, and not from the outcry of a few interested individuals, who seek only their own aggrandisement, careless of the honour of the noble profession to which they unfortunately belong, and heedless of the ultimate consequences to society at large.—I am, sir,

Your obedient servant,

A PROVINCIAL PHYSICIAN.

March 26, 1841.

THE COMPOUND CATHETER.

To the Editor of the Medical Gazette.

SIR,

It may terminate the discussion between Dr. Andrew Buchanan and Mr. Foulkes, on the subject of the priority of the invention of the compound catheter, which has been carried on in some late numbers of the *GAZETTE*, to inform them that it belongs to neither. In M. Gerdy's treatise "*Des Bandages*," at page 123, vol. i., the following extract may be found. He is describing a tube to conduct the small instrument to the obstructed part of the urethra, "which tube," he says, "may be filled up by other tubes of smaller diameter," and adds, "*Les canules de remplissage doivent être d'inégal volume, entrer à frottement l'une dans l'autre et dans le conducteur, de manière que l'on puisse y en introduire une ou plusieurs pour remplir et retrecir à volonté son canal.*"

This proceeding of Gerdy's is referred to in Dr. James Arnott's late work on Strictures

as an improvement or alteration of a plan which he had himself recommended, and which has been introduced to the professional public by Ducamp as his own invention. This consists in passing a wide canula as far as the stricture, through which a smaller instrument may be passed, or a number of small bougies may be passed at once, and the endeavour made to carry one after the other through the stricture.

All these plans are essentially the same, and no doubt each may have its advantages under peculiar circumstances. A thick elastic gum canula (or consisting of other flexible material), having a bore only large enough to allow a small catheter to pass, differs in some respects from all the varieties that have been proposed, and would probably, from simplicity and economy, be superior under ordinary circumstances to any of them. It would, from the bluntness of its point, pass down the urethra with ease; from its easy flexibility it would accommodate itself to the natural curve of the canal, and thus convey the instrument intended to penetrate the obstruction to its centre.

I am, sir,

Your obedient servant.

CHIRURGUS.

March 29th, 1841.

ON THE ACTION OF CERTAIN INORGANIC COMPOUNDS

WHEN INTRODUCED DIRECTLY INTO THE
BLOOD.

BY JAMES BLAKE, Esq., M.R.C.S.

THE following is a short abstract of a memoir lately read before the Royal Society:—

The present paper is a continuation of a memoir read at the Académie des Sciences of Paris in 1839, and entitled, "*Effets de diverses substances salines, injectées dans le système circulatoire.*" (See *Achives Générales de Médecine*, Nov. 1839.) After some preliminary remarks, the author gives a list of the various substances of which he noted the effects, when they were severally injected into the venous or arterial systems, arranged according to the nature of these effects. The substances experimented on were the salts of magnesia, zinc, lime, strontia, baryta, lead, silver, soda, potass, and ammonia. The author carefully inquires into the phenomena, apparently arising from the direct contact of each of these substances with the animal tissues, and more particularly into the effects produced on the heart, on the muscular and nervous tissues, and on the pulmonary and systemic capillaries. The general conclusion which the author is led to draw from these researches is, that there exists a close relation between the chemical properties of the

substances experimented upon and their physiological effects; his experiments tending to prove that, when introduced into the blood, substances that are isomorphous exert similar actions on the living tissues. He notices, however, two exceptions to this law; namely, the similarity of the actions exerted on the pulmonary tissue by the salts of lead and silver, although these salts are not isomorphous; and also the action on the nervous tissue of the salts of ammonia being different from that of the salts of potass. But he remarks that the oxide of lead bears a close analogy to the oxide of silver in its relation to organic compounds; and also that the salts of ammonia, in separating themselves from those of potass by their action on the nervous tissue, yet become closely connected in this respect with the poisons derived from organic substances, with which their chemical composition is so strikingly analogous. The general fact, previously announced by the author, in his memoir read to the Academy of Sciences, at Paris, namely, that salts with the same base have analogous actions when introduced directly into the blood, may be considered a corollary of the above law.—(*Athenæum.*)

HISTORY OF THE LAST
ILLNESS OF SIR A. P. COOPER, BART.
AND
EXAMINATION OF THE BODY AFTER DEATH.
(*From Guy's Hospital Reports.*)

FOR many months previous to his last illness, Sir Astley Cooper had occasionally experienced great dyspnœa, upon the slightest exertion: and it had been observed by his friends that the peculiarity of his complexion bespoke some serious impediment to the circulation. It was not, however, till about six weeks before his death that he found difficulty in assuming the recumbent posture; and about that time he began to pass the greater part of his nights in the arm-chair, rather than attempt to lie down. He still continued to see a few patients during the day, both at home and at their own houses. He now became the subject of frequent cough; which was immediately brought on, if he attempted to recline. The gout, of which he had for several years experienced periodical attacks, showed itself imperfectly in the fore-finger of the left hand; and his legs began to swell, owing to the depending position in which they constantly remained.

During all this time he refused medical aid; and it was not till the 22d of January that he consented to see any one, to whom he might state his symptoms. At the time he was first visited, he was sitting in his chair, with his body inclined forward, and

his chin nearly resting on his chest; the pulse accelerated; not the slightest *bruit* nor abnormal sound in the heart, though the beat was extensive, and heard quite to the right side of the chest. The lungs afforded considerable bronchial rattle, but were neither consolidated nor compressed, and filled both cavities of the chest.

Although remedies appeared more than once to produce a temporary remission of his symptoms, and a further attack of gout in one foot seemed to afford some relief to the chest, yet, upon the whole, the disease advanced, and was attended by frightful fits of dyspnœa, during which his face was purple and his mind confused; and it was in one of these paroxysms that he died, on the morning of the 12th of February.

Shortly before his death, Sir Astley Cooper expressed a wish that the appearances which should be presented on the inspection of his body might be recorded in the Guy's Hospital Reports. He had particularly alluded to four points, the investigation of which he thought desirable;—a cured oblique inguinal hernia; a cured umbilical hernia; some suspected indications of phthisis in his youth; and an inability to sleep whilst lying on his left side.

Examination of the body of Sir Astley P. Cooper, Bart., in the 73d year of his age, on February 13th, 1841, at 9 o'clock in the evening, 32 hours after death, by Mr. John Hilton, in the presence of Dr. Chambers, Dr. Bright, Mr. C. A. Key, and Mr. Edward Cock.

The weather was warm and damp: there were slight cadaverous indications, from gravitation towards the posterior part of the corpse: the face and anterior surface of the body exsanguine: there was general and extensive œdema of the lower extremities; but no evidence of serous infiltration in the arms, nor in any other part of the surface of the body.

The head was not examined.

A globular projection, about the size of a large nut, was found at the umbilicus; which receded on pressure, leaving a well-defined rounded aperture in the linea alba, capable of admitting the end of the little finger. This protrusion consisted of a few congregated lobes of fat placed immediately behind the umbilicus, between it and the peritoneum, the free surface of which was corrugated, and presented a puckered appearance, most probably inflammatory, and the result of the artificial curative means which had been employed for a long period during life.*

* Sir Astley Cooper wore a piece of cork adapted to the umbilicus; and maintained in its place by straps of adhesive plaster, during many years, and until his fatal illness.

The anterior, thoracic, and abdominal parietes were covered with a layer of fat, about an inch in thickness, soft, and oleaginous. The muscular tissue exposed during the inspection was pale, soft, and flabby: indeed, the latter expression is applicable to nearly all the tissues. No gaseous or fluid effusion was found in the cavity of the peritoneum: the greater omentum, loaded with adipose matter, was contracted, and did not extend downwards more than two inches from the transverse colon. Some very old membranous adhesions existed between the right angle of the colon and the gall-bladder: cadaveric transudation of the bile from this viscus had slightly tinged the surrounding parts.

The viscera occupied their natural positions; excepting the cœcum, which was completely invested by the peritoneum, and hence less fixed than usual.

The liver healthy in form: some parts of its surface were slightly contracted and uneven; and sections of it presented hepatic venous congestion, approaching what is termed a "nutmeg appearance."

The gall-bladder was small; and contained a moderate quantity of healthy bile, which, upon gentle pressure, passed rapidly into the duodenum.

The spleen was rather larger than natural, its capsule a little opaque, and the interior of the organ very firm; a section presenting a smooth solid surface of a purplish grey colour.

The stomach was large, and distended with gas; the cardiac extremity stained brown by cadaveric transudation, or the action of the gastric fluid upon the blood: its tissues appeared quite healthy.

The small intestines presented nothing abnormal: nor was there any thing remarkable in the large intestines, excepting the dilated condition of the cœcum, the parietes of which were thin; its mucous membrane congested.

The pancreas was healthy.

The kidneys were surrounded by a considerable quantity of adipose tissue, remarkably dense, and very firmly adherent to the fibrous capsule of the gland. Both kidneys were much congested with blood, rather larger than natural, their surfaces mottled, and slightly granular. These morbid conditions were most evident at the lower part of the left kidney; less advanced but more generally diffused, in the right: and on the anterior surface of the latter, near its convex edge, were found two small cysts, containing a straw-coloured fluid.

The supra-renal capsules were healthy.

The urinary bladder was healthy and contracted, and contained about two drachms of whitish turbid urine.

The internal abdominal ring, on the left

side, was rendered distinct by a tubular extension of the peritoneum for about an inch into the inguinal canal.

A depression existed in a corresponding situation on the right side, the bottom of which was firm, irregular, and corrugated; and upon very careful examination, a minute serous canal, not more than a line in breadth when opened, was traced extending from it, along the spermatic cord, into the cavity of the tunica vaginalis, being the remains of a congenital inguinal hernia.*

Upon raising the sternum and cartilages of the ribs, both lungs were brought into view; and retained their expanded condition, overlapping the pericardium, and manifesting no disposition to collapse. No pleuritic adhesions existed on either side of the chest; nor was there any effusion, except into the right pleural cavity, which contained about three ounces of sanguinolent rather turbid serum.

A little recent pleuritis was found on the middle lobe of the right lung, rendering it slightly adherent by plastic effusion to the adjoining limbs to a small extent. Both lungs presented general vesicular emphysema to a very great degree, and their edges were more rounded than natural.

The larynx was not examined.

The lining membrane of the trachea and larger bronchi was smooth, but of a dark purple hue, from congestion in the minute blood-vessels: the same appearances extended throughout the bronchial ramifications, the smaller of which were filled with a very tenacious puriform mucus; and many of them were observed much dilated. Both lungs were extremely congested with dark blood, especially in and near the central portions of their lobes. At the superior and posterior part of the right lung was a small depressed and somewhat contracted surface, about the extent of a sixpence; a section of which exposed a calcareous mass, very uneven upon its surface, and about equal to the size of a small pea: it was placed about three lines distant from the pleura.

When the pericardium was opened, the heart was seen, very large and distended; and about two ounces of rather dark or brown-coloured slightly turbid serum occupied the posterior part of the cavity.

The right auricle and ventricle filled with very dark-coloured imperfectly-coagulated blood. The auriculo-ventricular valves sound. Through one of the pulmonary valves, near its angle of union with an adjoining valve, was a perforation nearly the size of a small goose-quill. A tolerably firm fibrinous coagulum was found in the

* Sir Astley Cooper wore a truss on the right inguinal canal, from the age of 19 to 25.

pulmonary artery and its branches, extending, by minute prolongations, to the fifth divisions; these were made evident, by withdrawing them in a continuous mass with the forceps.

The left auricle and ventricle were occupied by a large quantity of black grumous half-liquid blood. A large portion of the mitral valve opaque, and a little thickened; otherwise healthy. The aortic valves thickened, and rather rigid at their attached margins; whilst the free margins presented a remarkably healthy appearance for their age.

The left ventricle was much dilated; its apex much broader, and more prolonged than natural: the parietes somewhat hypertrophied; and the muscular fibres of the whole organ were pale, flabby, and weak.

The aorta, which was small and narrow, pursued its usual course, but gave off the left vertebral artery between the left common carotid and left subclavian. The entrance to the arteria innominata was contracted, and slightly irregular.

Many small irregular yellowish opaque patches were seen under the lining membrane of the thoracic aorta and the ascending portion of the left subclavian artery. In most of the parts so affected, the internal membrane was much softened, breaking down under slight pressure: at three or four points it was destroyed to a small extent, admitting a thin layer of dark matter, probably altered blood, separating it in a slight degree from the subjacent tissue: this latter state was noticed near the origin of the arteria innominata and the commencement of the descending aorta. The whole length of the abdominal aorta was full of black grumous blood; its parietes thickened; the lining membrane opaque, and raised by the sub-deposition of hard, almost bony matter.

SOCIETY FOR RELIEF OF

WIDOWS AND ORPHANS

OF MEDICAL MEN IN LONDON AND ITS VICINITY.

THE half-yearly General Court of this Society was very numerously attended on Wednesday evening, at the Gray's Inn Coffee House; Sir Charles M. Clarke, Bart. in the Chair. As the private business of the Society was transacted, and the names of the pensioners mentioned, our reporter was requested to withdraw; but we learn from a gentleman present, that His Grace the Duke of Sutherland presented a donation of 20 guineas, and was elected a Vice-Patron; and that His Royal Highness the Duke of Cambridge had graciously consented to preside at the Annual Dinner on the 17th inst. These announcements were most cordially

received, and the respectful thanks of the Society were voted to His Royal Highness and to the noble Duke.

MARYLEBONE INFIRMARY.

Dr. Mayo was, on Friday, the 26th ult., elected one of the physicians to the Marylebone Infirmary.

BOOKS RECEIVED FOR REVIEW.

Dr. William Thomson's Practical Treatise on Diseases of the Liver and Biliary Passages.

Dr. Thackeray on the Sources and Mode of Propagation of the Continued Fevers of Great Britain and Ireland.

Dr. Robert Williams's Elements of Medicine, Vol. II. Morbid Poisons.

Guy's Hospital Reports, No. XII. April, 1841.

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Friday, March 26, 1841.

R. Webb.—J. Ellison.—T. Robertson.—J. Hough.—A. C. Brownless.—J. W. Firminger.—G. Bowring.—R. L. Baker.—S. M. Pelley.—J. Vaughan.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 20th March, 1841.

Small Pox	15
Measles	6
Scarlatina	7
Hooping Cough	51
Croup	7
Thrush	1
Diarrhoea	10
Dysentery	2
Cholera	0
Influenza	27
Typhus	19
Erysipelas	6
Syphilis	1
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	164
Diseases of the Lungs, and other Organs of Respiration	322
Diseases of the Heart and Blood-vessels	18
Diseases of the Stomach, Liver, and other Organs of Digestion	69
Diseases of the Kidneys, &c.....	7
Childbed	13
Ovarian Dropsy	0
Diseases of Uterus, &c.	5
Rheumatism	5
Diseases of Joints, &c.	3
Ulcer	0
Fistula	1
Diseases of Skin, &c	0
Diseases of Uncertain Seat	119
Old Age or Natural Decay.....	81
Deaths by Violence, Privation, or Intemperance	28
Causes not specified	3

Deaths from all Causes 990

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, APRIL 9, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC.

Delivered at King's College, London.

BY DR. WATSON.

LECTURE XXIX.

Apoplexy continued. Symptoms characterizing the apoplectic state. Pressure the ordinary physical cause. Hemiplegia. Affection of involuntary muscles. Anatomical characters. Situation of the clot of blood. Disease of the cerebral blood-vessels.

WE were engaged with the subject of apoplexy. I requested your particular attention to the threefold mode in which that fearful disorder has been observed to make its attack. In the first, the coma is sudden, and deep; the condition of the patient, thus struck in an instant senseless and motionless, warranting those epithets which the ancients applied to the victims of this disease, of *attoniti*, and *siderati*, as if they were thunder-stricken, or planet-stricken. In the second form of the attack, the earliest symptom is acute pain of the head, with sickness and faintness; the coma *supervening* usually in no long time. The third form is ushered in by sudden hemiplegia, which may or may not lead to loss of consciousness, or stupor. The cases which range themselves under the one or the other of these three forms of attack are called respectively by Dr. Abercrombie *apoplectic* cases; cases *not primarily apoplectic*; and *paralytic* cases: and so as you bear in mind what these terms really imply, they appear unobjectionable.

I next pointed out the classes of persons in whom an attack of apoplexy is chiefly to be apprehended: those, namely, in whose families that disease has been known to be

common: those who have large heads, thick necks, red faces, square shoulders, and a short stature; although persons of quite the opposite conformation are by no means exempt from it: and lastly, and above all, those who have passed the middle period of life, and are advancing towards old age: and more particularly is apoplexy to be apprehended in people of this description when they have already suffered what are called head symptoms; which symptoms have reference to the three great functions of the brain and nervous system; voluntary motion, sensation, and thought. Such symptoms consist, therefore, in slight and often transient paralytic affections, double vision, a dropping of one eyelid, occasional inarticulate speech, weakness perhaps of a single finger; headache, giddiness, unnatural sounds in the ears, numbness or tingling of the extremities, which last are all modifications of sensibility; some impairment of the intellect, marked, most commonly, by partial and strange defects of memory, and temporary confusion of thought. When several or any of these symptoms occur, and especially when they become habitual in persons in the decline of life, we have reason to dread the supervention of apoplexy; and to exhort and protect our patients against its ascertained exciting causes.

Symptoms characterizing the apoplectic condition.—When the apoplectic state is *fully* formed, in what manner soever the attack may have commenced, it is marked by most or all of the following circumstances. The patient lies totally unconscious as to all that is going on about him: he replies to no questions, he is unmoved by the cries and lamentations of his family; in fact, he does not hear them: his pulse is infrequent, often full, perhaps intermitting: his breathing is peculiar, being slow, sometimes interrupted or irregular, attended with snoring or stertor during *inspiration*, and a puffing

out of the cheeks, like the action of one who smokes a pipe, during *expiration*. Both these peculiarities are referable to the same principle, and both denote a profound insensibility to all external impressions. There is no longer any voluntary attempt to breathe, yet the involuntary movements of respiration subsist: the medulla oblongata still responds to the impressions which reach it from the lungs, still prompts contraction of the muscles that enlarge the capacity of the thorax; but the loose curtain of the palate, and the lips and cheeks, are passive: by the vibrations of the one the stertor is occasioned; the mouth is closed by the mere elasticity of the others, and the flaccid cheeks flap outwards with the explosion of the air, as it escapes when the chest again collapses. The countenance is frequently turgid, and livid; the blood which tinges it is already but half arterialized; the pupils are commonly contracted. The limbs lie motionless: either they are all absolutely palsied; or (what probably is often the case) the capacity of motion remaining, the *will* to move them is wanting. If you raise one of them it falls passively down again, when you leave hold of it, like a *dead* limb. Sometimes, however, they are rigid and stiff. Sometimes one is stiff, and the others limber. And sometimes one or more of them, or those of one side, tremble, or are distinctly convulsed. You find the patient is unable to swallow; if you put fluids into his mouth, they appear to choke him, or they run out again at the corners of his lips. His bowels are usually torpid; but if they act, the evacuations are passed in the bed without his knowledge or concern: his urine also flows involuntarily; or is retained in the distended bladder until it fairly *overflows*, and dribbles away perpetually.

When the attack terminates in death, that event is preceded, I believe in almost every case, by profuse perspiration, which bursts forth from every part of the surface, and is often cold and clammy. The pupils are sometimes at this period dilated: and I have more than once seen them of unequal size. The pulse becomes more frequent, the breathing more rare, and at last it ceases altogether.

Pressure the ordinary physical cause.—This state, so appalling and painful to look upon, but fortunately so devoid of suffering for the patient—this suspension of the functions of animal life—depends, we have reason to believe, upon *pressure* applied to the brain, the organ subservient to those functions.

That excess of pressure is a *vera causa* is obvious, and that it is adequate to the production of coma is capable of demonstrative proof. It is not enough to shew that they often exist together, for the coincidence

might be casual. Neither does their occasional disjunction, real or apparent, furnish any conclusive argument against the general proposition, that coma, in many and in most cases, is the result of pressure upon the encephalon.

Coma may exist without pressure. In other words, coma acknowledges *other* causes also, *besides* pressure. It is produced by many narcotic poisons; by the circulation of venous blood through the arteries. In these cases we have no proof of any compression of the cerebral substance.

The other disjunctive condition is much more puzzling, and has led some persons to question or deny the general proposition. Can there be unnatural pressure, yet no coma? It would seem so. Serum, pus, blood, have been met with in the brain, foreign matters have penetrated the cranium, and coma has not occurred.

The force of this difficulty is lessened by the consideration that foreign substances may be present within the skull, without occasioning any preternatural degree of pressure. We read of bullets being carried about for some time in the brain. In such instances it is probable that a portion of the contents of the skull was forced out at the time of the injury; or that coma has come on, and gone off again, in consequence of the gradual absorption of the cerebral matter to make room for the foreign body. The same explanation may be applied to the chronic accumulation of water within the cranium, and to the slow growth of tumors.

Farther, it is open to conjecture that it is not on every part of the brain that the same degree of pressure made, will produce the effect ascribed to it. It is stated in Mr. Mayo's Physiology, as the result of actual experiments on animals, that *lateral* pressure against the hemispheres of the brain produces no observable ill consequence; but that vertical pressure, pressure downwards, occasions stupor, which is attributable to the compression of the medulla oblongata. Now it is obvious that some injuries of the brain may tend more than others to cause pressure in that direction.

I confess that the difficulty is not wholly relieved by these considerations. But it is a difficulty which cannot invalidate the evidence of numerous facts that attest the agency of pressure, as, at least, one cause of coma. The presumption of such agency arises whenever coma immediately succeeds to pressure; and it is converted into certainty if, upon the removal of the pressure, the coma immediately departs. Now the annals of physic are full of instances of that kind. In experiments upon animals, stupor has been brought on, and made to cease, at the pleasure of the operator, by applying pressure to the exposed brain, and by remit-

ting that pressure. Nay, the experiment has been tried on the human brain itself. A man who had undergone the operation of trepanning, and had recovered, was in the habit of exhibiting himself for money in Paris, where Haller saw him. He suffered the spectators to make pressure upon his brain, where it was covered by the integuments only. This always put him into a state of coma or deep sleep; but sensibility and the power of voluntary motion returned at once when the pressure was taken off.

A most remarkable example of the occurrence of coma from pressure upon the brain, and of the removal of the coma by removing the pressure, was afforded by a patient who was in St. Thomas's Hospital under the care of Mr. Cline. Mr. Green, who was Mr. Cline's nephew, was in the habit of relating the case in his lectures here. It is quite pertinent to my present purpose. One of Mr. Cline's apprentices was visiting the dépôt at Deptford, and discovered there a man who had been for some time in a state of unconsciousness: and he had him removed to St. Thomas's. His main symptoms were apparent insensibility to all surrounding objects, and a total incapacity to make any communication to those about him; except that his attendants learned to infer, from certain instinctive movements or gestures, that he felt hunger, or thirst, or a want to relieve his bowels. His fingers were permanently bent towards the palm of the hand, and his eyes were turned upwards, so that the corneæ were completely concealed beneath the upper lids.

Upon examining this man's head, Mr. Cline found that there had been fracture with depression of one of the parietal bones. He trepanned that part, and elevated the bone. The patient seemed to feel the operation; and as soon as it was concluded, his eyes and fingers were restored to their natural position. On the evening of the same day, he sat up in bed, and though at first stupid and incoherent, soon became rational and well.

When he had entirely recovered his senses, it was ascertained that the last thing he remembered was his serving on board a vessel which made a capture off Minorca. He was wounded in the engagement, and carried afterwards to the hospital at Gibraltar. All this happened upwards of twelve months before the operation. So that one whole year of this patient's life was a complete blank, because, during that period, a little piece of bone was pressing upon his brain.

Cases of this kind show, very convincingly, the connexion that subsists between pressure on the brain and coma, and their relation to each other as cause and effect. The pressure and the coma begin together; the coma continues as long as the pressure continues; and

it ceases when the pressure is removed. The old definition of the *cause* of a morbid condition is completely satisfied. "*Prasens morbum facit, mutata mutat, sublata tollit.*"

From this digression — not altogether foreign to our subject—I return to the consideration of the pathology of apoplexy.

Hemiplegia.—If the patient recovers from the coma, he may live a few hours, or days, or he may live for many years. Sometimes, as the coma departs, all the natural functions are gradually restored; but much more commonly paralysis remains. You already know that it is apt to affect one moiety of the body only. If a line be drawn from the vertex to the perinæum, dividing the body into two halves, which, as far as the exterior is concerned, are symmetrical, all the *voluntary* muscles that lie on one side will be found powerless; or if they are not *all* so, those which are palsied are situated on the same side of the line. And this state of things is called *hemiplegia*. *Paraplegia*, that condition in which all the parts below a transverse line are palsied, though it sometimes results from cerebral disease, is much more commonly the consequence of mischief in the spine.

Now, of this hemiplegia, when it is complete, there are several particulars worthy of your notice; and there are many things worthy of your notice when it is incomplete. But we will take one of these predicaments at a time. By complete hemiplegia I mean palsy of all the voluntary muscles of one side. The patient may *will* the motion of his leg, or arm, but they no longer obey the act of volition; if they are lifted by another, and then let go, they drop down like logs of wood. You will find that, in well-marked cases, the *intercostal* muscles of the palsied side do not contract. The muscles of the face, also, are some of them inert on the same side. I have known many persons who have thought that the muscles of the *face*, in hemiplegia, when they were affected at all, were affected on the opposite side of the body from that to which the palsied *limbs* belonged. But they never could have examined actual cases of hemiplegia with any attention. How the error arose I cannot tell, but I have known a professed anatomist make it. I guess that it has arisen from one of two causes. An anatomist who had not looked closely upon disease, would expect, and not unnaturally, that the face and limbs would be affected on opposite sides of the body; seeing that the nerves which supply the muscles of the face are given off above the place where those fasciculi of nervous matter which are called the anterior pyramids, decussate each other. And a vulgar observer, who was not an anatomist, would be apt to conclude that the side towards which the mouth was drawn

was the affected side: whereas it is just the reverse. The face is drawn to the healthy side, because the muscles on that side are no longer counteracted and balanced by the corresponding muscles of the palsied side. The *blank* half of the face is that which answers to the paralyzed limbs: on that side the patient cannot frown, or smile. He presents a singular spectacle, which I do not dwell upon now, because I shall return to it again when I have to speak of certain important varieties of *local palsy*. What I wish you to bear in mind at present is, that when the muscles of the face are affected in hemiplegia, the *rule* is, that they are palsied on the same side with the limbs. But there is no rule, they say, without an exception: certainly the exceptions to this rule are very uncommon. I have not had leisure to look over the records of the very many cases of this disease which my position as physician to a hospital has brought under my observation; but I do not recollect more than two exceptions; and one of them, as it happens, is now exhibited in the person of one of my patients in the Middlesex Hospital. Some of you have seen the woman. It is a well-marked exception: but in this instance the hemiplegia followed a blow on the head, and I suspect that a double injury was inflicted; that the palsy of the face results from mischief on one side of the brain, and the palsy of the limbs from mischief on the other. This I only conjecture; because the phenomenon is so rare*.

Then, again, with respect to the *tongue*: when put out beyond the lips, its point is commonly turned to one side. To which side? Why *towards* the palsied side. For what reason? Why because the muscles that protrude the tongue are powerless on that side, and in full vigour on the other. That half of the tongue which corresponds with the sound side is pushed farther out than the other half, and therefore the tongue bends to the other side. Such is the usual fact, and such the explanation of it. But there are more numerous exceptions to this than to the paralysis of the external facial muscles. Sometimes the tongue comes out straight; sometimes the patient cannot protrude it at all; and sometimes, even, it bends towards the sound side. But the *rule* is as I have stated it.

This also has been noticed of the tongue in such cases; that the patient has been able, after some effort, to thrust it suddenly out, and then has required a certain interval of time before he could do so again; as if the spent nervous power was slowly regenerated. With these different affections of the tongue,

the patient's speech is variously altered. His voice is thick, muttering, inarticulate, or unintelligible. Sometimes, even though he may be quite conscious and rational, he is unable to utter a syllable; and seems vexed at finding that his attempts to speak are fruitless.

Supposing the patient to recover, wholly or partially, from the paralysis, it is the *leg*, in nine cases out of ten, aye and in a much larger proportion than that, which recovers first and fastest: sooner and faster than the *arm*, I mean. And another fact, quite analogous to this, is, that when one of the extremities alone is affected with paralysis, it is, in nineteen cases out of twenty, the *arm* that is so affected. I give you again the *rules*; they are liable to occasional exceptions. The reasons that have been assigned in explanation of this curious circumstance I shall lay before you by and by; after I have had an opportunity of describing the morbid appearances met with in the brain in these cases.

This, then, is one way in which the hemiplegia may become, or be from the first, incomplete: viz. in *extent*. One limb may be powerless and the other strong. But the palsy may also be incomplete in *degree*. The patient may be able to move and use his limbs, but they are feeble. He cannot bend his fist firmly; or lift his arm beyond a certain height. Or his leg feels heavy to him, and trails a little behind as he walks: he is unable to stand upon that limb; or to plant his foot securely, or with the usual precision. In short, there are innumerable gradations of paralysis, from slight weakness of the affected muscles to perfect immobility.

Besides the *palsy*, there is often *anæsthesia* also. But this is by no means so constant a symptom as the paralysis. The function of sensation (wherefore I cannot tell) is less frequently abolished or perverted than the function of voluntary motion. When the sensibility is lost, or blunted, or any how modified, it is so, commonly, in the same parts that are affected with paralysis. But sometimes there is anæsthesia, and no palsy; and, more strange still, there has been sometimes anæsthesia of one side, and palsy of the other. As a general rule, the anæsthesia is less common, and less intense than the palsy; and is much sooner recovered from.

The mental faculties are, in some few instances, quite unhurt by the attack: too frequently, however, they suffer irreparable damage. Of many persons, a striking alteration is evident in the whole character and temper. The brave man has become timid; the prudent man foolish; the calm and cheerful man peevish and impatient. There is no longer the same power of attention, the same capacity for business, the same

* This patient died afterwards, at her own home; and no opportunity was given of inspecting the body.

clearness and comprehension of thought. And, whatever other changes may be observable, there are two ways, especially, in which the patient, after he has emerged from the coma, is very apt to be affected: viz. by a defection of memory, more or less partial; and by a peculiar tendency to emotion, especially the emotion of grief: he will weep for very slight causes, sometimes long after the attack of apoplexy has passed over. This is very curious. I should have stated before that the same readiness to shed tears, and to be immoderately affected by trifling causes of emotion, is sometimes noticed among the *precursory* symptoms of apoplexy.

Tracing these cases onwards still farther—such cases, I mean, as do not perfectly recover—we find that the palsied limb wastes: inaction of the muscles, according to the principle which I explained to you in a very early part of these lectures, leads to a lessened nutrition, and a consequent diminution of bulk: in one word, to atrophy. Sometimes, indeed, the size of the helpless limb is maintained, or even augmented, by the supervention of oedema. The motion of the blood in its veins is not aided by the play of its muscles, and the cellular tissue becomes infiltrated with serous liquid.

Again, these palsied limbs are usually colder than their fellows. This probably is owing to the diminished circulation of blood through the capillaries: there is not so much blood converted into venous from arterial; and less animal heat is developed. This has been observed even when the main artery of the part has beat as forcibly as in the corresponding part on the other side.

It is necessary to be aware that these palsied parts do not resist the influence of cold or heat so well as the sound parts. When the sensibility is blunted, we can readily understand how the limb may become burned, from the absence of any warning pain that an injurious degree of heat is applied: but this is not all. A lower degree of temperature than would injure a sound part has often been found prejudicial to a palsied part: and if these palsied parts get chilled by frost, they more readily vesicate and inflame, on the return of heat, than other parts: merely *warm* water will sometimes act upon them like *scalding* water. I say a knowledge of this fact is of practical moment. That degree of warmth which the palsied limb fails to generate for itself, we must accumulate for it by warm clothing; and we must take care that it is never exposed to any artificial temperature which exceeds a certain point. We sometimes see mischief done by applying hot bottles or bricks—*too* hot—to such limbs.

In speaking of the palsy, I have dwelt especially on the loss of action and power observed in the *voluntary* muscles: but the

strictly *involuntary* muscles do not altogether or always escape. The pulse, as I have stated, will often become slow or irregular in the apoplectic attack; and the bowels are usually very obstinately costive;—their peristaltic motion, which results from the contraction of involuntary muscles, is suspended or diminished. Now the old writers on apoplexy puzzled themselves with devising explanations of the fact that the involuntary muscles are so little affected in this disease. Boerhaave, and others, fancied that the voluntary muscles were influenced by affections of the cerebellum; the involuntary by those of the cerebrum, of the brain proper: but this is quite in opposition to well-ascertained facts. Le Gallois entertained the notion that the movements of the heart and alimentary canal are under the dominion, not of the brain, but of the spinal cord; and this doctrine was received and sanctioned by the French Institute: but it has been refuted by the experiments of later observers. We are enabled, I think, to explain, now a days, why the muscles of involuntary motion often are *not* affected in apoplexy. I have before had occasion to shew you that the functions of organic life are not necessarily dependant upon any influence derived from the brain: they might go on, if a due supply of arterial blood were kept up, even though there was no brain at all. Some foetuses have arrived, in other respects, at their full growth, in which there was no brain; nay, in which there was neither brain nor spinal marrow.

In truth it seems to be more difficult to explain why the organs of involuntary motion *are* sometimes affected, in apoplectic and paralytic diseases, than why, in general, they are *not*. But some elucidation of this matter I have also attempted to give in a former part of the course. The organs of involuntary motion are not dependant upon the brain and nervous system; yet they are liable to be influenced through their medium; as we know by the effect produced upon those organs by certain emotions of mind. Dr. Wilson Philip has shewn clearly, by his experiments, that the way to affect the action of the heart, and of the involuntary muscles, through the brain and nervous system, is to act upon a *large portion* of that system at once. Hence any *disease* which inflicts extensive damage upon the encephalon will be likely to disturb and weaken the functions of the heart and alimentary canal.

Conversely, when we find, in a case of apoplexy, the involuntary muscles sensibly affected, we may infer, I believe, that the injury done to the nervous matter is great and serious.

Anatomical characters.—Let us next, with the view of farther explaining the pathology of this disease, direct our attention

to the appearances which are met with after death in the most common forms of the complaint.

I shall pass over those cases in which no morbid condition is detected, simply reminding you that the altered relation of the venous to the arterial circulation in the brain may perhaps account for the symptoms, and for the extinction of life: or the altered velocity of the blood circulating in the brain may account for them: or, what is more probable still, a determination of blood towards the head, or a detention of blood in the head, sufficient, by tightening the full vessels, to occasion extraordinary pressure upon the nervous pulp, may account for them.

I pass over likewise those cases in which serum only is found effused beneath the arachnoid, or into the ventricles. A moderate quantity of serous fluid poured out rapidly during life would certainly occasion a degree of pressure adequate to the production of fatal coma. How the serum comes to be so effused, it is not always easy to say. Yet there is one condition of the blood-vessels of the brain which, when it can be proved to exist in a given case, is sufficient to account for the effusion. Any real or virtual retardation of the blood in the cerebral veins would lead to what is tantamount to dropsy, *there*, as well as in any other part of the body; and the intelligible causes of such retardation are known sometimes to be in operation.

But I wish to consider more particularly the appearances that are met with in the brain after death by *cerebral hæmorrhage*, which, after all, is the most common source of apoplectic and paralytic disorders.

In the first place (as I have more than once stated before) the popular notion that hæmorrhage is owing to the giving way of a considerable blood-vessel—although this notion seldom has reference to the brain, because the blood cannot reach the external surface of the body, and therefore does not strike the popular sense—I say this notion is more true of cerebral than of any other hæmorrhage. Much more true, especially, as regards the brain than as regards the lungs, to which latter organ the bursting of a blood-vessel is, in vulgar parlance, most commonly ascribed.

This comparative frequency of hæmorrhage from the actual rupture of vessels may, in some measure, be accounted for by their peculiarities of texture and relation. The blood-vessels distributed within the cranium are long and slender; excepting the sinuses, the coats of both arteries and veins are thinner and weaker than in other parts of the body: the middle tunic of the arteries has not more than one-half its ordinary thickness; and the outer or cellular coat is

of such extreme tenuity that doubts have been entertained concerning its existence. These vessels, moreover, are not protected, as elsewhere, by investing sheaths of cellular membrane, and receive but slight support from the soft and delicate substance by which they are immediately surrounded. They are likewise very subject to a particular form of disease, by which their natural fragility is much increased, and lying near to the heart, and in the primary direction of the blood as it is driven from the left ventricle, they have been thought especially liable to sustain the additional momentum arising from the more forcible contractions of that chamber; whether these are determined by occasional and transient causes, or depend upon permanent organic disease of the heart itself. On this point, however, I shall have something more to say hereafter.

Still there is no reason, I think, for doubting, that hæmorrhage by *exhalation* may take place, though rarely, from the free surface of the brain, or rather of its investing membranes. But the *rule* is, that it proceeds from the mechanical rupture of a blood-vessel.

In cerebral hæmorrhage, the blood may be effused in one or other of three different situations; viz. upon the external surface of the brain, *i. e.* upon or between the membranes; or into one or more of its ventricles; or into the very substance of the brain itself. In the two former situations it is sometimes, perhaps, poured out by exhalation; in the latter, which is infinitely the most common, it always proceeds from the rupture of one or more blood-vessels.

It is necessary to remember that even when blood is found spread over the surface, or distending the ventricles of the brain, it frequently has not been originally poured out in those situations. If the hæmorrhage into the substance of the brain be considerable in amount, the blood generally forces a passage, by laceration of the cerebral tissue, either into the ventricles, or (less frequently) to the surface; or even in both these directions at once.

In some rare cases blood is found effused beneath or into the pia mater, over a small space only, as between one or two of the convolutions, and no where else. Sometimes it is spread in a thin layer over the whole of one hemisphere, and is found no where else. Sometimes blood is discovered in one or more of the ventricles, and no where else: but all these are comparatively unfrequent events.

When the blood is effused into the substance of the brain, and does not break a passage out, either in one direction or the other, its pressure is not necessarily or immediately *mortal*. The patient, as I have already explained, may survive for

weeks, or months, or years ; and the clot of blood will, in the meantime, undergo very remarkable changes.

The cell, or cavity, in which the extravasated blood is contained, varies much as to its *size*. It is sometimes scarcely of sufficient capacity to receive a large pea : sometimes it occupies nearly the whole of one hemisphere. It is seldom, however, I repeat, that a communication is not formed between the original cavity and the surface in the one direction, or the ventricles in the other, when *much* blood has been shed. Frequently a direct opening is made by the blood from one lateral ventricle to the other through the septum lucidum ; sometimes it passes from the one to the other through the foramen of Monro ; and even the chamber of the septum lucidum itself has been found distended by a certain quantity of blood.

If the clot of blood in an isolated cavity be examined soon after its effusion, it is found to be of a soft gelatinous consistence, and of a dark colour,* much like black currant jelly ; the sides of the cavity are irregular and ragged ; and the cerebral substance of which they are formed is generally, to the depth of a line or two, moist, soft, and as if stained of a reddish or yellowish colour, which is fainter in proportion as it is more distant from the coagulum, and gradually loses itself in the natural tint of the surrounding parts. This latter condition would seem to depend upon a slow imbibition of the serous portion of the effused blood mixed with some of its colouring matter. It begins to manifest itself about the third day from the attack, and is most apparent from the eighth to the twelfth day ; at which period, under ordinary circumstances, the whole of the serum has been removed, and the process of absorption seems to be in active operation. By degrees this stain disappears ; the coagulum becomes more and more compact ; assumes first a brownish, and subsequently a pale red or even yellowish hue ; diminishes continually in magnitude ; and at length may be entirely reabsorbed.

In the meantime the walls of the cavity are becoming less uneven, and clothe themselves, by degrees, as they contract upon the shrinking coagulum, with a distinct membrane of a yellowish colour, sometimes of extreme delicacy, and resembling the serous membranes ; sometimes thick, and apparently fibrous. When the opposite sides of the cell at length meet, they adhere together, and a true cicatrix ensues, the place of which is marked by a sort of fibrous knot, forming a remarkable contrast with the softer texture around it ; or less frequently by a similar induration of a *linear* form. In this case, the sides of the collapsed cavity are sometimes found to be merely applied to each other, without actual adhesion. When, from the great extent of

the original cavity, or from some other cause, its parietes are not ultimately brought into mutual contact, there remains a kind of *cyst*, lined by a smooth yellowish membrane ; sometimes traversed by a few slender threads of cellular tissue which cross each other in various directions ; sometimes filled with a soft, fine, orange-coloured spongy tissue, in which a number of minute blood-vessels ramify ; sometimes containing a gelatinous or serous liquid ; and sometimes apparently empty, having been occupied by some kind of aeriform fluid.

It is impossible to assign the precise period within which these remarkable changes may be accomplished. Dr. Abercrombie has detailed an instance in which a coagulum, that must have been of very considerable size, had entirely disappeared in less than five months. In another of his cases it was seen to be partially absorbed at the end of three months. “ On the other hand, Moulin found a small coagulum not quite gone at the end of a year : and Riobé observed some of the blood still remaining in a cavity of small extent after twenty months. In two cases Serres found a hard coagulum of blood remaining ; in the one at the end of two, and in the other at the end of three years.

It has been said that the cicatrization of the cavity takes place much more slowly and imperfectly when the effused blood has passed *across*, and *torn*, the *fibres* of the brain, than when it has been poured out in a direction *parallel* to those fibres, so as to separate without breaking them.

Dr. Abercrombie states that he had never seen any thing to satisfy him that the cysts are capable of being obliterated by cicatrization. Neither have I. But Dr. Sims, Dr. Bright, and several of the French pathologists of approved credit and accuracy, agree in their description of this obliteration of the cells. And you should bear in mind that a small cicatrix in the brain may very easily escape notice, if not expressly sought for ; especially as the examination of that organ is often conducted, viz. by cutting away thick slices from the hemispheres in rapid succession, in order to arrive as soon as possible at the lateral ventricles, and the base of the brain.

It frequently happens that a patient has suffered, during life, several distinct attacks of apoplexy or of cerebral hæmorrhage ; and that as many cells are met with after death ; exhibiting respectively various stages of that process of repair which has just been described.

These are the changes that mostly take place in the coagulum, and its containing cell, when the hæmorrhage does not prove fatal, and the patient recovers more or less completely. But the same changes do not always, or necessarily, occur. Instead of being gradually removed by absorption, the

extravasated blood appears occasionally to become a solid, organized, and consequently living mass, deriving its nourishment from the arteries of the brain. A man, whose case is related by Andral, was smitten with apoplexy, and remained thenceforward, for many years, hemiplegic. At length he died, of some other complaint, in the wards of La Charité. When his brain was examined there was found, in one of the hemispheres, a mass of a pale red colour and fibrous appearance, traversed by numerous small blood-vessels which anastomosed with those of the brain: the surrounding nervous matter retained its natural aspect; and there was no appearance of any cyst.

Situation of the hæmorrhage.—It has long been known that hæmorrhage does not occur in all parts of the *substance* of the brain indifferently. Morgagni had remarked the frequency of sanguineous effusions in or near the corpora striata and optic thalami; and more extensive subsequent research has amply verified the general correctness of his observation. Rochoux, in the treatise on apoplexy which I mentioned before, published in 1814, has given a tabular account of the morbid appearances observed by himself in the heads of 41 persons, dead after attacks of cerebral hæmorrhage.

In so many as 24 of these, *i. e.* in three-fifths of the whole number, the blood was extravasated in the corpus striatum; in two others in the optic thalamus; in one it was effused into the substance of both the corpus striatum and the optic thalamus of the same side; and in another, beneath the corpus striatum: so that altogether there were 28 cases out of 41, or seven-tenths of the whole number, in which the clot was confined to the corpora striata, optic thalami, and their immediate neighbourhood. In the remaining three-tenths the blood was found collected in several other parts of the cerebral mass; five times in the middle of one of the hemispheres; twice towards the posterior part of the ventricles; twice in the inner and *anterior*, and three times in the inner and *posterior* portion of the hemispheres; and once in the middle lobe.

In Andral's *Pathological Anatomy* you will find a much more extensive table relating to the same subject, and constructed by him from various authentic sources. It leads to the same general conclusions. Thus, among 392 cases of hæmorrhage into the nervous substance, there were 202 (or more than one-half) in which the blood was extravasated at once into the corpora striata, the optic thalami, and that part of the hemispheres of the brain which is on a level with those bodies. In 61 cases (or about one-seventh of the whole number) it was confined to the corpus striatum. In 35 (or one-eleventh of the whole) it was limited to

the optic thalamus; making, in all, 298 instances (or more than three-fourths of the whole number) in which the sanguine effusion occupied the corpora striata, optic thalami, and their immediate vicinity.

The result of my own observation coincides entirely with this, although I cannot, at present, reduce it to a numerical statement.

From the same table we may infer also the comparative infrequency of hæmorrhage into the cerebellum. It is mentioned as having occurred in 21 of the 392 cases; or in about 1 in 19.

Dr. Craigie states that the parts which are the seat of the hæmorrhage may be arranged, in the order of frequency, as follows:—the corpus striatum; the optic thalamus; the hemispheres; the pons Varolii; the crura of the brain; the medulla oblongata; and the cerebellum.

It is natural to seek for some physical explanation of the cause which determines the extravasation of blood in certain parts of the brain more frequently than in others. Some light may, perhaps, be thrown upon this inquiry, by a consideration of the sources of the hæmorrhage, in the various forms of its occurrence.

I stated before that the blood *may sometimes* be poured out by *exhalation*, in those less frequent forms of cerebral hæmorrhage to which M. Serres has applied the term *meningeal* apoplexy, and in which the blood is found distending the ventricles, or spread, like a cap, over the surface of the hemispheres, without any laceration of the cerebral matter. This supposition rests, however, rather upon the analogy drawn from what is known to occur in other parts of the body, than upon any decisive and unequivocal evidence. Blood has not unfrequently been discovered in each of these situations, when the most careful scrutiny has failed to trace its source to any ruptured vessel. Yet we cannot doubt that such rupture may have existed—either in some one or more of the numerous vessels of the pia mater, in the one case, or of the plexus choroides in the other—and yet have escaped detection by the most vigilant eye. In Dr. Abercrombie's book there are two interesting examples of extravasation upon the surface of the brain, without any obvious source of the hæmorrhage; the one detailed by Dr. Hunter, of Edinburgh, the other by Dr. Barlow, of Bath.

That the hæmorrhage proceeds from rupture of some of the vessels composing the choroid plexus, rather than from the membrane that lines the inner surface of the ventricles, when the effused blood is confined to those cavities, is the more probable, because the vessels have been actually found broken (as in cases of ventricular hæmorrhage, described by De Haen and Cruveilhier), and because they are liable to well-marked

disease of a nature to render them more than usually fragile. The arteries, for example, which belong to that plexus, are subject to a peculiar kind of alteration that I shall presently mention, as frequently prevailing the whole arterial system of the brain; and its veins are often partially enlarged and varicose. This latter condition has sometimes been mistaken for a collection of small hydatids.

But hæmorrhage into the *substance of the brain* depends *always* upon rupture of some one or more of its blood-vessels: and it is to this fact of the rupture of vessels that we must chiefly look for an explanation of the peculiar liability to hæmorrhage of certain portions of the brain; the corpora striata, namely, the optic thalami, and the parts immediately adjacent to these. The corpora striata are not only of much softer consistence than most other parts of the brain, but they are also traversed by more numerous as well as by larger blood-vessels than are other parts. These facts, and the conclusions to which they point, did not escape the sagacity of Morgagni. "On some occasions (says he) when I have cut the corpora striata into pieces horizontally, I remember to have observed in the external anterior part of each, a little pit as it were, across which lay a very conspicuous blood-vessel. And on other occasions, upon cutting obliquely and slowly, I have remarked in the same situation many red lines, like threads, which were in fact blood-vessels running parallel to one another, and of a larger size than elsewhere." In truth you may often notice the open mouths of a cluster of such vessels that have been divided. Morgagni saw in this anatomical fact a probable solution of the pathological fact that the parts in question are the most common seats of extravasation. In corroboration of these views it is worth remarking that the corpora striata are especially subject to laceration and sanguine effusion, while the surrounding parts remain unhurt, in violent concussion of the brain. And when injections are forced into the cerebral blood-vessels in the dead body, it is in the very same parts, the corpora striata above all others, that a sort of factitious hæmorrhage is produced by the rupture of vessels, and the escape of their contents.

Disease of the cerebral blood-vessels.—I have mentioned some original peculiarities of texture and relation, which may be thought to predispose the blood-vessels of the brain, more than others, to laceration. But the *main* predisposing cause of that event is, doubtless, their great liability to *disease*. Except the commencing portion of the aorta itself, there are no arteries in the body so frequently found in a morbid state as the *cerebral* arteries. And the change to which they are most subject is that deposi-

tion, between their tunics, sometimes of a substance resembling albumen or soft cartilage, sometimes of actual phosphate of lime, to which we commonly apply the term *ossification*. This earthy or cartilaginous deposit exists usually in whitish patches of a roundish or oblong form, disposed at various distances from each other: sometimes in a succession of bony rings, with healthier portions of the artery between them. One effect of this morbid condition is to diminish the *bore* of the affected artery, and to make it of unequal capacity. And as this variation of calibre impedes the free passage of the blood, it tends indirectly to increase the pressure of that fluid against the sides of the vessel. Another effect is to deprive the coats of the artery of their natural elasticity, and to diminish their power of cohesion: and thus to render them weak and frangible, and at length unable to sustain the increased impulse of the blood. This condition occurs in the smaller ramifications as well in the larger trunks of the cerebral arteries.

There is yet another occasional cause of hæmorrhage. The arteries at the base of the brain are subject to *aneurism*, and to consequent rupture. Morgagni has reported cases of aneurism affecting the internal carotid and basilar arteries. Serres has described a case of apoplexy resulting from perforation of the basilar artery, which was dilated, not far from its superior bifurcation, into an aneurismal pouch as big as a hen's egg. Dr. Baillie records an instance where both the internal carotids, on the side of the sella turcica, were distended into little aneurisms, one of the aneurisms being about the size of a cherry, the other somewhat smaller: and similar examples are related by other writers. I have seen two such myself; and a beautiful preparation of one of them is preserved in the museum of the College of Physicians.

CLINICAL LECTURES,

BY DR. CORRIGAN,

*Delivered at the Hardwicke Fever Hospital,
Dublin,*

During the Session, 1840-41.

LECTURE IV.—(FEVER, No. 2.)

WE occupied our preceding lecture with some general observations on fever, that were, I believe, absolutely necessary, previously to going into a description of fever as we see it at present under our immediate observation. I purpose now directing your attention to the characters of the fever at present epidemic, whether those characters serve to distinguish the disease, to serve as prognostic signs, or to be our guides in treatment.

Fever, as now in our hospital, naturally divides itself into three forms, and these are so well marked that we might almost call them classes. In one bed you see a representative of the first class—an approach to the simple inflammatory fever of Cullen. A patient is admitted on the second or third day of fever; the skin is hot; the pulse is quick and full; the tongue is creamy, soft, and white; the countenance is florid; and the skin, with considerable heat, gives a feeling of very slight moisture to the hand of the observer: a few rose-coloured maculæ are on the skin. This case, or the class of cases of which this is a representative, you might, perhaps, at first, suppose, from the vehemence of the symptoms, would proceed to still greater intensity, and would prove to be fever of great severity and considerable duration. It is, however, not so. These cases you will observe almost invariably turn out well, scarcely requiring any beyond the commonest attention. They terminate in a few days (seven or eight), either by gentle and continued diaphoresis, or seem quietly to go off without any marked crisis.

The maculated fever, however, as it prevails now, and has prevailed for some months, admits of a division into two forms; one, where the disease sets in with a heavy congestive stage, resembling almost the cold stage of ague; the other, where the onset of the fever is so gradual, that it seems to attack, as it were, by treachery; it seems hard, or sometimes impossible, to say at what precise moment the fever has commenced. The cases of two brothers, Herberts, just discharged cured, and the case of the woman Harrington, not yet out of danger, are good examples of the first form. One of the brothers, Herberts, presented himself at the hospital gate for admission immediately on his falling ill: he walked from his work to hospital. It would not be easy to forget his aspect: he was a stout-built man, and had been previously strong, yet he seemed, as it were, at once knocked down by the onset of the fever. He was chilled and sunken; his face anxious; his eyes slightly congested and drowsy; and his lips livid, or bluish; his pulse quick, and weak, and small. It is a singular circumstance that about ten days after his admission, his brother, who had been in England, and without any communication with him, was admitted immediately after he landed from the packet, into the hospital, with a fever of exactly the same character. Perhaps we may venture to explain this circumstance thus:—that members of the same family, having similar conformations and similar constitutions, are naturally liable to suffer in a similar way from epidemic influences; and thus brothers and sisters may, without any communication with each other, present fevers of similar characters. Perhaps

it may more often arise from this cause than is supposed, that several of a family will suffer from fever; and this similarity of constitution may often be the cause of fever attacking many of a family, when contagion has wrongfully been charged with the whole blame. We see an illustration of this same principle occasionally in children: when children of a family, separated from one another, will almost simultaneously catch whooping-cough, or other epidemic diseases. Popular observation says this has happened because the disease has run in the blood: the explanation appears to be that already given. The children of the one family resemble one another more in constitution than strangers; the constitution of the one child has felt the influence of the prevailing epidemic agent: there is nothing difficult to imagine in supposing that a similar constitution at some distance should bow to the same influence; and thus, perhaps, in those cases, as in the case of the brothers, Herberts, the same influence produced in similar constitutions the same results.

This form of maculated fever is always severe; reaction does not follow; the maculæ are all through the disease thick and dark-coloured, but well marked, although very dark in colour; the patient, even when crisis is going forwards, never loses the aspect of a person suffering under heavy congestion; convalescence is extremely slow, and returning strength a long time coming round.

The second form of the maculated fever is what may be called “*par excellence*” treacherous fever. It is a form that, were it only for your own character, you should remember: the first deviation from health is scarcely perceptible; the patient is for many days what he calls merely ailing; continues at his profession or business perhaps up to the moment of his sending for professional advice, or only remains in bed, and seeks for advice on the sixth or seventh day of his fever, or of the duration of his ailing, in compliance either with the urgent solicitations of his relatives, who see there is something wrong about him, or because he feels, as he describes himself, weak, and without appetite, and disinclined to leave the bed, even although he feels tired of it, and attributes the pains which he describes when questioned as flying down his limbs, as arising from his remaining in bed. When you see such a patient, there is apparently little to excite alarm. There is no complaint of pain except of trifling pains down the limbs; there is perhaps no headache; the pulse is not more than 88, and the tongue is only slightly coated; yet, as surely as such a case presents itself, will that case be a severe case, and one in a few days perhaps where life will hang by a mere thread. Did not experience tell us of the danger, of the severity of the class of which this is a slight

sketch, we could never anticipate that ultimately the danger would be so urgent. Such a case as this will perhaps go on with little change or little aggravation to the ninth or tenth day, when the patient rather suddenly becomes delirious, and either almost comatose, but still without sleep, or, what is worse, becomes exceedingly wakeful and restless; the whole nervous system continues in one uninterrupted state of agitation, resembling delirium tremens; the pulse rises in frequency, and sinks in strength, and the case is an accumulation of the worst symptoms of typhoid fever.

Let us, however, go back on one of those cases of insidious typhoid fever: I wish to impress, if I can, its picture on you, because its features are not so strongly marked, but that, if not well fixed in your memory, they may readily escape your detection. The greater number of cases of typhoid fever coming to our wards are of this class; so that there is no difficulty in your making yourself familiar with their aspect. The nature of the symptoms coincides with what we have already observed of the pathology of this fever. There is no local disease, nor local symptom of any intensity or urgency, but there is not a vital function that is not affected; trifling, it is true, in degree, as far as each may be concerned, but when the total is considered, and it is recollected that all are more or less affected, it is scarcely to be wondered at that in the course of a few days the result of the whole should be so overpowering on the system. The symptoms which point this out also diagnosticate this form of fever, and therefore I shall go over them a little in detail. I am now, recollect, describing the fever when it is most useful to recognize it—when, as I have already said, there is as yet to an inexperienced eye little, or rather nothing, to indicate the danger that is hanging over the patient. Care and attention in observing will, however, prevent any mistake. The patient's muscular and nervous strength is greatly reduced, so that he feels as weak as a child—as he often truly expresses himself. The same want of strength is exhibited in the countenance, which is dusky and anxious and heavy, with an eye often slightly congested, but still with no great heaviness over it. The cerebral system is also disturbed in its functions; there is no sleep, or it is unrefreshing and uneasy. There is perhaps nothing at this stage at all remarkable in the respiration, but the circulation is always a little quicker than natural, sometimes very little; perhaps the pulse beats only 88, but the next day it is 92, the day after 98, and so it goes on gradually rising: there is no sign of local disease to account for, or correspond with, this daily increase of pulse; and as surely as the pulse proceeds

in this way rising from day to day, there is danger preparing to burst. There is nothing to attract attention in the state of the bowels; the evacuations are perhaps natural, and the abdomen is soft. At first sight the tongue perhaps too seems natural, or very little altered; but if it be not dry, or even slightly brown in colour, it is of a grey colour, not with a creamy grey or white mucous coat, such as is presented in inflammatory fever, as already noticed, but without a fur or mucous coat, as if the villousities of cuticle on its surface were dyed of that colour. The patient, at this stage of fever, keeps his mouth generally closed, and breathes through his nose, so that he preserves the tongue somewhat moist; but even then pass your finger over its surface, and it gives you the feeling of not secreting any moisture on its surface, and your eye, if you look close, will detect the same. The patient replies, and the nurse often hastens with glee in such a case to tell you, that he has no thirst, that he does not call for drink; and this is often taken for a favourable sign of the fever being mild: for no other reason, I suppose, than that in inflammatory fever, there being great thirst when the fever is at its highest, it is supposed that when there is no thirst the fever cannot be dangerous. Experience, however, shows the conclusion to be ill founded: want of thirst is a still worse sign than want of appetite; it proceeds from the assimilating powers of the digestive organs being so completely arrested that there is not even the ordinary instinctive desire for drink; not even when the tongue is dry. I attach considerable value to the presence or absence of this sign; I have often foreseen the approach of aggravation of the case when the only warning symptom has been that absence of thirst, or cessation of desire for drink; and again anticipated a change for the better when the only sign to give me the information was return of thirst. The return of thirst, or desire for drink, tells us that assimilation is now going on, that the digestive organs are again taking up their duty, and the desire for drink is the instinctive calling for the supply thus required; as the absence of this instinctive calling had already told us that the digestive organs were arrested in their functions of assimilation. A little reflection will bear out, what experience tells us, that it is just as ridiculous to consider want of thirst a good sign, as it would be to consider want of appetite for solid food as a good sign. In fact, the want of desire for fluids shows a more extreme degree of the loss of the action of the digestive function, of which loss of appetite was the commencement. If you turn now your attention to the skin, it seems at first natural; but place your hand upon it, and although not hotter, perhaps, than natural, there is a dry biting

sensation given by it to the hand that is very disagreeable and very characteristic. It wants altogether the slightest approach even to that soft feeling that is conveyed even from skin much hotter. It is more like the extremely dry sensation given to the hand when laid on the side of a hot-air stove, than anything else I can compare it to; and which perhaps arises from its robbing the hand so quickly of its moisture: perhaps there is also some electric agency at work; but on this we can pronounce no opinion in our present state of knowledge. If the skin be more carefully examined by the eye, a few maculæ are detected about the epigastrium, abdomen, and loins: these are scattered, generally small and dusky, few, or in numbers so great as to give a mottled appearance to the skin, and yet so little different in colour as to require some care to discover them. They are sometimes mixed, even thus early, with the petechiæ of purpura. Various diagnostic signs are given to distinguish between them. This one is sufficient. Press the tip of your finger on the supposed stigma: if it be a petechia it will not disappear, for it is an effusion of blood, and cannot be pressed out of its place: if it be a stigma of fever, it is merely a congestion of a little circle of vessels, and the pressure of the finger will make the colour disappear—to return, however, in a moment again, after the pressure is taken off. The maculæ of a case of fever will sometimes present a very strong resemblance to the eruption of measles. We had a remarkable instance of this a few days ago in the female ward. All the peculiar distinguishing crescentic forms of measles were wanting. I notice the circumstance for two reasons: the first, to call your attention to the resemblance; the next, to fix on your minds the importance of what I am so often advocating, the advantage and facility of studying practical medicine by observation at the bed-side, instead of from books. All the distinctive signs of measles were recollected by you, and accurately given as you had learned them from books; but the most distinctive of all for some time escaped your observation, viz. that measles appear on the face, and maculæ of fever never do. The only remaining function I have now to notice is that of the kidneys. In the period of fever I am now describing, there are always some characters which indicate a departure from health. Indeed, I believe it is a secretion, giving us, by its changes, some of the most useful information in fever. It is often carelessly looked at, and described as natural, when the change is well marked and certain in its indications. To the characters of the urine we shall turn our attention in some of our succeeding lectures, begging of you, in the interval, to study it in the numerous opportunities that our fever wards now present.

AN ESSAY
ON THE
SOURCES OF TYPHUS OR CONTINUED FEVERS.

BY JOSEPH BELL, Surgeon, Barrhead.

[For the Medical Gazette.]*

A SOURCE of much ambiguity in all discussions, is the sense attached to the terms employed. This is well illustrated by writers on typhus fevers: scarcely two authors attach precisely the same meaning to this term. Many confound typhus fever with the typhoid state, which is, in reality, peculiar to no disease, and the attendant of many. "Typhus fever," observes a recent writer, "is not only an endemic disease *sui generis*, but so strong is the predisposition to that form of pyrexia, that it is prone to become an aggravation and superaddition to other forms of fever; and all the remittent types and degrees, as well as the catarrhal and peripneumonic fevers, are apt, either when long continued or improperly treated, under a heating regimen, to glide into it." Others, again, view typhus fever to be an exanthematous disease of a specific character. With this confusion existing regarding the nature of the disease, we cannot expect to arrive at a very satisfactory conclusion respecting the sources from which it originates, until we have a proper notion of what is really meant by typhus fever.

In order to prevent either ambiguity or confusion, I will state that I understand by typhus fever, that endemic disease, *sui generis*, by which our population is so frequently and so extensively affected;—disease which has a definite progress—succession of symptoms—has particular periods of increase and decrease: consequently the following inquiries will have reference to all those divisions of continued fevers adopted by writers on the subject.

Without entering upon any further preliminary remarks, we will at once proceed to examine the most prominent of the sources from which typhus fever is said to originate.

1st: *Contagion*.—I do not intend to trace the spread of fever from one individual to another: such a mode of investigation at best would only give

* This essay was read at a meeting of the Paisley Medical Society, March 2d, 1841.

rise to evidence of a very questionable nature. By taking a survey of our fever hospitals, I think we will be enabled to deduce a sufficiency of established facts to decide whether or not the disease be capable of being propagated by a contagious principle. In the elaborate report of Drs. Barker and Cheyne, on the fever which occurred in Ireland during the years 1817, 18, and 19, we find it stated that, "in the hospitals of the House of Industry, in Dublin, no clinical clerk or apothecary escaped an attack of the disease; and on the 20th January, 1819, it was reported to government that five of the medical attendants of the House of Industry were at that time lying ill of fever. In the city of Cork, nine physicians, in attendance either on dispensaries or fever hospitals, were attacked. Every medical attendant at the South Fever Asylum in that city suffered. At the hospitals of the House of Industry, 170 persons were employed in different offices of attendance on fever patients; and from this part of the establishment were recorded 198 cases of fever*."

These gentlemen state, that the ministers of the Gospel who visited patients labouring under fever during these epidemics, suffered in a very remarkable degree. Dr. Stokes, in his essay on Contagion, remarks, that "the deaths from fever recorded in Saunders' News Letter, from August 1st to December 12th following, are sixty-four, and of these nineteen are clergymen of some of the different persuasions, or of medical men of different descriptions; which appear greater than the proportion which these two classes bear to the whole of those whose deaths we may suppose were mentioned in that manner."

In Dr. Crampton's medical report of the department of Stevens' Hospital, we find it stated, that, "with the exception of Dr. Harvey and himself, all those concerned in attendance on patients caught the disease. None of the nurses, none of the barbers, porters, or those occupied in handling, washing, or attending on the sick, escaped, and many of them had relapses and recurrences of fever."

Dr. Roupell, in his essay on Typhus, gives a similar statement regarding the nurses attending patients in St. Bar-

tholomew's Hospital. He affirms that "among the nurses in attendance upon fever patients in that establishment infection was almost universal."

Dr. West, in his account of the fever cases that occurred in the same institution in 1837-8, states "that since last summer eleven gentlemen who were in the habit of frequenting the hospital have been attacked by the fever, to which three have fallen victims; sixteen nurses and twenty-one patients, admitted for other affections, have likewise suffered from the disease, which terminated fatally in ten instances; and I do not doubt but that similar cases occurred which did not come under my notice."

Dr. Tweedie, in his *Clinical Illustrations of Fever*, observes, at p. 87, "The London Fever Hospital is placed in an open space, situated in the vicinity of the metropolis, close to the Small-pox Hospital. Both these establishments stand in the centre of a large field, where the production of malaria is extremely improbable. I can state, from the most authentic sources, that every physician, with one exception, (the late Dr. Bateman) has been attacked with fever during his attendance, and that three of the eight physicians have died. The resident medical officers, matrons, porters, laundresses, and domestic servants, not connected with the wards, and every female who has ever performed the duties of a nurse, have one and all invariably been the subjects of fever. Last summer a most convincing illustration of contagion occurred. The present medical officer was attacked with fever, and it was necessary in consequence to appoint some one to perform his duties during his illness. The first person who officiated for him resided constantly in the house during the day, but took the precaution of sleeping at home. He was, of course, very much exposed in the wards during the performance of his duties. These, however, were soon interrupted by an attack of fever, which confined him for a considerable time. The duties were then taken by a medical pupil who had completed his education, and entered the hospital in the most robust health. He had been taught, and did implicitly believe in, the non-contagious nature of fever, and ridiculed the idea of any personal danger from residing in the hospital. He

* arker and Cheyne, vol. i. p. 135.

performed the duty of house-surgeon for ten days only, when symptoms of fever appeared."

The same writer mentions several important circumstances respecting the fever which prevailed in the city of Edinburgh in 1817. He states that in the immediate neighbourhood of Queensbury House (which was then fitted up for the reception of fever patients) fever was decidedly less prevalent than in any other quarter of the town. All those, however, who resided in the hospital, including the resident house-surgeon, clerks, apothecary, nurses, were successively attacked."

Dr. Alison, in alluding to the same subject, in his report in the *Edinburgh Medical and Surgical Journal*, vol. xxviii. page 238, observes, "When Queensbury House was formerly occupied by fever patients, every resident clerk, and every nurse in the house, were successively affected with the disease; and since it was re-opened in December last, (1826) the resident physician, two of the clerks (who have not been resident, but have been several hours in the day in the house), the apothecary, several servants, and all the nurses, except two, in all about forty individuals, who had necessarily close intercourse with the sick there, have had fever. . . . During the present epidemic, (1827-8), as well as that of 1817-9, many of the clerks and nurses employed in the Royal Infirmary have taken fever. Since November last, six of the clerks employed in the clinical wards only, four of those employed in the ordinary wards, and twenty-five nurses or servants, have taken fever. All these persons had necessarily frequent and close intercourse with the fever patients in the house, having been employed more or less constantly in the fever wards, excepting four of the servants: of these four, two had been employed in the laundry, where the linen from the fever wards was washed. One was a porter employed at the gate, who would of course have communication with the fever patients at their entrance and dismissal, as well as their relations coming to visit them; and one was in the habit of visiting the fever wards. Not one of the nurses whose duty has confined them to the medical and surgical wards, where no fever patients were admitted, has taken fever, with

the single exception of the woman in the servants' wards above mentioned; and of the numerous patients in the ordinary wards, the only one who has taken fever within my knowledge during the present year, was a patient in the men's general clinical ward, who lay in the bed next the door that communicates with the fever clinical ward.

In the Fever Hospital of Waterford, Dr. Bracken tells us that "in 1818 there were twenty-seven attacks and relapses of fever among the nurses, servants, and porters, whose numbers fluctuated according to the demand for them, but who, on an average, may have been about twenty-two during the year." He also states that the year 1819 had a close resemblance to the preceding one in respect to the nurses and servants being attacked with fever, eighteen of the former having suffered under the disease; seven of them once, three twice, and one three times. The apothecary, who had not been long in the hospital, caught fever, and relapsed twice. During his illness, a young man who performed part of his duties was attacked after a short attendance. A temporary apothecary was then engaged for a few weeks; but he had not been many days in his new employment when he also contracted fever." In the tenth volume of the *Dublin Journal of Medical Science*, Dr. Mateer, of Belfast, gives a table of cases which were admitted into the Belfast Fever Hospital from the year 1818 to 1835, showing the number of patients who had either mediate or immediate communication with affected persons. It appears, from this table, that 7246 of the cases were clearly traceable to contagion, and the remainder, 2342, to other sources.

Dr. Cowan, in his pamphlet on the *Vital Statistics of Glasgow*, says that "all the gentlemen who have acted as clerks in the Glasgow Fever Hospital for many years past, have been attacked with fever, unless they had it previously to their election. During last year twenty-seven of the nurses of the establishment were seized with fever, and five of them died; several of the students have been affected. One gentleman, who acted as apothecary, died in the house; and if I have escaped, it must be attributed either to being past the period of life in which fever usually takes place, or to my being secured by having had two dangerous attacks at

an earlier period of my career, when acting as physician's clerk in the infirmary during the epidemic of 1816-17-18."

Dr. Davidson, in his recent prize essay, at p. 6, states that "in the Glasgow Royal Infirmary, which is capable of containing 220 patients, during the last six or seven years almost every clerk and nurse of the establishment have caught fever while acting in the wards. On the other hand, the nurses connected with the medical or surgical wards in the adjoining building have almost uniformly escaped."

I will not occupy your time by citing any other authority on the subject. The foregoing quotations afford sufficient facts for our purpose. It must inevitably follow, from these facts, that typhus fever is a contagious disease; and that contagion is a most prolific source of its origin. I say that such is the legitimate conclusion to which we must come, unless some fallacy be detected that overthrows this relation.

Let us, then, examine this matter with due caution and discrimination. In the first place I would observe, that the atmosphere of hospitals may be deteriorated by exhalations emanating from the congregation of patients, and from bad ventilation; and this may be the cause of the attacks of fever occurring in these hospitals. If fever originated from these sources, we would expect to find it occurring with the same degree of frequency in the medical and surgical wards of hospitals as in fever institutions. Human exhalations, and bad ventilation, exist even to a greater extent in the former than in the latter; and yet we find fever to be a rare occurrence in the former places. Hence, neither bad ventilation, nor emanations arising from the congregation of patients, can be received as a source of fever.

But in the second place I would observe, that the same cause which produces fever among the inhabitants of ill-ventilated and filthy places, may also exist in hospitals (namely malaria generated in large towns), and give rise to the disease among the inmates of an hospital. If this supposition were true, we would surely find this malarious principle exercising the same effect in the medical and surgical departments of our hospitals, as in the wards set apart for the reception of patients

labouring under fever. Such, however, we have seen not to be the case. Fever appears, indeed, to occur very seldom among either the patients or their attendants in the medical and surgical wards of hospitals.

There is still another circumstance which we must take into consideration before we can be justified in concluding that fever is contagious. Though it were proved that malaria be not generated in an hospital, yet this malaria may be conveyed there by means of the patient's clothing; and thus the attendants may be seized with the disease by coming in contact with them. Such an occurrence, however, is utterly impossible in the vast majority of fever hospitals. It is the rule, in these institutions, to remove the clothing from the patients, to put them into a bath, to shave their heads, and to give them clean linen, before they are admitted into the wards. This plan is adopted in the Glasgow Royal Infirmary; and Dr. Bracken tells us that the same regulation is observed at Waterford Hospital. This gentleman states "that according to one of the rules of that hospital, every patient has his hair cut at the time of his admission; he is also well washed with warm water and soap, and supplied with linen before he enters the sick ward."

Taking every thing into consideration, we are duly authorised to conclude from the facts before us, that contagion is a most fertile source of the typhus or continued fever of this country. Having established this point, let us in the next place ascertain if contagion be the sole source of fever; or if it be also originated by other causes.

If I were to adopt the mode of reasoning employed by Dr. Davidson, of Glasgow, in his recent essay, to which I have already alluded, the question would be soon settled. He contends, that if it be proved that fever is contagious, it must follow that it never originates from any other source. At page 2d he observes, "Some authors, apparently to get rid of the difficulty, and to account for the occurrence of typhus where no contagion could be traced, have adopted the opinion that it may be generated by common causes, such as impure air, filth, &c., and be afterwards capable of propagation by contagion. The argument of analogy is directly opposed to this belief; for

if, in nature, there be no exception to the law that two causes are never required to produce precisely the same effect, it will follow, that whatever cause can be best reconciled with the phenomena of typhus, must be considered the true source of the disease. But in order to apply this principle more immediately to the subject, it may be necessary to appeal to the various morbid poisons, the laws of which are known and generally admitted. The first we shall notice are those which are admitted by all writers to be propagated by one cause only, viz. matter, whether ponderable or imponderable, as the pus contained in a variolous pustule, or imponderable, as the effluvia issuing from a patient labouring under small-pox. Measles, scarlet-fever, hooping-cough, are propagated only by the effluvia which are generated by the patient. . . . Almost all the contagious diseases of the skin, such as syphilis, scabies, the yaws, &c. furnish examples of propagation by only one cause, viz. contagion. . . . If it be true, then, that all the contagious fevers known can be propagated only by contagious matter, and by no other cause—however much their contagious qualities, their prevalence, and their fatality, may be increased by other causes—it must follow from the law of analogy that if typhus can be proved to be contagious, it must also be propagated only by one cause, viz. contagion.”

I confess that I am very cautious in adopting either the statements or conclusions of a writer who asserts an opinion, and then brings forward facts and statements from all quarters in support of his views; while he either omits or explains away every circumstance which favours an opposite opinion. No person, indeed, however high his attainments, is qualified to investigate any subject, if he have not divested his mind of every preconceived opinion, and entered on the consideration of the subject in a truly eclectic manner.

Not to speak of the ambiguity of the above quotation, it displays a spirit very reprehensible, an animus totally opposed to all investigation and research, and highly injurious to the advancement of medicine. If we adopted his views, we must exclude ourselves from an investigation, “which, though

it should appear barren at present, may afford important results in some more advanced era of medical science.” His reasoning, however, is illogical; his arguments are perfectly futile. Unless he proves that the laws by which the operations of nature are governed are precisely analogous to those by which contagion is regulated—until he proves this, “the law of nature, viz. (that two causes are never required to produce the same effect), can have no reference to the question at issue.”

His second argument, though it possess more plausibility, is yet equally fallacious. It amounts to that species of sophism denominated *petitio principii*. It is not admitted that small-pox, measles, hooping-cough, scarlatina, and the skin affections, which he has placed in the same category, are never originated but by contagion. The instance of small-pox is perhaps more favourable to his opinion than that of the other diseases. With reference to measles, and still more to the others which he mentions, every experienced practitioner must have met with instances so apparently sporadic, that if his mind had not been prepossessed with the idea of contagion, he would never once have thought of attributing them to this source; and he must also have seen a number of cases of each of these diseases occur in a neighbourhood so simultaneously, as to exclude the supposition of contagion as a probable cause of their appearance*. But even admitting that these contagious diseases were never originated but from contagion, and admitting their analogy to typhus fever, it does not follow that the latter is only propagated by contagion, unless we have facts to substantiate the conclusion; much less if facts exist to favour a contrary inference. But does there exist any real analogy between the other contagious febrile diseases and typhus? I am inclined to think that, so far as we have facts to guide us, any analogy which may obtain is merely apparent.

Analogical reasoning at best is open to fallacy, and never ought to assume the place of facts. In medical investigations it is a source of great error; little reliance can be placed upon its evidence, owing to the state of our ig-

* Brit. and For. Med. Review.

norance respecting the laws by which diseases are originated and propagated. In such a state of knowledge we should receive with very great caution and discrimination any inference deduced from either apparent or slight analogies, which, I am convinced, occurs in the present instance.

The analogy is attempted to be proved by the following laws of exanthematous fevers:—

1. The contagion can be traced in families, hospitals, schools, &c.; and those exposed to it are generally infected.

2. They only affect persons once during their lives.

3. They are characterized by an eruption which has a rise, progress, and decline; and the disease cannot be checked *in limine*.

Do we find these laws prevailing in typhus fever?

With regard to the first law, it does not always hold good, even in exanthematous diseases. Many of these affections occur where no such connexion can be traced; and innumerable instances take place where persons escape the disease although exposed to the contagion. But even though this law were fully admitted, regarding both classes of disease, yet it would only prove that typhus fever was a contagious disease, not that it always originated from contagion.

2dly. Does typhus fever only affect persons once during their lives? The writer states himself, at p. 16, “that the evidence on which this law is founded in typhus, is by no means so clear and satisfactory as it is in exanthematæ.” If such be the case, why then does he assume this alleged law as a fact, and argue from it, as such, that because typhus is analogous to exanthematæ in this particular, it cannot originate from any other source than contagion. Such a mode of reasoning, or of deducing general laws, is too absurd to merit comment, especially when many facts have been observed by competent authorities to prove the contrary. I have witnessed many instances of typhus occurring more than once.

3dly. Is typhus always characterized by an eruption which has a rise, progress, and decline, analogous to what occurs in exanthematæ? We will

adduce the proofs which the learned Dr. has brought forward on the subject. At p. 18 he states, “It was, however, observed by Rodgers, in the fever which prevailed in Ireland during the year 1731; and one of the characteristic symptoms is described as a *universal efflorescence of petechiæ*.” Also by Huxham in 1734-5; Sir John Pringle in 1750, &c. No particular conclusions can be drawn from these authors’ account of it! Farther, at p. 19, he states that “the typhoid eruption was also a very general characteristic of the epidemic fever which prevailed in Ireland during the years 1817-18-19.” At p. 20 he states “that Dr. Alison described it as a very frequent symptom which prevailed in Edinburgh at that period.” “M. Louis observed it in 26 cases out of 35; Chomel, out of 70 cases, in sixteen had no eruption; Roupell found it in 70 out of every 100 cases; Dr. West observed it in 42 cases out of 60; Dr. Cowan, in 2000 cases, found the average to be 73·99 out of every 100 admitted; Dr. Craigie saw it in 79 among 169 cases, in the Edinburgh Royal Infirmary; Dr. Henderson discovered it in 108 cases out of 130, in the same institution, at a subsequent period.”

In the Glasgow Royal Infirmary, from May 1st to November 1st, 1839, out of 691 admissions, 250 had no eruption. If I may be allowed to quote my experience on the matter, I have found it in 45 out of every 100 of the cases of which I have notes.

Such, then, are the proofs adduced on this point. I would ask, is there any analogy between the frequency of the eruption, as proved here, and the appearance of the eruption peculiar to exanthematous diseases? not to speak of all want of evidence respecting the existence of regular periods of rise, progress, and decline of the typhoid eruption. That an eruption, a peculiar eruption, does frequently attend typhus, I frankly admit; but that this eruption occurs with such a degree of frequency and regularity as either to establish an analogy between the disease and exanthematæ, or to authorize the inference that, because the latter are never propagated except by contagion, the former must always arise from contagion, I am not prepared to allow.

With regard to the impossibility of checking typhus *in limine*, he does not adduce a single fact to prove this statement. It is true, he argues that those cases of fever said by some writers to have been checked in limine, may not have been cases of typhus; but this is no evidence either for or against his general law. He has fallen into the error common to many writers, of deducing positive conclusions from negative considerations. We are, therefore, justified in concluding that there exists no analogy between the two classes of disease, that authorizes the inference which Dr. Davidson would have us to adopt.

Typhus fever and exanthematæ are very dissimilar in many respects, and may they not also differ in this very particular—namely, that, though exanthematæ are never produced except by contagion, yet fever is frequently originated from other sources? A writer in the British and Foreign Medical Review observes on this subject, “Nor does it appear that there need be much hesitation in making the assumption, since it is so much in accordance with the convictions of many practical men of acute discernment.” Dr. Bateman says, on the same subject, “We cannot doubt that a great number of the cases of fever which occur during an epidemic season, are entirely independent of contagion for their origin.” Dr. Prichard observes that “instances are very numerous in which fever has arisen under circumstances almost precluding the possibility of an origin in contagion; and so many examples of this description have fallen under my own observation, as fully to persuade me that this disease does originate spontaneously, or independently of communication with any infected body.” Dr. Percival, and a host of other practical observers, hold the same opinions.

It is my own conviction that not only typhus, but also many of the exanthematæ, arise sporadically, and become the centres from which the respective diseases are propagated. Such, at all events, must have been their origin at one period. To reconcile the opposite doctrine with experience, this contagious principle must have a kind of nature, and be occasionally for considerable periods shut up,

as a writer on the subject has expressed it, in “holes, chests, and caves, and has even for a time made its hiding-place in spiders’ webs*.”

[To be continued.]

OBSERVATIONS

ON

THE STRUCTURE OF THE GUBERNACULUM,

AND THE DESCENT OF THE TESTIS IN THE FŒTUS.

COMMUNICATED BY T. B. CURLING, ESQ.

[For the Medical Gazette.]

DISSATISFIED with the description of the structure of the gubernaculum, and the causes of the descent of the testis, commonly given in English works on anatomy and physiology, I have recently made a careful examination of these parts, and conceiving that the results may be interesting to the younger members of the profession, I beg to forward the following observations to your journal for publication.

The chief facts ascertained by Mr. Hunter, in reference to the descent of the testicle, are so well known that it is needless to occupy space by repeating them here. Mr. Hunter observes, in regard to the structure of the gubernaculum, which is so intimately connected with this process, “It is hard to say what is the structure or composition of this ligament; it is certainly vascular and fibrous, and the fibres run in the direction of the ligament itself, which is covered by the fibres of the cremaster or musculus testis, placed immediately behind the peritoneum. This circumstance is not easily ascertained in the human subject, but is very evident in other animals; more especially in those whose testicles remain in the cavity of the abdomen after the animal is full grown†.” He adds, “In the human fœtus, while the testis is retained in the cavity of the abdomen, the cremaster is so slender, that I cannot trace it to my own satisfaction, either turning up towards the testis or turn-

* British and Foreign Medical Review.

† Works, with notes by Palmer, vol. iv. p. 7.

ing down towards the scrotum. Yet, from analogy, we may conclude that it passes up to the testicle, since in the adult we find it inserted or lost on the lower part of the tunica vaginalis, in the same manner as in the adult quadruped."

Sir Astley Cooper states that the gubernaculum "is composed of several strong ligamentous fibres, which proceed through the inguinal canal to the cellular membrane of the scrotum, in which it is lost*." He also remarks, "The cremaster, as far as I can distinguish it in the foetus, passes upon the gubernaculum to the epididymis and testis, and is attached to the process of peritoneum, which descends with the testis as a pouch, to the lower part of the inguinal canal; and the testis descends into this muscle as into a purse, as it is directed down by the gubernaculum, and hence the loops which it forms."

From these passages it will be seen that Mr. Hunter and Sir Astley Cooper described the gubernaculum as attached to or lost in the scrotum, but mentioned no other attachment. Mr. Hunter conceived that the fibres entering into the composition of this body were muscular, and identical with the cremaster—a conclusion at which he arrived chiefly from analogy, but which was not clearly established by anatomical observation in the human foetus†. Sir Astley Cooper seems to have traced the fibres of the cremaster passing upon the gubernaculum to the testis, though not very distinctly. His meaning in the second quotation given above is somewhat obscure; but when the passage is considered in conjunction with fig. 3, in plate 9 of the same work, in which the testis is represented still in the abdomen, whilst the cremaster is seen surrounding the pouch of peritoneum in the inguinal canal, it would appear that he regarded the cremaster as forming a

pouch ready for the reception of the testis previous to its descent. He says nothing of the cremaster becoming everted, as obviously takes place in certain animals in the descent of the testicle, at the period of sexual excitement. Sir A. Cooper and Mr. Hunter both contended that the cremaster is not the cause of the descent of the testicle.

M. Jules Cloquet, whose description of the cremaster is now generally regarded as the best and most accurate, observes, "The cremaster does not exist before the period at which the descent of the testicle takes place; it is formed in proportion as this organ, which is drawn down by the gubernaculum, passes from the abdomen into the scrotum."* In a foetus of not more than five or six months, he says, the fibres of the internal oblique are completely enclosed within the inguinal canal: "towards their middle they adhere somewhat closely to the gubernaculum; and when this part is drawn downwards, they are seen to descend with it through the external ring, forming successively curves, whose concavities are turned upwards, and which afterwards become developed upon the testis and the spermatic cord. The laxity of these inferior fibres facilitates their elongation, and finally their descent through the external ring. By drawing down the gubernaculum, we simulate in some degree the natural descent of the testis, and form an artificial cremaster."† M. Cloquet here adopts a somewhat similar view to that previously suggested by Carus,‡ viz. that the cremaster is formed during the progress of the testicular descent. It seems surprising that this excellent anatomist, who, it may be presumed, was not ignorant of the facts presented by comparative anatomy, should have arrived at a conclusion so incompatible with the relations and condition of this muscle in animals, in whom it is clear that no cause of a mechanical nature could thus operate in producing such an arrangement of the muscular fibres.

Having briefly stated the views commonly entertained and received in this

* Observations on the Structure and Diseases of the Testicle, p. 41.

† Mr. Owen, reasoning likewise from analogy, and looking upon the cremaster as a part too important to be formed accidentally, fully believes that this muscle exists as such in the human foetus, prior to its descent, though he does not admit to have seen it at that period—See note to Palmer's edition of Hunter's Works, vol. iv. p. 8. It is but justice also to an excellent anatomist, Mr. Wilson, to mention that he observed the fibres of the cremaster running upwards on the surface of the gubernaculum—Lectures on the Urinary and Genital Organs, p. 102.

* Anatomical Description of the parts concerned in Inguinal and Femoral Hernia, translated by A. M. M'Whinnie, p. 7.

† Ibid. p. 8.

‡ Comparative Anatomy by Gore, vol. 2, p. 347.

country, in regard to the structure of the gubernaculum and the descent of the testis, I proceed to relate the results of my own observations on this subject, in the examination of numerous foetuses at different periods of utero-gestation. For the opportunity of making these inquiries, I am indebted to the kindness of several medical friends.

The gubernaculum is a soft solid projecting body of a conical form, but which varies somewhat in shape and size at different stages of the testicular descent. It is situated in the abdomen, in front of the psoas muscle, to which it is connected by a reflection of peritoneum, which invests it in the same manner as this membrane surrounds the different abdominal viscera, the posterior and attached part at which the vessels enter being uncovered. The gubernaculum is connected above to the inferior extremity of the testicle* and to the lower end of the epididymis and commencement of the vas deferens. The lower part of this process passes out of the abdomen at the abdominal ring. On laying open the inguinal canal, the gubernaculum, which is further exposed, seems to diminish in substance, and to spread, but by a little careful dissection it may be found to terminate in three processes, each of which has a distinct attachment.

The central part and bulk of the gubernaculum is composed of a soft transparent gelatinous substance, which on examination in the microscope is found to consist of nucleated cells—the primitive cellular tissue. This central mass is surrounded by a layer of well-developed muscular fibres, which may be distinguished with the naked eye, and which can be very distinctly recognized in the microscope as fibres of animal life, by the appearance of the transverse striæ. These muscular fibres, which may be traced the whole way from the ring to the testis, are surrounded by a layer of the soft elements of the cellular tissue, similar to that composing the central mass, and the whole is invested, except

at the posterior part, by the peritoneum. On carefully laying open the inguinal canal, and gently drawing up the gubernaculum, the muscular fibres may be traced to the three processes, which are attached as follows. The external and broadest is connected to Poupart's ligament in the inguinal canal. The middle forms a lengthened band, which escapes at the external abdominal ring, and descends to the bottom of the scrotum, where it joins the dartos. The internal passes in the direction inwards, and has a firm attachment to the os pubis and sheath of the rectus muscle. Besides these, a number of muscular fibres are reflected from the internal oblique on the front of the gubernaculum. From this description the anatomist will at once perceive the similarity between the attachments of the muscle of the gubernaculum, and those of the cremaster in the adult. I have succeeded in tracing out the attachments of the former before the testicle has descended, at different stages of the process, and immediately after its completion, when the fibres have assumed the relations of the cremaster muscle; and of the identity of the two no doubt can I think continue to be entertained. In the descent of the testis, the gubernaculum, including its peritoneal investment and muscular fibres, undergoes the same change as that which takes place in certain of the *rodentia* at the access of the season of sexual excitement. The muscle of the testis is gradually everted, until, when the transition is completed, it forms a muscular envelope external to the process of peritoneum which surrounds the gland and the front of the cord. At this period the fibres connected with the remains of the gubernaculum can easily be demonstrated to correspond with the cremaster. As the testicle approaches the ring and scrotum, the gubernaculum diminishes in size, owing to a change in the disposition of its cellular elements. The muscular fibres, however, undergo little or no diminution, and are very distinct around the tunica vaginalis in the recently descended testis. The mass composing the central part of the gubernaculum, which is so soft, lax, and yielding, as in every way to facilitate these changes, becomes gradually diffused, and after the arrival of the testicle in the scrotum contributes to form the loose cellular

* Every anatomist is aware that the inferior extremity of the testis of an adult is not invested by the tunica vaginalis, which membrane is reflected from the gland at a distance of about five lines, the point being marked by a white and rather irregular line. This uncovered portion of the testis corresponds to the original attachment of the gubernaculum.

tissue which afterwards exists so abundantly in this part. The middle attachment of the gubernaculum, which may be traced to the dartos at the bottom of the scrotum, gradually wastes away, and soon becomes indistinct. Slight traces, however, of this attachment often remain to the latest period of life. After death, in dragging the testicle of an adult out of the scrotum by pulling the cord, the lower part of the gland, which is uncovered by serous membrane, is often found connected to the bottom of the scrotum by a band of firm and dense cellular tissue, which requires division with the scalpel in order to complete the separation. I believe this band to be the remains of the middle attachment of the gubernaculum. In boys, whose testicles are retracted by the action of the cremaster muscle, little pits or indentations may sometimes be noticed at the bottom of the scrotum, occasioned by the integuments at that part being drawn up in company with the testicles. When the testis is restrained in the groin, a cord of dense cellular tissue, the remains of this process of the gubernaculum, may occasionally be traced from the gland to the lower part of the scrotum.

It has been seen that Mr. Hunter, though disposed to believe that the cremaster passes up to the testicle whilst in the abdomen, nevertheless came to the conclusion that this muscle is not the cause of its descent. It is difficult to understand why, after arriving at the former conviction chiefly from analogy, he was not induced by the same process of reasoning to conclude that a muscle capable of drawing down the testicle in animals would be adequate to accomplish the same purpose in the fœtus. The necessity for some active agent to effect this change in the latter, would appear to be greater even than in animals, since, in the usual position of the fœtus in utero, the passage of the testicle is contrary to gravitation, and unaided by the movements of respiration. Now when we consider the attachments and connections of this muscle in the fœtus, the perfect condition of its fibres, as ascertained by microscopical examination, and the circumstance that there are no other means, no other motive powers, by which this change can be effected, or in any way promoted, I see no reason to question

that the cremaster discharges the same office in the human embryo as that which it undoubtedly performs in certain animals at a particular season. The fibres proceeding from Poupart's ligament, and the obliquus internus, tend to guide the gland into the inguinal canal; those attached to the os pubis to draw it below the abdominal ring; and the process descending to the scrotum to direct it to its final destination. As the descent approaches completion, the muscular fibres, which perform so important a part in it, gradually become everted, assume new relations, invest the gland, and instead of drawing down the testicle, acquire the new functions of elevating, supporting, and compressing it.

After the preceding observations had been put on paper, I was led to consult the writings of some of the continental authors on the subject of the descent of the testis. In works, evidently little known, and rarely referred to by English writers, whose leaves remained uncut in the libraries of the College of Surgeons and Medico-Chirurgical Society, I found much interesting information, and not only a fuller and more satisfactory account of the gubernaculum and testicular descent than is to be met with in anatomical works of recent date in this country, but that many of the points which I had myself remarked were previously noticed by these writers. The authors whose works I have chiefly examined are Palletta,* Brugnioni,† and Seiler;‡ and their observations, particularly those of the two latter, are certainly well deserving the careful perusal of scientific anatomists. Brugnioni, who wrote in 1785, but a few years after Mr. Hunter, described the attachment of the gubernaculum to the os pubis, and distinctly recognised the existence of the cremaster in the fœtus; as will appear from the following passages. "*A testis extremitate inferiori corpus quoddam pyramidale cujus basis erat superior, apex inferior, versus inguen pergebat, ut, disjunctis fibris, partim in Fallopii ligamentum et musculi obliqui interni inferiorem oram, partim, superato obliqui externi*

* *Nova Gubernaculi Testis Hunteriani et Tunicae Vaginalis Descriptio, &c.* Edit. Sandifort 1788, but originally published in 1777.

† *De testium in fœtu positu, &c.* Edit. Sandifort, 1788, but first published in 1785.

‡ *Observationes nonnullae de testiculorum ex abdomini in scrotum descensu*, Lepsiae 1817.

annulo, in ossium pubis latera, et scroti (quod ea ætate vix ullum est) imam partem se insereret.”* “Albus autem stipatus, et planiusculus ille funiculus, diversus omnino a cellulari commune, qui uno fere filamento in os pubis superius, duobus verò, tribusve in inferius os implantatur, nil aliud, ut opinor, est, quam fasciculus aliquis carnosus ipsius cremasteris a pubis osse ortus, vel quaedam tendineae fibrae a femoris vagina aberrantes.”† Brugnani also correctly described the gradual eversion of the gubernaculum and cremaster in the descent of the testis.‡ Seiler gives the following account of the gubernaculum. “Hæc vagina parvum ligamentum conicum (gubernaculum Hunteri) includit tela cellulosa spissa formatum, quod in apice plicæ vaginæ illius, ubi testiculi et epididymitis inferiores apices se tangunt, incipit, descendendo fit crassior, per anulum abdominalem transit, in duos tenues se dividit ramulos, quorum unus cum tela cellulosa se coniungit, quæ ante anulum abdominalem aponeuroticam expansionem tegit, alter gracilior adhuc versus symphisin ossium pubis cum tela cellulosa spissiore se commiscet. Si involucrum illud a peritoneo formatum vaginam cante levamus, musculares fibras detegimus, quæ a musculo abdominali obliquo interno et transverso sursum se incurvant, massam illam cellulosa obducunt et cum illo cohærent ita, ut ligamentum gubernaculum Hunteri, maxima saltem ex parte, tendinosa illarum fibrarum muscularium productio esse videatur, transitus enim telæ cellulosa in membranosa serosa et tendinosa compagem, est fere inobservabilis.”|| These observations are illustrated by a well-executed engraving, in which the fibres reflected from the internal oblique to the gubernaculum are distinctly represented. Seiler remarks, “Cremasterem constituunt fibrae illæ musculares, gubernaculum Hunteri obducentes, quæ augentur et amplificantur.”¶

After giving a minute and accurate description of the cremaster in the rat, and others of the Rodentia, and of the descent of the testis in these animals

during the season of sexual excitement, this intelligent author asks, “Si vero in his animalibus talem exercent functionem, cur illis non similem in embryonibus humanis concedamus actionem, ubi similem invenimus structuram?”* I might quote many other passages full of interest and information; but enough has been adduced to call attention to the labours of these neglected authors. Meckel, a more recent writer, whose *Manual of Anatomy* has always maintained a high repute, described the gubernaculum as commencing at the upper part of the scrotum, and receiving fibres from the internal oblique and transversalis, which, becoming everted in the descent of the testis, form the cremaster muscle. He appeared to be unaware of, or not to admit, the connection of the gubernaculum to the pubis, and the process extending to the bottom of the scrotum, and he consequently came to the same conclusion as Dr. Hunter—that the muscular fibres are insufficient to bring the testis lower down than the abdominal ring and complete the descent, the cause of which he regarded as unknown.†

I have been chiefly induced to bring forward the foregoing observations, in order to correct the wrong impression in respect to the cremaster, which it appears has been made in this country by the work of a French anatomist of considerable reputation, M. Jules Cloquet (*Recherches Anatomiques sur les Hernies de l'Abdomen*).‡ I have no hesitation in declaring that the views of this writer on the formation of the cremaster, as quoted above, which have evidently been adopted by many British anatomists§ without due examination,

* Ibid. p. 37.

† Manuel d'Anatomie générale, descriptive et pathologique, tr. par Jourdan et Breschet, tom. iii. p. 670, et seq.

‡ This work has been made more extensively available to English students by the excellent translation of my friend, Mr. M'Whinnie, of St. Bartholomew's Hospital, who offers, however, no objection to the views of his author on the formation of the cremaster.

§ Mr. Harrison, in speaking of the cremaster, observes: “This muscle is probably formed incidentally, the testis, in its descent to the scrotum, carrying before it the lower border of the internal oblique (Dublin Dissector, 5th edit. p. 838, p. 143.) Mr. Quain's account of the cremaster strictly agrees with that of M. Cloquet. He believes that it does not exist before birth, and describes the gubernaculum merely as a fibro-cellulose cord, which extends down through the inguinal canal to the external surface of the pubic symphysis. (Elements of Anatomy, 4th edit. 1837,

* Lib. cit. p. 219.

† Ibid. p. 229.

‡ Ibid. p. 226 & 7.

§ Speaking of the peritoneum.

|| Lib. cit. p. 20.

¶ Ibid. p. 27.

are erroneous and inaccurate, and moreover, inconsistent not only with the original and exact observations of Mr. Hunter, but also with the subsequent investigations of Brugnoli, Seiler, and other continental anatomists, and, I may add, with my own inquiries, as detailed in this paper.

1, Mount Place, London Hospital,
March 25th, 1841.

ON HIP DISEASE AND LUMBAR ABSCESS.

By WM. OLIVER CHALK, ESQ.

Resident Surgeon to the Royal Sea-Bathing
Infirmary, Margate.

Of Psoas Abscess.

THE symptoms which characterize this disorder may be divided into three stages. In the first we have flattening of the nates, apparent lengthening of the limb, canting of the pelvis, and dragging of the extremity as in the early periods of hip disease, originating in a like cause, that of giving rest. The gait is uncertain, accompanied by frequent stumbling. If the patient be desirous of picking up any thing from the ground, the limb is thrown behind, the sound one flexed, and the body bent forward: in this way any pressure on the psoas muscle of the side affected is avoided. Now this is altogether different from the manner (already described) in which the same object would be obtained by one suffering under morbus coxæ. There is more or less languor after exertion, dull pain in the loins (not, however, invariably present), occasional pains along the limbs, chiefly confined to the knee, especially at night and in the recumbent posture: they are at first erratic, but afterwards become fixed in that articulation. The latter symptoms are peculiarly diagnostic of this disorder, particularly the pain at night. The patient, when lying, rests the limb on the sound one, the knee is bent, and the thigh is flexed on the pelvis. In the erect posture the extremity is thrown forward, and the toe is scarcely

allowed to touch the ground: by this means the knee is bent, and the thigh partially flexed on the pelvis. The position adopted in both instances is obviously the one most calculated to relax the psoas muscle, and to maintain it in a state of quietude. The rest is broken, accompanied by occasional moanings and startings: these symptoms become more constant and distressing as the disease advances. If the patient be now submitted to a careful examination, by making pressure in the lumbar region on the side of the spine of the limb affected, or be placed in a supine position, and the abdominal muscles relaxed by flexing the thighs on the pelvis, and the psoas muscle be examined through the parietes of the abdomen, we may succeed in discovering a tenderness of the parts. At this time it will occasionally happen that a slight projection of one or more of the vertebræ may be observed, but this is not necessarily the case; for should it proceed from caries the bodies may not as yet have undergone sufficient change to produce curvature; besides which, depositions of tubercular matter in the loose cellular tissue surrounding the psoas muscle (constituting psoas abscess) constantly take place, independent of any spinal affection. Yet it should always be borne in mind that accumulations of pus in this situation do give rise to it; so that the spinal affection may be cause or effect.

The duration of the symptoms already described, constituting the first stage, is very uncertain; it may be confined to weeks, months, or even to years, (obeying the laws which regulate depositions of scrofulous or tubercular matter in other structures of the human frame), depending entirely upon the state of the patient's general health, unless suddenly developed under the influence of a local injury superadded to the original mischief. The following case is an instance in which the primary symptoms extended throughout a period of nearly six years.

John White, a delicate boy, æt. 11, admitted July 1st, 1838. He states that about five years since he fell whilst at play; immediately afterwards he complained of weakness and of occasional slight pain in the right hip and groin. About twelve months afterwards he began to experience darting sensations in the hip and knee, more especially at

pp. 381 and 877.) Mr. Morton, in a useful work just published (*The Surgical Anatomy of Inguinal Hernia, the Testis, and its Coverings*), quotes Cloquet's description of the cremaster, and fully adopts his view of its formation. (p. 231 and 269.)

bed-time: these have continued up to the present time. The spine is curved laterally, he is pigeon-breasted, the buttocks on both sides are of equal fulness; locomotion is unimpaired. He complains of great pain on pressure at the pit of the stomach; the tongue is furred, bowels irregular, pulse quick; he suffers from slight thirst. The hepatic treatment was adopted, and he underwent a course of bathing, beginning with the warm salt-water bath three times a week, which was afterwards followed by the douche, warm and cold, applied along the course of the spine at similar intervals. By these means his general health and strength were greatly improved, the pains in the hip and groin, together with the spinal and thoracic deformity, were much relieved. He was discharged October 12th. He returned to the Infirmary June 18th, 1839, much in the same state as when he left in the previous autumn, except that he had acquired a limping gait, and walked with a stick. There was also a slight recurrence of the hepatic symptoms. A similar medical treatment was now pursued, together with the warm salt-water bath. His general health and strength improved as before. Just previous to his departure (October 8th) I observed a fulness on the upper and outer side of the thigh, which yielded an obscure feeling of fluctuation. The swelling continued to increase during the winter, and he suffered much pain in the hip and knee. Any attempt at motion caused great uneasiness. He was again admitted as an in-door patient, May 20th, 1840. The abscess, which had increased to a considerable size, broke the day previous to his admission, and about a pint and a half of thin straw-coloured fluid, mixed with the usual tuberculous matter, escaped from a very small opening low down on the outer side of the thigh. The discharge was profuse for several weeks, but gradually abated. The general treatment consisted in a careful regulation of the bowels by mild aperients and enemata of warm water, together with tonics, and a generous diet of wine, ale, &c. The local applications were, ointment of the iodide of mercury applied over the walls of the abscess, with gentle support by a circular bandage applied daily. Ioduretted lo-

tions, and poultices of bread and Ext. Conii, were used whenever the abscess became painful and irritable. He was allowed to move about on crutches, when he could do so without exciting too much pain. By these means the abscess was nearly healed at the time of his departure (Oct. 12), and his general health was excellent. I have just now had an opportunity of examining the patient, and a small cicatrix is all that remains of a once formidable disease. The knee is slightly contracted, and the muscles of the hip have not yet recovered their power, as he is unable to flex the thigh on the pelvis, except by the aid of his hand, but he suffers no pain from so doing. He walks with a stick and crutch. The spine is quite straight, and the thoracic deformity has disappeared. His general health has continued excellent.

I have just now observed that the symptoms of psoas abscess are divisible into three stages, but I should not consider such a division at all necessary, were it not that by so doing I am enabled to point out more particularly a circumstance peculiarly diagnostic of the disorder, and one which will often serve to clear up our doubts when we are called upon to treat it in the latter stage, where it is often so very difficult to determine whether the hip or the psoas muscle be the original seat of the mischief. During the time that pus is accumulating in, and remains confined to this muscle, pain in the hip, knee, &c. must be present in a greater or less degree, according to the extent of the abscess; but no sooner does it acquire sufficient size to gravitate, quit the pelvis, and form on the inner side of the thigh, (or, as is very commonly the case, follow more directly the course of the psoas muscles, and occupy the interspaces of the muscles of the hip-joint,) than the pain in the hip, knee, &c. ceases for a while. The reason of this is obvious: the lumbar nerves now are no longer compressed and irritated, as when the matter was confined to the body of the psoas muscle. This symptom is so constant in lumbar affections that it seems to me of sufficient importance to constitute the second stage. The following case is one in which, from this circumstance alone, I was enabled to establish the diagnosis:—Edward Peel, ætat. 8, was

admitted as an out-door patient, August 16, 1833, from one of the London hospitals. It was stated by the person in whose house he was placed, that he was convalescent from hip disease, and that a slight weakness of the limb, together with general debility, was all that remained of the disorder. On examining the hip, I found the integuments covered with the cicatrices of leeches, blisters, &c.; and I learnt that, for some months previous, he had suffered severe pains about the hip, knee, and along the limb, more especially at night, and whenever in the recumbent posture. It did not appear that the topical remedies, though often repeated, had been productive of relief, but that suddenly, and not long before his arrival at the Infirmary, the pain had ceased. I carefully examined the spine, and found one of the lumbar vertebræ slightly projecting, it afterwards became more prominent as the disease advanced. He was discharged at the closing of the house, on October 30th following, much in the same state as when he came. He did not, however, return home, and I determined on watching the case, directing the party who had charge of him to let me know immediately if any swelling over the nates or in the groin should be observed. Not long after this I was requested to see him. A fluctuating tumor, about the size of a bantam's egg, just beneath Poupart's ligament, was now perceptible. I did not interfere with it, and merely prescribed some general treatment. The tumor increased to a considerable size, but did not cause much pain or inconvenience. He was allowed to move on crutches as usual. At the end of four or five weeks an opening was about to form, the integuments were inflamed, and beneath them could be seen the pus. I now made a small opening with a sharp pointed bistoury, the precise size of the inflamed skin, and liberated four ounces of straw-coloured fluid, with flakes of tuberculous matter. The discharge, after the first exit of the pus, never exceeded a table-spoonful in the twenty-four hours. The orifice soon after ulcerated to the size of a shilling. The after-treatment consisted in gentle compression by means of compresses and a bandage applied round the pelvis, and the application

of the ioduretted lotions to the walls of the abscess, and he recovered in the course of eight or nine weeks. I have heard of him several times since, and have been informed that he continues to enjoy an excellent state of health. I might here take the opportunity of relating many other cases bearing on this particular point, but I trust the foregoing one will be sufficient to indicate the importance of attending to the circumstance. The third stage of lumbar abscess may be considered that in which the suppurative process is fully completed, and an exit to the pus produced either by nature or by art. The soft fluctuating tumor of the nates, formed by the insinuation of pus among the interspaces of the muscles of the hip-joint, has often been confounded with dislocation of the head of the femur on the dorsum ilii. Some surgeons have gone so far as to say that psoas abscess may be distinguished from morbus coxæ from the circumstance of its pointing on the inner side of the thigh only. It would be fortunate if this were true, but the fact is the very reverse, for it commonly points and finds exit behind or near to the great trochanter; and even if it breaks in the groin in the first instance, the pus may subsequently burrow about the muscles of the hip, and form an outlet in the vicinity of the articulation.

The case of George Hastings, æt. 16, shewn in the accompanying woodcut, is an instance of such an occurrence. He was admitted May 12, 1836. He stated that he was a sailor, and that two years since he was occupied in scouring the deck without his shoes or stockings during a wet and cold day; three days after this he was seized with violent pains in the lumbar region, (these symptoms were accompanied by severe febrile action); in a fortnight they subsided, and he then began to complain of pain in the groin, which shortly extended to the hip and knee, and became more especially painful at night, and whenever in the recumbent posture. In attempting to rise suddenly, or after walking, he experienced an acute lancinating sensation in the hip and groin. — These symptoms subsided twelve months prior to the formation of the abscess, which formed a tumor of considerable size, and pointed in the groin

Case of George Hastings.

No. 1. Outlet to the abscess in the groin.

2. 4. Orifices to the abscess.

3. The site of an issue degenerated into a strumous ulcer.

(this happened in December 1835, eighteen months prior to the commencement of the disorder): nearly four ounces of pus were liberated: soon after this several outlets formed in the vicinity of the hip-joint, and discharged profusely. The limb was turned inwards, the knee contracted, and the thigh flexed on the pelvis. He suffered great pain in the hip, knee, and ankle, accompanied by irritative fever. The integuments were covered with the cicatrices of cuppings, leeches, and blisters, and an issue placed behind the great trochanter had degenerated into an irritable ulcer*. On pressing the abdomen in the lumbar region, pus issued freely from all the openings. The patient was altogether in a lamentable condition. It is unnecessary to detail the treatment, which extended over a period of some months. He only quitted his bed a fortnight previous to his departure from the Infirmary (Oct. 25th); and on his first rising it was observed that he had increased considerably in height: the circumstance was so obvious that I felt anxious to ascertain by admeasurement the precise increase, which, according to his own statement, was rather more than three inches. The discharge was now very slight, comparatively speaking, and from two orifices only; all the other wounds were healed. He could walk tolerably well on crutches, and the limb was less contracted. His general health was greatly improved; and a diarrhoea, from which he had been suffering at frequent intervals during

his stay, had now entirely subsided. I selected this case as the subject of a drawing, as it gives an excellent idea of the state of the parts when psoas abscess finds an exit in the vicinity of the articulation and exterior of the thigh.

The situation in which the abscess points would appear to be regulated by the quantity contained in the sac. If it be small it is usually in the groin, immediately beneath Poupart's ligament. It seems probable that the matter remains some time on the brim of the pelvis before an exit is formed, for I do not remember to have examined any patient who died of lumbar abscess, where caries to a greater or less extent was not observable at some part of it. When the accumulation is larger, it follows the course of the psoas muscle, and insinuates itself among the muscular interspaces of the hip and outer side of the thigh, and generally points behind the great trochanter, and in the vicinity of the articulation. If, however, it be very great, as is sometimes the case, it descends beneath the fascia of the thigh, producing great increase in the size, and fluctuation becomes perceptible in all parts of it. In this case various openings may form, both on the inner and outer side of the thigh, and about the hip-joint.

An abscess originally very small may give rise to one of great extent, by involving other parts in its descent. In the case of William Gibbard, æt. 20, admitted July 31, 1837, suffering under posterior curvature of the spine, and a scrofulous affection of the left knee-joint, and who died of enteritis the fol-

* The case had been treated as disease of the hip.

lowing September, I found, on a post-mortem inspection, that a slight caries of the bodies of the 3d and 4th dorsal vertebræ had given rise to an abscess, which had insinuated itself between the condensed cellular tissue at the side of the spine, descended along the psoas muscle, beneath the pelvic fascia, and through Poupart's ligament to the intermuscular space at the upper and inner part of the thigh: no opening had been formed during life. Upon a moderate computation the sac could not have contained less than a gallon and a half of pus.

Occasionally the abscess points in the lumbar region, at the side of the spine, and sometimes it does so after the matter has found an exit elsewhere. In two instances of this description, where the orifices sloughed, and the cases did badly, it was followed by hæmorrhage, which, although restrained for a time, ultimately proved fatal. In the case of W. H. Hilder, æt. 12, admitted July 14, 1835, [who was suffering from scrofulous abscess on the left side of the thorax, and from an accumulation of pus in the lumbar region, which was perceptible at the side of the spine, and formed a tumor of some size], just prior to his departure, Oct. 9th, the matter gravitated along the psoas muscle, and he began to experience pain in the hip and knee, while the swelling entirely disappeared. During the interval that took place between the time of his departure and return to the Infirmary, May 14, 1836, his case had been treated as hip disease. Two openings had formed in the vicinity of the articulation; one of them was situated behind the great trochanter: the discharge was profuse. The case went on badly, and ultimately the integuments sloughed, where a blister had been placed over the articulation, extending to the precise size and form of it. Nothing could exceed the suffering caused by this circumstance: he returned home Oct. 1st, in a hopeless state.

The position of the limb in psoas abscess is almost invariably the same: it is turned inwards, and rests on the sound one; the thigh is flexed on the pelvis, and the knee bent. I have only met with two instances to the contrary, and will take this opportunity of relating them, as they are by no means devoid of interest.

The first is that of Thomas Stapleton, æt. 14, admitted May 11, 1836. I was much struck by the position of the limb: the thigh was situated at right angles from the pelvis, the knee was flexed, and the patient lay on his face. I was at a loss, in the first instance, to account for this, never having had an opportunity of witnessing a similar case. There were two orifices, one on the upper and anterior part of the thigh, and another a little below the former, on the outer side. The discharge was not profuse, but it was unhealthy. On questioning him as to the cause of his position, he said that he was induced to adopt it in consequence of the pain he experienced when lying down. His general health was greatly impaired; he was suffering under hepatic symptoms, and from obstinate constipation of the bowels. The following is the history I gained from the patient:—He stated that on Sept. 3d, 1835, he was seized with sudden and severe pain along the whole course of the right lower extremity, since which time he had been entirely confined to his bed; and that for eighteen months previous to this, when taking violent exercise, such as running, &c. &c., he had been subject to occasional darting sensations along the limb, &c. The hepatic treatment was adopted, and the bowels carefully regulated. The febrile symptoms were allayed by a strict antiphlogistic treatment, and an amelioration of the symptoms ensued. He was afterwards placed on a tonic regimen. The discharge from the orifices diminished. The patient now complained of tenderness in the lumbar region, and a slight blush was observable on the integuments near to the posterior spine of the ilium. A circular bandage was passed round the thigh, and compresses of lint placed over each orifice. These soon healed, and an opening was formed Oct. 17th, where the integuments had become inflamed, and about half a pint of healthy pus was liberated. He suffered considerable constitutional irritation whilst the abscess was forming in this situation.* He left the Infirmary Oct. 24th, greatly relieved in every respect.

John Lineham, æt. 28, admitted May 14th, 1836, suffering under lumbar abscess; this was excited by rheumatic action. He is a post-boy. He states

that about two years since he suffered from pains in the knee, more particularly at night. They continued increasing for a year and a half, and at last, becoming so violent that he could not lie down in bed, he adopted the expedient of placing himself at the side of it, letting the limb hang down; when, according to his statement, it became considerably enlarged (probably from œdema). There were several sinuses discharging profusely in the neighbourhood of the hip-joint. The limb was shortened to about four inches; the knee was much contracted. He left the Infirmary Oct. 24, the wounds for the most part healed, and his general health greatly improved.

[To be continued.]

ON ARTIFICIAL ANUS FOR RELIEF OF STRICTURE OF THE RECTUM.

To the Editor of the Medical Gazette.

SIR,

IN the account of *Cancer of the Rectum* in the Cyclopædia of Surgery, the following paragraph occurs:—"Surgery has yet another mode—in the establishment of an artificial anus—of relieving sufferers from obstructed rectum, and of warding off imminent death. There are, at least, seven cases on record, in which this operation has been performed on the adult subject, when every effort to relieve the distended bowels having been made in vain, symptoms of stercoral tympanites and strangulation, with momentary risk of rupture of the intestines, had set in. Five of these operations proved completely successful, and in one of the others the fatal event was mainly due to previous unskilful treatment: the operation is, therefore, warrantable in a surgical point of view."—(*Cyclopæd.* vol. i. p. 679.) A foot-note refers the reader to the MEDICAL GAZETTE, July 1840, for further information. This reference was made under the impression that a note on the subject, of which I intended to beg the insertion, would appear in your pages of that date. By some accident I mislaid the note, and, until recently, forgot the circumstance. As the point to which it refers is one of practical importance, I

shall feel much obliged if you will give it a place, as now enclosed, in one of your forthcoming numbers.

I am, sir,

Your obedient servant,
W. H. WALSHE, M.D.

Principal circumstances connected with seven cases, in which an Artificial Anus has been established in the adult for the relief of urgent symptoms produced by Stricture of the Rectum.

[1. FINE (An.?) *Vid Odier, Man. de Médecine*, p. 274, ed. 2, 1811.]—Female, æt. 70. Symptoms of gangrene from fæcal retention; tumor completely obliterating the rectum at its origin.

Opening made at "most prominent part of the abdomen;" one or two stools daily afterwards; patient lived more than a year, and died dropsical.

[2. PILLORE (1776). *Vid. L'Expérience*, Janvier 30, 1840].—Adult male. No stool for upwards of a month; not a particle of two pounds of mercury, which had been exhibited by the mouth, discharged. Eight or nine inches of the end of the colon and beginning of the rectum totally obstructed from schirrous induration, &c.

Transverse incision of the integuments above the fold of the right groin, and same of the cæcum; abundant fæcal evacuation. Peritonitic symptoms on the 20th; death on the 28th day. The bowels had not been completely emptied; the exact original weight of mercury being found in a knuckle of the jejunum, behind the bladder.

[3. FREER (1817). *Vid. Med. and Phys. Journal*, vol. 45, p. 9, 1821.]—Male, æt. 47. Contraction of gut complete; attempt made to divide the stricture; no stool for ten days, vomiting, hiccup, &c.; "death inevitable."

Incision above and within anterior superior spine of the left ilium, and in descending colon; painful evacuation; patient, however, gradually sank, and died on the ninth day.

[4. PRING (1820). *Vid. Med. and Phys. Journal*, vol. 45, p. 1, 1821.]—Widow, æt. 64. Obstruction seven inches above the anus; total retention of fæces for twelve days.

Incision within and above antero-superior spine of left ilium, and in descending colon; immediate and forcible discharge of fæces. Three months

after, indurated fæces passed through the natural anus, and continued to do so. Six months after, the patient was in good health.

[5. MARTLAND (1824). *Edinb. Med. and Surg. Journ.* p. 271, 1825.]—Male, æt. 44. A large tumor protruding, as it were, from the neck of the bladder; no stool for twenty-six days; marked stercoral tympanites.

Incision an inch above and within antero-superior spine of the left ilium, and in descending colon; instant escape of fæces and flatus; a year after the patient enjoyed good health: soft stools passed on a few occasions at one period through the natural anus.

[6. AMUSSAT (1839). *Vid. Gazette Médicale*, Oct. 1839.]—Woman, æt. 48. No stool for upwards of *twenty-six* days; nausea, vomiting, &c. Hard, round, immoveable tumors, at the upper part of the rectum.

Incision in left lumbar region, and in descending colon. Four months after the operation, patient in excellent health: one or two evacuations daily: flatus passed by natural anus.

[7. AMUSSAT (1839). *Vid. same paper.*]—Case previously treated by breaking down fungous masses in the rectum, and subsequently by cauterization. Patient in a deplorable state.

Opening made in left lumbar region and in descending colon; no escape of fæces till the fifth day. Four months after, the patient in a better state than before the operation, and about to quit Paris for the country.

“When the disease is cancerous, the chances of ultimate advantage are, of course, vastly less than in cases of retention from simple induration; but even here it may be justifiably performed, provided the patient, after having been made fully acquainted with the nature and likelihood of the benefit to follow, still desire to undergo it.” It is scarcely necessary to add, in defence of this position, that (even admitting the benefit obtained to be necessarily of short duration, which is by no means certain), there are cases wherein the preservation of a given life, even for a few days, may be of the utmost consequence to families, in a worldly point of view.

It will be observed that in Pillore's case the unfortunate issue probably depended on the obstruction caused by the gravitation of the mercury into the

pelvis: the site of the incision was also evidently ill-judged. The cause of death in Freer's case does not very clearly appear from his details.

M. Amussat's motives for preferring the operation proposed by Callisen (the lumbar incision, whereby implication of the peritoneum is avoided) to the inguino-abdominal incision of Littre, are explained in his papers referred to.

ON WOUNDS OF THE FEMORAL VEIN IN OPERATIONS FOR ANEURISM.

To the Editor of the Medical Gazette.

SIR,

So much depends, for the satisfactory and successful performance of all important operations in surgery, upon the state of preparation of the mind of the operator for every possible emergency that may arise, and the possession of means calculated to remove the embarrassment which ensues, that I presume the following brief observations upon a melancholy occurrence, to which even the best surgeons are liable, may not be unacceptable:—

There cannot, perhaps, occur to the surgeon a more lamentable accident than an injury of the femoral vein at the time of applying a ligature to the femoral artery. Quiet, and unaffected by it, as the patient, at the time of its occurrence, appears, the practised surgeon beholds in this apparently trivial incident a sad calamity, fraught with the most certain, though not instant, destruction of his unhappy patient. As the most skilful* and experienced hands are liable to fall into this fatal error as well as the unskilful, and as “there is scarce an instance on record of the patient's recovery†,” when it does happen it becomes the duty of the surgeon to devise, if possible, some means by which he may secure his patient from a danger of his own infliction, more sure to destroy, than the disease he is attempting to cure. Every recorded instance of this kind I have met with proved fatal. In the course of a few months I have seen it

* See *Lancet*, vol. 1, for 1828-9, a case by Sir A. Carlisle; and a paper on Phlebitis, by Mr. Arnott, in *Med.-Chir. Trans.* vol. 15; also Travers's *Surgical Essays*, part 1.

† Hargrave's *Operative Surgery*, p. 94.

twice happen in the hands of different individuals. The first patient died in consequence; the second was rescued by a measure to be presently stated.

That simple wounds of veins, both superficial and deep, properly treated, are unattended by danger, will, as a general rule, be acknowledged by all, I take for granted. It is proved by the result of superficial and deep wounds in phlebotomy, and amputation. I have seen, during the removal of a tumor deeply seated in the neck, the internal jugular vein wounded, and bleed profusely till the conclusion of the operation, when the parts being allowed to return to their natural state it immediately ceased, and caused no further annoyance; and I am of opinion that a simple incision of moderate extent in a vein, parallel to its course, will rarely produce trouble, provided the vessel remains undisturbed, the contiguous parts are brought into contact with it, and the member is placed in a motionless and favourable position. Even a transverse wound, unless the slit is very large, will, I doubt not, do best when left to itself. The practice of tying wounded veins has the support of excellent authority, and is occasionally done on the face of stumps with impunity, as I have repeatedly observed; yet I am quite satisfied it should always be avoided till all other means have failed to arrest the bleeding—an event almost always attainable by patience and judicious management. A wound of the femoral vein, caused in passing the aneurismal needle round the artery, scarcely bleeds, as I have witnessed on two occasions, unless the openings are dilated by the elevation of the needle or ligature. I have had repeated proofs of this in the operations already alluded to. Whenever the parts were elevated by the needle, or by the ligature, the blood freely flowed; but the instant they were allowed to subside, it ceased. In a case of this kind it must be borne in mind that we have a double wound in the coats of the vein; for it is transfixed by the needle; and if the ligature is passed and tied, a segment of its circle is enclosed within the noose. This appears to me to be the cause of the fatality attending this occurrence. The injury of the vein is succeeded at first by adhesive inflammation, which quickly closes the orifices, and, were

not the ligature there, would suffice for its cure. But in a few days this action is extended, through the incessant irritation of the foreign body, passes the limits of safety, proceeds along the lining membrane of the vein in the direction of the heart, and gives rise to the first symptoms of constitutional disturbance. The ulcerative process then begins, the parts immediately surrounding the noose slough, and that portion of vein within it is cast off, leaving an opening in the vessel, through which dark unhealthy blood mixed with pus escapes, and the patient speedily passes into an irremediable state of typhoid phlebitis.

From what we experience in other wounds of veins, we are justified in concluding that the ligature is the whole and sole cause of this fatal phlebitis. And I am of opinion, if the ligature in every instance of the kind that has happened had not been applied, but the parts been brought together and treated as a common wound, the deaths ascribed to this accident would never have occurred. The removal of the instrument leaves a double injury of the vein, consisting of two small orifices, their long diameters parallel with the venous trunk. I consider that a case of this kind, judiciously treated, presents nothing in itself necessarily dangerous. The inflammation of the vein, induced by the ligature, is the cause of death. Nothing, I am therefore convinced, will excuse the practice of allowing the ligature to be applied upon the artery at the part where the vein is injured; and I should regard myself, were this misfortune to occur under my hands, guilty of the most culpable malpractice if I applied the ligature at this part, and allowed it to remain. The practice I would adopt in such a case is, to open the sheath of the vessels an inch above the injury in the vein, and tie the artery there, provided there should appear sufficient space between the wound and the deep artery of the thigh, to justify its application; if not, to go the same distance, or more, below. If the aneurism is in a state which would admit of the necessary delay, it becomes an inquiry whether it might not be better practice to allow the wound entirely to heal before the artery is exposed a second time. The only recommendation this course ap-

pears to present is the entire cessation of inflammatory action it would admit of in the vein, before the possibility of fresh disturbance could arise from the ligature. On the other hand, we must bear in mind the effect of a failure and second operation on the patient. These are circumstances, however, which each individual case and operation must determine. Future experience may decide whether the safest plan would be to pursue the treatment I am about to mention, but at present I am not prepared to give it unqualified recommendation.

In the second case of injury of the vein, alluded to above, though the plan I have proposed was present to my mind, I was withheld at the time, by motives of delicacy, from obtruding it upon my friend who was operating, as well as from a feeling that it was due to his judgment that he should have time to reflect upon a proposal so novel. There was only one opinion among those present—that the femoral vein was injured; consequently there was only one opinion, that the patient was placed in the most certain danger. With this conviction I thought I should be inexcusable did I not state to my friend, that if the case were my own, I should allow the ligature to remain till a sufficient time had elapsed to admit of a fair chance of the artery being obliterated, and then carefully remove it. In such a time I conceived the inflammation would not have exceeded a salutary extent; and the results of the experiments of Sir A. Cooper, Mr. Travers, and Mr. Roberts, on the temporary ligature, presented a fair chance of the proceeding being attended with success, as far as the aneurism was concerned. The advice was followed. In a fortnight the wound was quite healed, and the patient in perfect health. As it is the intention of my friend to publish his case, I cannot go farther into its details.

It would be difficult, perhaps impossible, to collect from a single instance stronger support for the position I have assumed than this case affords. Mr. Hodgson gives, in his work on Diseases of the Arteries and Veins, an interesting and instructive case, communicated by Mr. Freer, in whose practice it occurred, which seems to bear upon the point. The most violent constitu-

tional disturbance, following the application of a ligature to a varicose vein, ceased immediately on its removal. And though on the tying and instant removal of subsequent ligatures, similar alarming symptoms did not fail to arise, requiring active treatment for their reduction (which was accomplished with a favourable result), no one, I think, can doubt, if the ligatures had been allowed to remain, but that a very different termination would have ensued.

Your obedient servant,
SAMUEL HADWEN.

Lincoln, March 1841.

PTOSIS.

To the Editor of the Medical Gazette.

SIR,

IN Mr. Curling's communication on the cure of ptosis by operation, contained in the MEDICAL GAZETTE of March 26th, 1841, he remarks—"A somewhat similar operation has, I believe, been performed by Mr. Hunt, of Manchester, in a case of traumatic ptosis of one lid, consequent upon a division or laceration of the levator palpebræ muscle; but I am not aware of its having been practised in any case of paralytic ptosis." Since I published the account of the new method of operating for this disease,* which corresponds in principle with that adopted by Mr. Curling, I have not met with any other case in which it appeared desirable to resort to this means of cure. The following extracts will however prove that I then considered the operation applicable to instances of paralytic ptosis. After noticing the failure of the operations formerly adopted, the following observation occurs, to which I beg to direct Mr. Curling's attention. "Nor can the same operation be available in cases of ptosis caused by paralysis, or injury of the levator, when we consider that shortening the eyelid cannot restore to this muscle the power which it has entirely lost."† And again, nearly at the conclusion of the paper, "It may be well to repeat, that this method of performing the operation applies only

* North of England Medical and Surgical Journal, Nov. 1830, p. 164.

† Ibid. p. 165.

to the following causes of ptosis, viz.: those occasioned by loss of power in the levator, whether attributable to actual destruction of a part of the muscles, or to paralysis of the nerve supplying it, caused either by injury or disease."* Dr. Mackenzie, in his standard work on the Diseases of the Eye, remarks, "This method may also be useful when this disease arises from congenital deficiency, or from palsy of the levator."† And he concludes his section on paralytic ptosis by the following observation—"Mr. Hunt's operation may be tried in paralytic cases, when no signs of improvement appear. The epicranii‡ is active, depending on the nervous stimulus of the facial nerve, and the plan of bringing the lid under its influence deserves a trial."§

There is one objection to such an operation in paralytic cases, even although the power of raising the lid were restored by such a proceeding. In most of these instances all the branches of the third pair of nerves are paralyzed, and in consequence of the connection of one of them with the lenticular ganglion, from which body the ciliary nerves supplying the iris proceed, the circular muscular fibres of the iris generally become paralyzed, and a widely dilated pupil is the result. It is a question worthy of consideration, whether continued exposure of the retina to a greater quantity of light than usual, which must be the consequence of such dilatation of the pupil, may not materially interfere with the integrity of the functions of this delicate nervous structure; and thus lay the foundation for amaurosis.

Paralysis of the third pair of nerves may depend upon some altered condition of the vascular system of that part of the brain connected with the origin of this nerve, or with its course through the cranium, unaccompanied by structural disease; or upon some tumor, abscess, or other organic cerebral affection. I am induced to believe the former the more frequent. The difficulty of the diagnosis in the incipient stage is noticed by Dr. Mackenzie. If the paralysis is not confined to the structures supplied by the third pair,

but affects other parts of the face, or if the patient has been subject to fits for a length of time, as in the case reported by Mr. Curling, there is great probability that a slow structural change is going on within the cranium, and the prognosis is decidedly unfavourable as regards the fatal tendency of the disease. In such cases the operation may add much to the patient's comfort during the remainder of his life, if the objection of the exposure of the eye to light, with dilatation of the pupil, should prove groundless. Among the other cases I have not met with a single instance, either in private or hospital practice, which has not yielded to the medical treatment usually recommended, although, in many cases, three or four months elapsed before the entire removal of the disease. I fully agree with Mr. Curling in the propriety of not operating for strabismus in such cases. In the common forms of this affection the patient has the full power of commanding the direction of the affected eye when the sound eye is closed; but, in paralysis of the third pair, affecting only one eye, the closure of the sound eye produces no change in the patient's power of direction of the diseased eye; the cornea being still turned outwards by the abductor.

I am, sir,

Yours respectfully,

R. T. HUNT.

Manchester, March 30th, 1841.

ON DISTORTIONS OF THE CHEST AND SPINE IN CHILDREN FROM ENLARGEMENT OF THE ABDOMEN.

BY JOHN SNOW, M.R.C.S.*

(*For the London Medical Gazette.*)

It is not my intention to describe the various deformities to which the chest and spine of children are liable, but only to speak of one or two distortions which arise from enlargement of the abdomen—a cause of deformity which, hitherto, has not, that I can find, been recognised by authors.

I shall relate only one, out of a few cases that I have witnessed, which will serve sufficiently to illustrate the points that I wish to establish.

* Read at the Westminster Medical Society on March 13, 1841.

* Mackenzie, Practical Treatise on the Diseases of the Eye, 3d edition, p. 167.

† Ibid. p. 181.

‡ Occipito-frontalis muscle.

§ Mackenzie, Practical Treatise on the Diseases of the Eye, 3d edition, p. 184.

Aug. 16, 1839.—Hugh Lynch, a twin child, aged two years and five months, has had a double scrotal hernia from birth. He has been ill for the last four months; his mother says much in his present condition. The abdomen is very large and tympanitic; the chest is broad behind, very narrow in front, and flattened at the sides; the sternum projects forwards very much, especially at the lower end. The cartilages of the ribs, instead of passing outwards from the sternum on each side, leave that bone at an angle by no means very obtuse, and pass backwards to meet the osseous part of the ribs at another angle; and the cartilages of the false ribs project laterally from the bony portion, so that the lower part of the chest is much expanded where it unites with the enlarged abdomen. The last of the dorsal and first of the lumbar vertebræ project backwards, whilst the lower lumbar vertebræ project forwards and the sacrum backwards. The child is emaciated and feverish; its bowels are disordered, and its appetite is craving. Its mother feeds it chiefly on potatoes. The breathing is quick, the inspiration being easy, but the expiration difficult, and attended with an effort approaching to a cough; the upper part of the air-passages being closed after each inspiration, and then the air escaping with a slight sound, similar to that which takes place after the breath has been held for the performance of any muscular exertion. During each inspiration the abdomen descends and protrudes, and the cartilages of the ribs are forced inwards on each side of the sternum. During expiration, on the contrary, the abdomen retreats, and the ribs return to their previous situation. There is loud puerile respiratory murmur, and the chest yields a clear sound on percussion at all parts.

To take some Hydrargyrum cum Creta, and be fed in a more rational manner.

Sept. 27th.—The child is more emaciated. His belly is still large, but much less than before. His chest is of the same form, but the cartilages of the true ribs do not fall in so much during inspiration; those of the false ribs, however, are drawn inwards by the diaphragm at each inspiration, and so project towards the lower end of the sternum, whilst a hollow is left just beneath at the scrobiculus cordis.

The thorax yields a clear sound on percussion, but there is a mucous rale.

The child died on Oct. 29th.

Examination, seven hours after death — The spine is now pretty straight. Abdomen very much less than formerly, but yet tumid. The chest yields a dull sound on percussion throughout the greater part of its extent, although a few hours before death it sounded clear. On opening the abdomen, the diaphragm, instead of its usual arched form, is found to be stretched horizontally across the body, so that it is not so high as the seventh rib. The lungs are healthy in structure, but the whole of the left lung and the lower lobe of the right are collapsed and totally void of air, and gorged with dark fluid blood; the remainder of the right lung is crepitant and healthy. The heart is healthy, but the pericardium contains three or four drachms of serum. Each rib is enlarged into a spongy head at the part where it unites with its cartilage.

The large intestines are distended with flatus, except in portions where they are firmly and preternaturally contracted. The colon is so much lengthened that it crosses the abdomen three or four times. The cæcum is in the usual position, and from this the colon extends to the left side and back again, then passes upwards to the stomach, across the abdomen, and down the left side in the usual route to the sigmoid flexure; which flexure extends into the right iliac fossa, and back to the median line, where it unites with the rectum. The remainder of the abdominal viscera were healthy. The head was not examined.

In order to shew satisfactorily that this deformity of the chest is caused by the enlargement of the abdomen, I must prove that the space within the thorax is increased by any great distension of the abdomen, and not diminished, as is generally supposed. As the belly increases in size, the false ribs, with their cartilages, are pressed upwards, and approaching to a right angle with the spine; the circumference of the chest is thus increased, and the abdominal muscles, which by drawing down the ribs are the chief agents of expiration, can but ill perform their duty; they are kept on the stretch by the bulging out of the viscera, or whatever they enclose. Moreover, the diaphragm,

being attached to the base of the chest all around, has its borders drawn further apart by the increased circumference of the thorax, and thus its natural arched form is removed; it approaches to a plane partition, and the chest, so far from being encroached upon by the abdominal cavity in this direction, has its perpendicular length increased.

I may here state I have always observed that the difficulty of breathing arising from enlarged abdomen, whether in human beings or quadrupeds, consisted in obstructed expiration, and not obstructed inspiration, when there was no other cause of dyspnœa.

The bellies of children are subject to a very much greater proportional enlargement than ever obtains in the adult, and the cartilages and ligaments of the ribs being more flexible and distensible, the expansion of the base of the chest becomes very great; and as the lungs are compelled by the atmospheric pressure to occupy every part of the chest, they must either be preternaturally distended, or the chest must be depressed in some other direction. Now as the ribs and their cartilages are slender and flexible in children, the latter takes place; the chest becomes depressed laterally, and the sternum projected forwards, whilst a channel is left down each side of the chest where the cartilages unite to the bony portion of the ribs. The action of the diaphragm, which presses down the abdomen, and at the same time draws up the cartilages of the lower ribs, during each inspiration, makes room for more air than the lungs are inclined to receive, and the sides of the chest are pressed further in during each inspiration, and return again during expiration: thus the motion of the ribs becomes the reverse of the natural one. I do not conceive this arises from the mechanical resistance of the lungs, but from the sudden stoppage at the throat to the further access of air, which I have observed to be never absent during this deformity with inverted motion of the ribs. It is made evident by the slight explosive sound when the passage is again opened at each expiration. This check to the further ingress of air is no doubt a voluntary or instinctive effort to avoid the uneasy sensation arising from too great distension of the lungs. I do not think it consists in a

closure of the glottis, but in the approximation of the posterior palatine arches, and the pressure of the root of the tongue at the same time against the palate of the mouth: the method by which, as Dzondi has discovered, the breath can be held. It is the office of the serratus magnus and the pectoral muscles to expand the sides of the chest during inspiration, but the diaphragm enlarging the chest more powerfully in another direction, these muscles yield to the atmospheric pressure, and eventually, so far as respiration is concerned, become paralysed.

I have never seen enlargement of the abdomen to great extent in a child under three years of age, that was not accompanied with this deformity of the chest. The degree of deformity is always in proportion to the enlargement; and in observing any individual case, the deformity is found to increase with the increasing size of the abdomen. Even when the enlargement of the belly is not great, there is a tendency to this deformity observable in the slight lateral projection of the cartilages of the false ribs. After the age of three or four years I have not observed this deformity to commence, probably because the ribs become of a strength which prevents it; besides that the abdomen is not so liable to become tumid.

The other deformity, that of the spine, seems to be only an occasional, and not a constant attendant on enlarged abdomen: I think it is in the worst cases that it prevails. It consists in a projecture, frequently an angular one, of the last dorsal, or great lumbar vertebræ, or both. In the case I have, just detailed it was accompanied by a secondary projecture of the sacrum; and in another case it was attended with a slight lateral deviation. This projecture of the spine is probably caused by the stretched abdominal muscles and integuments, drawing, by means of the pelvis and chest, on the opposite ends of the spinal column, whilst the increased contents of the abdomen make a resistance in the centre. That this angular projecture of the spine depends on disease of the bodies of the vertebræ in the first instance, we are forbid to suppose, by the fact that the projecture subsides as the abdomen diminishes.

In one child, an angular projecture

for which the formation of issues had been previously recommended, perfectly disappeared as the child resumed his health, under tonic and alterative medicines, with attention to diet, and bandaging of the abdomen. It most likely depends on partial absorption of the intervertebral substance, or bending of the bodies of the vertebræ at the affected part, and, if continued, might no doubt lead to permanent disease.

Baron Dupuytren, in a paper published in the "*Repertoire Général d'Anatomie et de Physiologie*," in 1828, described a deformity of the chest of children, which I believe, in many of his cases at least, to be the one of which I am speaking. It occurred in children badly clothed and fed, born of unhealthy parents, and living in damp situations, and was accompanied, in most cases, by enlarged tonsils, requiring sometimes to be extirpated. He said that there was a keel-like projection of the sternum in front, and a sharp prominence of the spine; that the ribs were not only flattened, but that they were sunk into the chest as if they had been compressed from one side towards the other. This deformity was accompanied with great difficulty of breathing. He did not mention the abdomen, except in his preliminary remarks, to say that there was a projection forwards of the sternum and belly; and he only alluded to it, I think, in one of his illustrative cases, in which, however, he said the belly was five times as large as the chest. In another case he spoke of the width of the chest at the base. He appeared to attribute the deformity to arrest of ossification, and softness of the bones; and recommended some mechanical measures and exercises, in addition to medical treatment, for its cure. He spoke of some cases in new-born infants, which, I think, could not have the same origin as the cases I have seen. The nature of the connection between these deformities and the enlargement of the tonsils, he could not tell.

I have only seen one case since I read Baron Dupuytren's paper, and in this there was no enlargement of the tonsils. I can, however, suppose that the obstruction to inspiration from enlarged tonsils, might cause the ribs of a child to be pressed inwards by the

atmosphere, and produce a deformity of this kind.

In the *MEDICAL GAZETTE* of January 12th, 1839, is a letter by Mr. Rees, describing the deformity of the chest of which we are treating as occurring in four or five children seen by him at the Tower Hamlets Dispensary. He describes the depression at the line of union between the ribs and their cartilages, producing a channelled appearance external to the sternum on each side; and he likewise describes the inverted action of the ribs in respiration. Mr. Rees does not mention the abdomen, except in the case he relates for illustration, in which he incidentally says it was tumid. He attributed the deformity to chronic pneumonia, which causing the lungs to shrink and become solidified; the ribs are forced in by the atmospheric pressure to occupy the space. We can perceive that shrinking of the lungs might have this effect, but in the cases I have witnessed there was no disease of consequence in the lungs, except in one child that had hooping-cough.

Mr. Amesbury describes this deformity, and the enlargement of the abdomen in connection with it; but he does not speak of them in the relation of cause and effect. He attributes this deformity to weakness of the muscles. He says, "Deformity of the chest, arising from weakness of the muscles, commonly takes place during teething, but may be produced by any complaint that tends to debilitate the system. This distortion usually assumes the form called "chicken-breast." In this variety of deformity the chest is usually more or less contracted laterally, the sternum is thrust forward, and the abdomen is preternaturally enlarged. The intercostal muscles act very little in respiration, the breathing being principally abdominal." Together with medical treatment for the improvement of the general health, Mr. Amesbury recommends bandaging the abdomen for the cure of this deformity.

Baron Dupuytren and Mr. Amesbury do not mention the reversed action of the ribs in breathing. Mr. Rees and Mr. Amesbury do not allude to any deformity of the spine; and the projection to which Baron Dupuytren alludes appears to be one of the whole spine, and not of particular vertebræ.

In two or three of the cases I have seen, the inferior extremities became deformed under the weight of the enlarged belly, but it was the joints which yielded, and not the shafts of the bones; and there was only in one case the enlargement of the heads of the bones peculiar to rickets: where rickets exists it may assist to aggravate the deformity, but cannot of itself cause it. A scrofulous diathesis, by predisposing to mesenteric disease, may be considered favourable to the development of these deformities; but in the cases I have seen, I do not remember to have observed enlarged glands, or other decided marks of scrofula. And in the only two cases in which I have had the opportunity of an examination after death, there was no disease of the mesenteric glands, and the enlargement depended chiefly on the elongation and distension of the colon.

I have generally been able to trace the increased size of the belly to improper food, and have found it mostly amongst those infants who, from the poverty or intemperance of their parents, are, after weaning, fed almost entirely on potatoes. I believe the best treatment to consist in alteratives and tonics, with occasional purges, and the careful avoidance of crude and indigestible food. At the same time the abdomen should be firmly bandaged, as recommended by Mr. Amesbury, which measure, whilst it assists to reduce the belly to its natural size, will relieve the breathing by aiding the efforts of expiration; and by pressing the diaphragm upwards, will reduce the capacity of the base of the chest, and thus lessen the cause of the contraction higher up. If this deformity be left to itself, and should not prove fatal, the serratus magnus, and other muscles, may not recover their power of expanding the ribs, or the ribs may have become firm in their abnormal shape, and the deformity may continue, after the enlargement of the abdomen, which gave rise to it, has subsided. I have seen two or three cases of this deformity in grown-up persons, which, so far as I could gather the history, appear to have originated in this manner.

Thus, then, I have endeavoured to establish that enlargement of the abdomen in children leads to deformity of the chest, and occasionally of the

spine; and that although the deformity of the chest, or one nearly resembling it, may now and then arise from other causes, yet that this enlargement is of itself sufficient, and will never fail to induce it, if proceeding to any great extent in young children.

CURE OF STAMMERING.

To the Editor of the Medical Gazette.

SIR,

ON the evening of Monday, the 8th instant, I transmitted to you a communication on the cure of stammering, but it appears to have been too late for your number of yesterday, which, however, contains two interesting letters on the same subject. These papers are valuable to me, as conveying statements which can be relied upon regarding the mode of practice and views of Professor Dieffenbach and Mr. Yearsley, both of whom stand high in professional estimation. They also settle the question of priority in the performance of the operation, which I was anxious to know. It appears Mr. Yearsley's first operation was in December last, and that of Dieffenbach on the 7th of Jan., 1841, while my first operation for stuttering was performed in September last, being about three months prior to theirs. As noted in my letter of the 8th instant, it is a curious circumstance that we should have been prosecuting the same subject about the same time, and with somewhat similar views, without the knowledge of what each other was doing.

Mr. Yearsley's notion as to the cause of stammering, and the mode of cure, are very nearly the same as my own; but not entirely so; and after an attentive perusal of his paper, I see nothing in it which can lead me to alter my opinion, as stated in my letter of the 8th, namely, that stuttering is caused by the glottis being prevented from rising sufficiently to admit the quantity of air required, and acting with that promptitude necessary to secure free and correct enunciation. Mr. Yearsley, on the contrary, conceives that *no* air is allowed to escape from the chest; and that there is besides an impossibility of any passing forward into the mouth, from the absolute closure of the anterior fauces by approximation of the

dorsum of the tongue and palate. I think it will be found that there is always a certain quantity of air admitted into the mouth of the greatest stammerer, although not enough to enable him to speak correctly and freely; and that the pressure of the lips causes this air to react on the epiglottis and root of the tongue, so as to cause a still farther closure of the larynx and difficulty of egress of air. In proof of this I may observe that stammering is most apt to happen when the labial or palatine consonants occur; the former from the action of the air just stated, the latter by the pressure of the tongue against the palate, tending to produce the same effect. The fact of stutterers being able to sing what they cannot speak, seems to favour the same view; and both Dieffenbach's, Mr. Yearsley's, and my own mode of operating, tend to secure the same freedom of opening of the glottis; the former by drawing the root of the tongue, and of course the epiglottis, upward and forward; the latter by removing the obstacle to its free opening, in allowing it to rise by removal of the enlarged tonsils, and its advancing forwards by removing the bridling of the tongue by division of the frænum, or preventing the titillation of the enlarged uvula, stimulating that ever watchful sentinel, the epiglottis, to too vigorous exercise of its functions of guarding the entrance of the glottis from all impertinent intruders.

As to the mode of operation, that of Dieffenbach is evidently and avowedly a formidable one. Mr. Yearsley's and my own are comparatively simple. For the division of the frænum linguæ I use a pair of sharp-pointed knife-edged scissors; and although I have had occasion to cut so freely as completely to expose the blood-vessels in that situation, I have never had more than a tea-spoonful or two of blood from any single operation. For the excision of the tonsils I observe Mr. Yearsley uses a tenaculum and scalpel; but I have no hesitation in saying that the instrument I use for the purpose is one which will effect the operation with much greater exactness, celerity, and safety. I regret that I cannot name, at this moment, the inventor of this ingenious instrument; as I would wish to award the full mead of praise to every man who makes an improvement in any

department of our profession. It was invented about two years ago, I think, by a London practitioner, for excising the tonsils, and consists of an oval ring, with an oval concealed blade, attached to a handle, with finger-bowls to hold it with the more steadiness. The flat edge is pressed along the tongue, and is then turned and pressed laterally, so as to cause the enlarged tonsil to protrude through the ring, when it is to be secured in that situation by pressing onward a sliding skewer attached to the handle of the instrument, when the handle of the cutting blade is pulled forward, and of course cuts off that portion of the tonsil which has protruded beyond it. The pillars of the palate are effectually protected from injury by the sheath in which the blade is placed, and there is no risk of injuring the internal carotid by any struggling of the patient, which might occur in the other mode of operating with the tenaculum and scalpel bistoury, especially in young patients. If the tonsils are very little enlarged, but still so much so as to require excision, from their relaxed state, and might not protrude through the ring so as to be readily secured by the skewer referred to, it might be assisted by a tenaculum, to extend it whilst cut by the blade, as in other cases. The instrument I prefer for amputating the uvula is a pair of scissors rounded at the point, so that they cannot possibly injure the posterior fauces, and a pair of forceps, such as those used for the extraction of polypus, which at once serve the double purpose of commanding the tongue and uvula during the division of the latter by the scissors.

I can bear testimony to the correctness of Mr. Yearsley's statement of the improvement of the voice which occurs immediately after the excision of the enlarged tonsils of those who are not stammerers, but articulate badly from enlargement of these organs. Where it has been husky and guttural in a high degree, the change is so great and immediate, that it could scarcely be credited by those who had not heard it; but I have so often performed those operations, that I consider it may be regarded as a certainty, if properly performed, and the cases have been judiciously selected. In my former letter, when stating the remedies I had found useful, where stammering, hawking, and

coughing, were occasioned by a relaxed state of the fauces, and which, of course, are always associated with an irritable condition of the mucous membrane, I omitted to name the nitrate of silver, either in substance or solution, as one of the most important remedies.

I am, sir,

Your obedient servant,

JAMES BRAID.

10, Piccadilly, Manchester,
March 13th, 1841.

CASE OF
POISONING WITH GUN-BARREL
"BROWNING."

To the Editor of the Medical Gazette.

SIR,

A FEW months since, as I was walking through the streets of this town, in company with Mr. Atkinson, surgeon to the Union, a respectable tradesman of the name of Beckley rushed up to us with a death-like countenance, in a state of the utmost trepidation, exclaiming that his "son was poisoned." We immediately proceeded to his residence, and found a child, about five years old, sitting upon its mother's knee, making ineffectual attempts to vomit. He complained of a constant burning pain at the epigastrium, a peculiar sensation in the throat, and the pulse was feeble and quick. He looked listless and heavy, but was perfectly rational, replied promptly to questions, and was extremely obedient in adopting the measures of relief which we recommended.

It appeared that his father, who is a gunsmith, had left a bottle containing "browning" within reach, and, during his temporary absence, the child had swallowed a considerable quantity of its contents. Gun-barrel browning is composed of bichloride of mercury, sulphate of copper, and the tincture of the sesquichloride of iron; a precipitate is the result of the union of these heterogeneous ingredients.

The first object was to neutralize the bichloride of mercury, if any existed in a free and uncombined state; to effect which, copious draughts of milk, combined with whites of eggs, were administered. The successive doses, owing probably to the emetic action of the sulphate of copper, were ejected after

short intervals, in a partially coagulated state. The poison having at length ceased to act upon the stomach, or the organ having become insensible to its action, a slight ipecacuan emetic was given, followed by draughts of milk: vomiting was renewed, the pulse became more natural, a gentle perspiration broke out, and the child was taken to bed, where he soon fell asleep, and awoke convalescent.

REMARKS.—The quantity of corrosive sublimate swallowed could not have been very considerable, as a pint of the mixture contained only about a scruple of the salt, with the same weight of the sulphate of copper. All authors, however, agree in assigning dangerous properties even to a minute dose. Orfila, Campbell, and Smith, state that three, four, or five grains, will cause death in the course of the second, third, fourth, or fifth day. Irritation of the alimentary canal, the first symptoms of poisoning, had been clearly induced, and was as manifestly allayed, almost instantly, by the antidote. Professor Orfila has related many satisfactory experiments in proof of the virtues of albumen in destroying the corrosive properties of bichloride of mercury, and converting it into the comparatively harmless albuminous protochloride. Peschier considers that the white of one egg is sufficient to render four grains of the poison innocuous. Dr. Christison also remarks, that the prompt exhibition of albumen, just as the symptoms of uneasiness appear, prevents further ill consequences.

With regard to the salts of copper, Orfila found that albumen is also the surest antidote to their poisonous qualities, and his experiments have induced him to recommend that substance in preference to any other. He found that when 25 or 36 grains of verdigris were mixed with the whites of six eggs, the poison which, if pure, and administered alone, would have caused death in three hours, did not produce it for seven days; and had no effect whatever for five days, although in some experiments the œsophagus was tied, to prevent its expulsion.

I have the honour to be, sir,

Your obedient servant,

R. H. ALLNATT, M.D.

Wallingford, March 30, 1841.

PERFORATION OF STOMACH.

To the Editor of the Medical Gazette.

SIR,

I SHOULD have been glad that a communication from my pen, likely to be the last, had been of some utility in relation to the cure of disease, rather than the record of an unusual and distressing case. It may, however, be of some service to have put down accurately the symptoms which attended such a case, and will probably designate all similar ones, in order that none concerned may be taken by surprise, encouraged by false hopes, or led to adopt what is called active treatment, which can but accelerate the fatal event.

Cases of perforation of the stomach or intestine are already on record. I have quoted, as I recollect, in my lectures, as many as ten or twelve; but it has not fallen to every one's lot to have seen, as I have, or been intimately acquainted with two. I have no means here of consulting a single book on the subject, nor indeed would it be to much purpose. The general fact is, I think, that the accident takes place always without warning; never by laceration, but by the giving way of a small insulated ulcer, which is found probably with callous edges, and of so small a size that a common drawing pencil would just, as in the present case, pass through it. Of course I have not to learn that perforations of the intestine occasionally happen in fever and other acute diseases; but these cases of perforation of the stomach are of a totally different character, and their awful peculiarity is, that they happen in the midst of health.

I cannot say how many years ago it is, but it chanced that at the very time when I was lecturing on the various maladies of the stomach—for which a text is supplied by those two unfortunate titles, gastritis and dyspepia—a letter was put into my hands from my son, the late lamented Dr. John Badham, relating the fate of a young lady with whom he was intimate, who had parted with him and others one evening after a ball in florid health, and was almost immediately attacked with atrocious pain in the belly (which was, of course, mistaken as to its nature), and went on without a moment's palliation till she died, which happened in

about twenty-four hours. On examination it was found that a small ulcer, evidently of some standing, had been developed from within, and proceeding from the mucous coat to the peritoneal, had at length given way. It was this distressing case that led me immediately to consult authorities which were then at my hand, while the case itself made an indelible impression.

Dr. Cox, a gentleman who is settled at Naples, requested me, a few days since, to see with him a case of alarming character. He had been summoned at ten the night before to a young woman in dreadful suffering from abdominal pain, with which she had been seized at four in the afternoon (at three she was still in perfect health). After careful examination he had no hesitation in considering the case as enteritis. Accordingly she was largely bled, but with very small relief; and as things continued in the same state, a few hours later this practice was repeated, and thirty-six leeches, with fomentations, &c. &c. were also applied.

I went with Dr. Cox about eighteen hours after the attack; and the following were my notes:—

The pain had begun under the false ribs of the left side, quite suddenly, but soon became abdominal. She lies with her knees up, and cannot turn ever so slightly without great aggravation of pain. Pressure cannot be endured for a moment, but still there is no great tension or tympanitic resistance—no probability of the existence either of air, or fluid effusion.

She has great and incessant thirst; but, notwithstanding all she drinks, no action of the kidneys: she has passed urine but once, and in small quantity, since the seizure.

The bowels had acted but the day before, and therefore could not be considered as obstructed.

The tongue has no remarkable character, neither of redness nor coating of any kind; there is a light brown speck down the middle, and to the finger it is dry, but not remarkably so.

The pulse is exceedingly small, rapid, and difficult to be felt.

There neither is, nor has been, any sickness or vomiting; and even nauseous medicines have been retained.

The blood has no appearance of a buffy coat.

Her intelligence was perfect; she was, she said, a stranger to illness of any kind; had but lately arrived in Naples, had taken no cold, but for a short time had cared rather less for food.

I saw her again a few hours later, when her sufferings were intolerable, and before next morning, thirty-eight hours from the attack, she expired.

The remaining report is from Dr. Cox. "Intestines acutely inflamed. Ulceration opening near the cardiac extremity of the stomach, and on its greater curvature; its edges smooth and indurated, and evidently but of recent formation. (Had often made mention of pain or uneasiness on her left side.)"

The rest is of small importance. In conversation with Dr. Cox I had ventured on the whole so far to differ from him, as to be inclined to consider the disease rather as of the investing membrane than of the bowel. I had often seen peritonitis assume a very ambiguous character, but it was not till after the death of the unfortunate patient that the case mentioned above occurred forcibly to my memory; and accordingly I addressed a note to Dr. Cox, stating this case, and adding, that as, either on his supposition or mine, death could not well have occurred so soon, it was really important to ascertain the state of the parts.

The weather at Naples has been, and that for the whole winter, so bad, that a patient could scarcely be worse circumstanced: a fine day has only been occasional. Those who have been sent to Nice must not expect any thing better in the climates of any parts of Italy.—I remain, sir,

Your obedient servant,

CHARLES BADHAM.

Naples, March 7th, 1841.

MEDICAL GAZETTE.

Friday, April 9, 1841.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

SIR CHARLES BELL ON MEDICAL REFORM.

IN times of civil contention, said the ancients, it behoves every citizen to

take his side, and not affect a neutrality dangerous to the state; and now that our medical republic is so sorely distracted by fears and jealousies, we hail with pleasure each document which gives the deliberate opinion of some man of note.

Sir C. Bell, whose pamphlet is now lying before us*, comments on the two bills which have attempted to apply a salve to our professional grievances; and, like all other persons, he condemns them. When we *do* agree, as Sheridan says of stage concord, our unanimity is wonderful; and the bills for improving the medical profession have reconciled the most violent antagonists, and, like some great public calamity, have made all other misfortunes appear trifling by comparison.

Sir C. Bell, however, though utterly rejecting the little medical parliaments, and all the other queer devices proposed to make us happy, is by no means perfectly satisfied with things as they are. He is a reformer, but then it is after his own fashion; he does not come into the field with any well-known uniform on his back, but fights like a partisan, in plain clothes, and does not draw up with the regular troops.

The consequence of this is, that he cuts right and left without much mercy; old corporations and new reformers share his censure, and his motto seems to be

Tros Tyriusve fuat, nullo discrimine agetur.

Sir Charles warns the members to whom his pamphlet is addressed against the bias arising from local prejudices; and so far he does well; but when he begs them to reflect that in London they are "exposed to the influence of men who form their judgment on very limited experience," and "who have

* "A Letter to the Members of Parliament for the City of Edinburgh, on the two bills now before Parliament for the Improvement of the Medical Profession. By Sir Charles Bell. 1841."

not hitherto taken an enlarged view of this matter," we think that, in phrenological language, his organ of caution is too strongly developed. The members for Edinburgh, like all other M.P.s, have in London the opportunity of learning the wishes of medical men of every calibre; from the dashing practitioner who clears his 3 or £4,000 a year, down to him whose scanty revenues are not to be mentioned to ears polite—every shade of opinion is daily unfolded to our legislators. These varied sentiments cannot be attributed to local prejudice; they are not the unmixed growth of the London district, nor alien to the feelings of men born in Hants or Salop, but the calm representation of English opinion elaborated by long discussion and experience, and expressed in a kind of national congress. Far from London being under the influence of local prejudices, it seems rather to approach the undefined line where impartiality borders on indifference; so that metropolitan sluggishness occasionally forms a curious contrast with provincial vivacity. In fact, London, instead of being the heart of the empire, as it is often called, has sometimes seemed to us more analogous to the brain—the source of sensation to every other part, but itself almost destitute of feeling.

Did we find, for instance, that Mr. Warburton's Bill, that bundle of whims and oddities, was discussed in the capital with heat and agitation, as a thing likely to annoy the profession extremely? or, was it talked of, like the fortifications of Paris, as a strange freak that nobody need mind? Does Mr. Hawes's bill, again, fill us all with perplexity and terror? We can assure Sir Charles that every one waits for its extinction with the most philosophic composure, and that scarcely an epigram will be written on the occasion.

Sir Charles tells the story of Walter

Scott finding Mungo Park throwing stones into the pools of the Yarrow, that he might learn to judge of the depth of the rivers in Africa by the time the bubbles took to rise from the bottom. Park was at this time a country surgeon, with a practice of £150 a year, and "rather than suffer the privations of his condition, he was contemplating a perilous adventure." Mungo Park's talents clearly did not lie in the country practice line; "to sit by the side of a groaning woman till dawn," as Sir Charles phrases it, was not his *forte*; and he did well to give up the Yarrow for the Niger. This anecdote scarcely proves, as it is intended to do, that the income of country surgeons is so small that the legislature must, at all events, refrain from increasing the expense of their education. The case is clearly an exception; and we have no doubt that Mungo Park's successors have made larger incomes on the same ground: but *non omnia possumus omnes*; and the man who was destined to immortality as a traveller, was unable to grow popular amongst the farmers' wives on the Yarrow.

Nevertheless, we agree with Sir Charles Bell, that some of the studies which adorn the Corinthian capitals of the profession may safely be dispensed with in the case of ordinary practitioners, in spite of the crotchets of the one-faculty men.

How are these things managed in other countries? Why, they found professorships, and teach medicine gratuitously, or very cheaply, lest the poorer parts of the kingdom should lack advice. But would any one make our students "paupers depending upon the state?" No one, we should hope; neither the author of the letter, nor we, at any rate.

But our agreement with him in this point makes us differ from him when he laments the sums levied on the "poor students" in the University of

Edinburgh; for the prodigious superfluity of M.D.s yearly sent forth by this body is almost a demonstration that the fees by no means amount to what merchants call a prohibitory duty. Sir Charles says, that in his office of Dean, he has of late years "exactd from their pockets eleven hundred guineas annually," which go to government. The newspapers often tell us the number of oxen and sheep consumed every year in London; and the tale appears immense till we recollect that when divided among a million eaters, the quotient, or ration of beef and mutton for each mouth, is not very large: just so, if the eleven hundred guineas are extracted from five or six hundred purses, the tax on each will not be heavy. At the same time he is perfectly right in disapproving of the number of different lecturers whom the student is obliged to hear, and of the protraction of minor subjects through six long months. Far better would it be to go over the more important topics a second time, and spend more hours in clinical instruction, and in attending cases under skilful superintendence. This would be a great improvement, but would be far from satisfying every one; for a social problem remains to be solved, compared to which the riddle of the Sphinx was a joke. It is this: to devise a distinction so easy, that every capacity may earn it; so cheap, that every fortune may buy it; so quickly won, that the slenderest patience may wait for it; yet so brilliant, that, like the coat of a clergyman or an officer, it may be a universal passport in society.

The legislature, complains Sir Charles, when they wanted to pass a Medical Act, sent the inquiry to a Committee, "and then there is a farce of taking evidence and receiving the statements of corporate bodies, all tainted with the imperfections of a rude age."

But surely the Professor would not have rejected all evidence save that of the malcontents; and after the Committee had heard the statements of frail contented creatures, they must have hastened with a higher zest to evidence untainted by imperfection; for none was excluded—good was allowed admission as well as bad. Perhaps the following sentence contains a nearer approach to the marrow of the question:—"The radical defect being the attempt to legislate for a profession, to reconcile the interests of medical men, instead of looking steadily to the interests of society."

In rude ages, or even now in some continental countries, it may be advisable for the supreme power in the state to meddle with the details of ordinary life. Catharine of Russia issued regulations for the conduct of evening parties; but though it might be easy to prove to the satisfaction of a Committee that for want of a ukase, ices melt, and lemonade grows lukewarm, no one has yet advised us to copy the Muscovite model.

In Malta, again, our government has fixed a tariff for medical and surgical advice, and while venesection is recompensed by six taris, or 10½d., the reduction of hernia claims six times as much*.

In this country, however, we leave the settling of the tariff to doctor and patient, with probably less dissatisfaction in the main, than on the straight-laced system intended to prevent all disputes.

Some few and obvious improvements might be safely attempted. First, sweep away the minor examining bodies. Secondly, let each remaining board qualify the successful candidate for practice throughout the British empire. Thirdly, on all occasions where the state comes in contact with

* Hennen's Medical Topography of the Mediterranean, p. 544.

our profession, let a more liberal scale of remuneration be adopted: it is degrading to the profession, and therefore injurious to the public, that the services of an assistant-surgeon in the army, or of the medical officer of a Union, should be rated at less than those of a tolerable clerk in a house of business. These are points with which the State may advantageously interfere. But we would warn legislators and reformers that the customs of a nation cannot be suddenly changed by Act of Parliament. The very attempt is painful. The fable of the shrubs which, when pulled up, dropped blood, and uttered lamentable cries*, is but a type of the uneasiness which such attacks occasion. Old habits are found to possess life as unexpectedly as the myrtle-trees in Virgil, and to part from it almost as reluctantly.

A very delicate point touched upon by Sir Charles is the indifference of the College of Surgeons to the moral influence of the profession. He complains of their receiving certificates from anatomists of irregular habits, and observes that during his thirty years' experience of teaching in London, hundreds of young men entered the profession with the worst examples before them. But what would have been said of the College, had it attempted an inquisition into the morals of lecturers, or refused a certificate from a teacher, because, though a good anatomist, he was a free liver? The only solution that we can propose for this dilemma is the establishment of medical colleges, as we set forth in our last number, where each professor would be in some measure a sponsor for the character of the others. As it is, there is no doubt that even the study of the human frame (which Galen called a hymn to the Deity,) may tend to blunt the finer feelings of our nature†.

One more point, and we must have done—at least for the present. Sir Charles Bell complains of the small rewards bestowed on the exertions of practitioners of physic, compared with those lavished on other learned professions. So far he is right: no medical practitioner occupies in English society the station of a bishop or a judge. But when he goes on to say that there is no worthy tribunal to judge the merits of our profession, and that all success depends on ignorant rumour, or the prejudice of fashion, we are compelled to differ from him. Indeed, we may quote his own example against his own theory. In any scientific association, from the Neva to the Tagus, when the nerves become the subject of discussion, the name of Bell is quoted as an authority. Is not this fame? Can the most brilliant leader on the northern circuit, or the deepest equity judge, go higher? Our utmost exertions, says Sir Charles, can gain no more than the gratitude of a person subdued by sickness, whose opinion is farther questioned by old ladies round the tea-table. Here, again, we think the Edinburgh professor too hypochondriac, and would defy all the ancient dames in the kingdom, inspired by the strongest Howqua, to wither one leaf of his laurels.

We could have wished for something less disjointed and more detailed than this letter, from a man of genius like Sir Charles Bell; yet, such as it is, it will be perused with advantage, and will supply its readers with larger materials for thinking than many more regular disquisitions.

per feeling, extinguishing religious, as well as natural sentiment, there are none like a dissecting-room. Nothing more necessary, more profoundly useful than dissecting; and no place so productive of evil to youth, when uncontrolled by the presence of a respectable teacher and head, as a dissecting-room."—Sir C. Bell. "A Letter," &c. p. 13—14.

* *Æneid*, lib. iii.

† "Of all the scenes destructive of every pro-

PARIS SURGICAL PRACTICE.

No. II.

To the Editor of the Medical Gazette.

SIR,

FRACTURES of the inferior extremity, when not treated by the *appareil amidoné* in the Paris hospitals, are still for the most part put up with the limb in the straight position, and resting on its back, in a sort of junk formed of a piece of cloth and two wooden splints, not more than two and a half or three inches wide; bags, or cushions of chaff, of an equal length with the splints, being interposed between them and the soft parts. A third splint and cushion is placed on the anterior part, and the whole tied together, the foot being frequently without proper support. This apparatus is not suited to cases in which inflammation or much swelling exists, as it keeps the part hot, and prevents the application of cold water irrigations, which are now frequently employed in Paris. Neither is it well adapted to compound fractures, as tending to favour suppuration. When a wound exists, it is generally dressed with charpie, and the perforated linen rag smeared with ointment; or, if extensive, and suppurating freely, with poultices. During the dressing, or the reapplication of the apparatus (which frequently becomes loosened), sufficient attention is not paid by the dressers or assistants to support the limb; only one hand, perhaps, being applied to the knee, and none to the lower part. But little general treatment is adopted, except bleeding if there should be much accompanying fever. In fractures of the thigh a long narrow splint extends the whole length of the limb, from the hip to the foot. A second splint on the inside also extends to the foot; and a third short one is placed on the anterior part of the thigh, without any means being adopted for keeping up due extension; and as the apparatus is frequently slack, I should think that shortening of the limb is a very common occurrence, having myself seen some instances of this. Fractures of the upper part of the bone, about the trochanter, or of the neck, are generally treated by Boyer's, or some other apparatus, by

which efficient extension may be made. When the last kind of fracture occurs in old people, the plan of Sir A. Cooper is frequently acted upon, viz. of not applying any apparatus, but merely of keeping the patient in bed for ten days or a fortnight.

Amputations of the leg are frequently performed very low down, in order to enable the patient to wear an artificial foot. This succeeded very well in two boys on whom M. Velpeau operated during my recent visit; and in one of them, when the patient walked, many persons could not tell, without great attention, on which leg the operation had been performed.

An amputation of the leg was performed in February, by M. Roux, on a man 65 years of age, of good general health, but labouring under depression of spirits, affected with gangrena senilis. The foot and toes were cold, and completely discoloured, and a distinct line of demarcation existed on the leg, about five inches from the ankle. The patient, however, did not survive the operation more than five or six days.

The question of the propriety of amputation in cases of mortification of the extremities has frequently been agitated; and it is now pretty generally admitted among the profession, that as far as concerns gangrene resulting from wounds or external causes, amputation is frequently advisable; but the great majority (and I may say almost all in this country) are averse from recommending it in the idiopathic kind, though patients have subsequently recovered; and of those who have succumbed, a large proportion would in all probability have died if the parts had been left to separate by the effects of nature. It may therefore fairly be questioned, whether, when there are no other counter-indicating circumstances, the operation might not in some cases be preferable to the chances of recovery afforded by the patient waiting, during months of suffering and confinement, and breathing an atmosphere tainted by the presence of a loathsome disease, for the tedious process of separation of the dead parts from the living.

An operation was likewise performed by M. Roux, about the same time as the preceding one, for the removal of a cancerous tumor, near the size of a pullet's egg, occupying the

left side of the base of the tongue. As its extirpation by the mouth was impracticable, M. Roux performed an operation similar to the one which was successful in the hands of Professor Regnoli, of Pisa, two or three years ago, which consisted in drawing the tongue out of the mouth beneath the lower jaw, through a wound of the soft parts, formed by a longitudinal incision extending from the symphysis to the os hyoides, and two lateral incisions along the edge of the jaw-bone. M. Roux, however, considering that there would be less likelihood of the collection of blood and matter if the lips of the wound were in a more depending position, made his incision through the skin, cellular texture, and platysma myoides, commencing anterior to the facial artery, where it crosses the bone on the left side, carrying it in a semicircular form down to the os hyoides to terminate at the corresponding point on the right side. The muscles were then divided by a sharp-pointed bistoury, being passed into the mouth from below, and the flap being held aside, the tongue was seized with the *pincers de Museaux*, and drawn through the aperture, by which the whole of the tumor was exposed, and excised with comparative facility; though the operator was obliged two or three times to wait a minute or two, on account of the difficulty of breathing, and tendency to syncope, experienced by the patient. The remainder of the tongue having been replaced in the mouth, the edges of the wound were united by suture and adhesive plaster, and the patient was conveyed to bed. He did not, however, survive more than three days. The quantity of blood lost was not very great, and was chiefly from the branches of the lingual artery.

In any similar case, would not the section of the soft parts, from the angle of the mouth to the ascending ramus of the jaw, be a preferable operation, and one attended with less danger than that above described?

Your obedient servant,
EDWIN LEE.

38, Golden Square,
March 31, 1841.

SUDDEN DEATH.

To the Editor of the Medical Gazette.

SIR,

It affords me much pleasure in having it in my power to answer the query of your correspondent, "Inquisitor," contained in your last number, "as to what is to be done in a case of sudden death." Without entering upon a consideration of the various causes producing sudden death, we may, I think, divide the states in which we generally find the individual into two, according to the symptoms presented. In the one we have the quick, strong, resisting pulse, noisy respiration, *warm extremities*, frequently general perspiration, flushed countenance, with some spasmodic muscular action: in this case bleeding is evidently strongly called for. In the other case, on the contrary, as stated by "Inquisitor," with *cold extremities*, cold clammy moisture on the surface, slow and laboured respiration, with feeble, or total absence of radial pulsation, and the other symptoms of depressed vital action (though far from always indicating a ruptured vessel.) In such a case, indeed, it is both "unscientific and malpraxis," and I may add unjustifiable, to bleed the patient. Most assuredly is it "*infra dignitatem*" for any member of so liberal and enlightened a profession as the medical, for one moment to think of so pandering to the ignorance of the crowd collected, or of the jury at the inquest, as to detract blood; and acting not only against knowledge and conscience, but also preventing, by so doing, the only chance of recovery.

In such a case I would briefly but clearly explain my views to those around. I would assure them that to bleed would be certain death. I would recommend the immediate exhibition of diffusible stimuli, and application of warmth to the extremities; and if still urged by the by-standers to bleed, I would positively refuse, and leave the case open, if wished, to a second opinion.

In the event of being quite extinct, I shall only add, that the performance or non-performance of the operation being of no moment, venesection, in order to satisfy the public that "all has been done," may be considered both justifiable and sometimes prudent; but even in this instance I should also apply warmth to the extremities, and give a stimulant; not for the sake of public approval, but to leave no stone unturned, with a view to saving the life of a fellow-creature.

If the above remarks, sir, do not occupy too much space in your valuable periodical, you will greatly oblige me by inserting them in your next number.—I am, sir,

Your obedient servant,

RESPONSOR.

March 10, 1841.

MEDICAL REFORM.

To the Editor of the Medical Gazette.

SIR,

ALTHOUGH you have hitherto shown but little disposition to assist medical reformers, I have sufficient confidence in your impartiality to believe that you will admit the letter of one of these *rarae aves* into your columns; and, acting under this impression, I will briefly reply to some of the remarks of a "Provincial Physician," inserted in your last number.

This gentleman very liberally insinuates, that reformers, or those who advocate uniformity of education, title, &c. "are careless of the honour of the profession to which they belong, and heedless of the ultimate effect upon society at large," and that "they only seek their own aggrandizement." These accusations, to the generality of your readers, must appear so absurd, that it may seem almost useless to reply to them; but as others (I am sure without reflection) may entertain the same opinion as your correspondent, I will briefly state what I believe to be the motives which really actuate all true reformers. They contend, that the generality of medical practitioners are insufficiently educated. They also believe that no examination can be efficient without combining medicine and surgery. They are advocates for a good preliminary education. They see the profession, as at present constituted, a perfect chaos—a *rudis indigestaque moles*; or, as Dr. J. Johnson very aptly expressed it, "a body with three heads, all pulling in different directions." They witness the physician obtaining his degree from a dozen different sources; many of them of so disreputable a nature, that the well-educated M.D. is becoming ashamed of the title. They see many who practise as pure surgeons lamentably ignorant of the study of medicine. They find amongst general practitioners many who have passed no examination; some only members of the Hall, and others of the College; yet all possessing nearly the same privileges. Seeing these things, and believing that equality of education would add much to the dignity and honour of the profession, as well as to the respectability of its members, they press on for reform; not afraid, like the "Provincial Physician," to admit young men to the same or to superior privileges to themselves—disregarding all pecuniary considerations—not desirous of self-aggrandizement—but anxious only for the good of the noble science which they cultivate, and for the welfare of the community at large.

The above by some will be considered an overdrawn picture, but let it be observed I

have spoken only of *true* reformers. Many there are whose motives may be questioned; but this cannot apply to the great bulk of the profession, which is now agitating from one end of the country to the other.

So much for the motives of the generality of reformers; and now for some of the "Physician's" objections to our plans: and, first, to that of equality. He asks, "If all are to be captains, who is to pull the boat?" Why, sir, all could not be captains long; one would soon be selected by the rest, from his superior acquirements, to take the command. So with the members of our profession; and I cannot better illustrate my argument than by using your correspondent's own words, viz. "that merit combined with talent, like oil, always rises to the surface;" provided, I would add, that it has a fair field. We must always have men who, in consequence of their superior talents and experience, will be consulted by their brethren in cases of doubt and difficulty. Indeed, I cannot perceive that the proposed changes will in any respect affect the existing grades; for although all will take the same degree, yet it is evident that the great majority will act as general practitioners; and it is fair to presume that those gentlemen who devote themselves exclusively to medicine or surgery will, as at present, be the referee men. Take the College of Surgeons as an example: all undergo the same examination, but how few act as consulting surgeons. The Physician fears that "if all be educated alike, the lower orders will not be attended to, and that the chemist and druggist will take the place of the general practitioner." I have no apprehension of such a result. Well-educated men will always be found to attend to the poor; and the restrictions proposed to be put upon chemists and druggists will entirely, I think, obviate the latter tendency. I could say much about the corporations, as well as the general misgovernment of the profession; but I am anxious only on this occasion to shew that your correspondent's views respecting medical reformers are illiberal and unjust.—I am, sir,

Your obedient servant,

A REFORMER.

April 6, 1841.

To the Editor of the Medical Gazette.

SIR,

IN reflecting upon the present much agitated question of medical reform, and the plans proposed by the advocates for one faculty, and their opponents, I find something of good in each, mingled with much that is to be deprecated. In legislation, generally, the greatest good for the greatest number should be the ruling maxim. The proposal of the College of Physicians to

admit into their body all graduates of British Universities on payment of certain fees, is not just towards the general practitioner, inasmuch as he was submitted to as severe a test of capability as the graduate of a Scotch University, and should therefore be equally privileged: again, they who would attempt to level the enlightened physician with the apothecary who vends his pennyworth of salts or jalap, are equally unjust. For a uniform code of examination and governing laws, I am a decided advocate, and would propose that all candidates twenty-one years of age, on producing the requisite testimonials, and passing an examination which would guarantee their possession of a good literary and sound medical and surgical education, should receive the degree of M.B. qualifying them for the *general practice* of the profession.

That Candidates of the age of thirty, possessing the above-named degree of M.B. or one from a British University, should undergo an examination in *practice of medicine* and general medical literature, of such a character as to exhibit matured views of the science, and receive the degree of M.D. qualifying them to practise as physicians throughout Great Britain. I might have enlarged much on this subject, but I trust I have sufficiently explained my views, and remain,—Your obedient servant,

A GENERAL PRACTITIONER.

Notting Hill, April 5, 1841.

UNIVERSITY OF CAMBRIDGE.

Regulations respecting Candidates for a Licence ad practicum in Medicina, and also respecting Candidates for the Degree of Doctor of Physic.

1. That Candidates for a Licence *ad practicum in Medicina*, being previously Bachelors of Physic, be required to produce to the Regius Professor of Physic Certificates of their having attended on Hospital practice for three years exclusive of the nine Terms which they kept by residence for the Degree of Bachelor of Physic, and of their having attended Lectures on the following subjects; namely—

Practice of Physic and Pathology.
Anatomy and Physiology.
Chemistry.
Botany.
Medical Jurisprudence.
Materia Medica and Pharmacy.
Principles of Surgery.
Principles of Midwifery.
Practical Anatomy for two seasons.

2. That Candidates for a Licence *ad practicum in Medicina*, being previously Masters of Arts, be required to bring satisfactory

evidence to the Regius Professor of Physic of their having been employed in the study of Physic for five years after they became Bachelors of Arts; and to produce to him Certificates of their having attended on Hospital practice for three of the said five years, and of their having attended Lectures on the subjects before mentioned.

3. That every Candidate for a Licence *ad practicum in Medicina*, be required to pass an Examination to the satisfaction of the Regius Professor of Physic, the Professor of Anatomy, the Downing Professor of Medicine, and a Doctor of Physic to be nominated by the Vice-Chancellor and approved by the Senate at the first Congregation after the tenth of October in each year.

4. That in case any of the three Examiners ex officio be prevented by illness or absence from taking part in such Examination, it be competent to him to appoint a Doctor of Physic to examine in his stead, subject to the approbation of the Vice-Chancellor.

5. That there be two such Examinations in every year; one in the week immediately preceding that in which the division of the Michaelmas Term falls; the other in the week immediately preceding that in which the division of the Easter Term falls.

6. That a Candidate for a Licence *ad practicum in Medicina*, being previously Bachelor of Physic, shall not be examined for the said Licence until the Examination which shall occur next but one after his having passed the Examination required for the Degree of Bachelor of Physic.

7. That every Candidate for the Degree of Doctor of Physic, who has not previously obtained a Licence *ad practicum in Medicina*, be required to produce to the Regius Professor of Physic the same Certificates and pass the same Examination as are required in the case of Candidates for a Licence *ad practicum in Medicina*.

8. Provided that the foregoing Regulations shall not take effect, until after the end of the Easter Term 1841; provided also that the seventh Regulation shall not apply to any person who was admitted *ad intrandum in Medicina* before the Lent Term 1836.

April 1, 1841.

VACCINE REPORT.

Feb. 1841.

To the Rt. Hon. the Marquis of Normanby, Her Majesty's Secretary of State for the Home Department.

MY LORD,—The small-pox has prevailed epidemically with considerable severity since our last report; but we do not abate an iota

of our confidence in vaccination as the best protective against its malignant influence.

We have vaccinated from this station alone 15,588 persons within the last year, which exceeds the number in any former year by 2,444, and have sent out to our correspondents at home and in the colonies 165,395 charges of vaccine lymph; and we feel considerable satisfaction in being able to state that the matter we employ is obtained by succession from the original virus communicated by Dr. Jenner himself, and that we find it as effectual as ever. We may be excused, therefore, we hope, if we discourage an incautious dissemination of matter obtained from new sources, which has not stood the test of an ample experience.

HENRY HALFORD,
President of the Royal Coll. of Physicians,
and of the Vaccine Board.

JOHN P. VINCENT,
President of the Royal College of Surgeons
in London.

JOHN BRIGHT, M.D.
Senior Censor of the Roy. Coll. Physicians.

CLEMENT HUE, M.D.
Registrar.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 27th March, 1841.

Small Pox	22
Measles	8
Scarlatina	11
Hooping Cough	50
Croup	10
Thrush	3
Diarrhœa	1
Dysentery	0
Cholera	1
Influenza	24
Typhus	25
Erysipelas	4
Syphilis	1
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	147
Diseases of the Lungs, and other Organs of Respiration	264
Diseases of the Heart and Blood-vessels	23
Diseases of the Stomach, Liver, and other Organs of Digestion	44
Diseases of the Kidneys, &c.....	5
Childbed	7
Ovarian Dropsy	0
Diseases of Uterus, &c.	2
Rheumatism	2
Diseases of Joints, &c.	1
Ulcer	2
Fistula	0
Diseases of Skin, &c	0
Diseases of Uncertain Seat	103
Old Age or Natural Decay.....	82
Deaths by Violence, Privation, or Intemperance	22
Causes not specified	1
Deaths from all Causes	865

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Friday, April 2, 1841.

T. H. Graham.—T. Johnson.—E. Menzies.—C. J. Parrott.—T. Longmore.—F. Weatherley.—E. Swasey.—T. Moore.—J. C. A. Franz.—W. Newbegin.—R. S. O. Thring.—T. G. Heathcote.

Monday, April 5, 1841.

W. A. Rogers.—J. Medicott.—A. J. Shepard.—R. T. H. Bartley.—A. Emson.—T. J. Young.—T. Jones.—T. Taylor.—E. F. Mercey.—D. J. T. Francis.—H. Stott.—H. C. Stead.—C. G. Borehends.

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

March.	THERMOMETER.		BAROMETER.	
Wednesday 24	from	36 to 56	30·14 to	30·15
Thursday . 25		28 57	30·08	29·92
Friday . . . 26		30 62	29·67	29·54
Saturday . 27		37 56	29·65	29·81
Sunday . . 28		29 55	29·86	29·88
Monday . . 29		45 56	29·85	29·77
Tuesday . 30		38 52	29·77	29·84

Winds, South and S.W.

On the 24th, generally cloudy; sun shining frequently during the afternoon. The 25th, morning overcast, otherwise clear. The 26th, generally clear; rain in the afternoon. The 27th, morning cloudy, otherwise clear. The 28th, generally clear. The 29th cloudy; raining very fast, and at times heavily, during the evening. The 30th, morning clear, otherwise cloudy; a little rain in the afternoon.

Rain fallen ·255 of an inch.

Wednesday 31	from	38	53	29·59	29·50
April.					
Thursday . 1		39	55	29·54	Stat.
Friday . . . 2		38	57	29·55	Stat.
Saturday . . 3		30	54	29·60	29·59
Sunday . . . 4		27	55	29·59	29·46
Monday . . . 5		40	52	29·38	29·50
Tuesday . . 6		37	54	29·56	29·74

Winds, S.W. and S.E.

On the 31st ult. evening overcast, with rain, otherwise clear. The 1st inst. generally cloudy, a little rain fell about 2 p.m. The 2nd, and two following days, generally clear. The 5th, morning cloudy, with rain, otherwise clear. The 6th, cloudy, raining frequently during the afternoon.

Rain fallen, ·35 of an inch.

CHARLES HENRY ADAMS.

ERRATUM.

In Dr. Mackenzie's communication on the Insertion of the Straight Muscles, for "Bernouilli," read "Bernoulli."

WILSON & OGILVY, 57, Skinner Street, London.

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FRIDAY, APRIL 16, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXX.

Apoplexy continued. Relations between the symptoms and the appearances found in the brain after death. Exciting causes. Treatment.

I LEFT off in the last lecture, after having described the appearances met with, in the head, at different periods after an attack of cerebral hæmorrhage; and pointed out the various sources of the hæmorrhage; and endeavoured to explain why it happens that the blood so much more commonly proceeds from a ruptured vessel in or near the corpus striatum and optic thalamus, than in any other part of the brain.

Relations between the symptoms and the morbid appearances.—I have already anticipated, in some degree, the account that may be given of the connexion which subsists between the morbid appearances, and the symptoms. I shall proceed to touch upon certain points, relative to that connexion, which have not yet been noticed.

One of the most remarkable circumstances which dissection teaches us, when there has been partial palsy, is, that the palsy is on *one* side of the body, and the hæmorrhage of the brain on the *other*. This is a very general law. But exceptions to it are said to have been observed. Morgagni mentions such. Dr. Bright has recorded a somewhat doubtful case of exception. I have never met with any: and I cannot help suspecting that in some of those which are said to have occurred, mistakes have been made: that either they have been incorrectly observed,

or inaccurately described. You may consider the rule as almost, if not altogether, universal.

This crossing over of the morbid effect of the extravasated blood, or of any other diseased state, has long been supposed to be satisfactorily explained by that crossing over of nervous fibres which takes place at the upper part of the spinal cord. Just where the medulla oblongata and the medulla spinalis unite, the anterior pyramids decussate each other, and send their fibres mutually to the opposite side of the body. All this of course you know. The right anterior pyramid is continued into the centre of the left half of the spinal cord; and the left anterior pyramid into the centre of the right half of the cord. Now supposing, as we have every reason to suppose, that the nervous influence, whatever may be its nature, travels in the course of the fibres of the brain, we see in this decussation of the anterior pyramids an easy and pleasing solution of the phenomenon in question. But then comes this serious difficulty. How does it happen that the muscles of the *face* and *tongue*—which are supplied by nerves that arise from the brain *above* the place of decussation—how does it come to pass that *these* muscles sustain the same cross injury, and are paralyzed on the same side on which the limbs are paralyzed? And again, how does it happen (as it certainly does) that hæmorrhage into the *cerebellum* should have a similar cross influence?

I am not aware that any one has undertaken to explain this anomaly, except Mr. Mayo; and his conjectures upon the subject are so recent, and some of them so ingenious, and some them, in my opinion, so erroneous, that I cannot do less than touch upon them here.

He takes some pains, in the first place, to shew that the morbid influence is communicated from one side of the brain to the limbs of the other side of the body, by means of the fibres of decussation already

described. This point did not, I think, require any laboured demonstration; but he has made a happy use of two facts previously ascertained, which, taken together, afford a very neat proof that the transference of the morbid influence from one side to the other actually takes place in that very part of the nervous system where the decussating fibres cross. The *facts* are stated by Dr. Yelloly, in the *Medico-Chirurgical Transactions*. Sir A. Cooper divided the right half of the spinal cord of a dog in the space between the occiput and the atlas; immediately, that is, after the cord has emerged from the skull through the foramen magnum: the result of this division of the cord was hemiplegia, paralysis of the limbs, on the *same side* with the injury. The bridge by which the morbid influence crosses over must therefore be *above* that point. We have got a limit on one side. And a case observed by Dr. Yelloly gives us a limit on the other. He examined the head of a man who had died hemiplegic; and he found a tumor, as big as a filbert, imbedded in and pressing upon the *right* side of the *annular protuberance*. The palsy had existed on the *left* side. The bridge of communication must consequently lie *below* that point. It must lie, therefore, between the two points now indicated; *i. e.* it must be either in the medulla oblongata, or just at the junction of the medulla oblongata with the medulla spinalis. Now in this very interval, *and here alone*, a decussation of the nervous filaments is found to exist. There can be no doubt that the decussating fibres form the channel of communication.

Supposing (what I think very questionable) that the prevalent notion respecting the uses of the anterior and posterior columns is correct, Mr. Mayo next explains, more clearly than I have found it explained by any previous writer, that the decussation of the anterior pyramids accounts both for the palsy and for the anæsthesia, which are apt to accompany cerebral hæmorrhage into the opposite side of the brain; inasmuch as the decussating fasciculi, on plunging into the opposite column of the spinal marrow, strike into its *centre*; and implicate themselves nearly as much with the posterior, as with the anterior, *i. e.* nearly as much with what is thought the sentient, as with what is thought the motor portion of the cord. The wonder seems to be why the numbness is not more constant; why it is infrequent in comparison with the affection of the voluntary muscles.

Mr. Mayo holds that palsy does not result from "the interruption of the ordinary supply of nervous stimulation furnished by the brain"—for this reason, that in living animals the brain has been gradually removed, sliced away, yet sensation, and the power

of voluntary motion, have subsisted; and that instances of acephalous human infants, which have survived their birth, shew the medulla oblongata and spinal cord to be sufficient, *without the brain*, for the production of sensation and voluntary motion. He conjectures therefore that the immediate cause of the hemiplegia is "*a depressing influence, or shock*, (a *withering influence* he elsewhere calls it) originating in the brain when in certain states of lesion, and propagated from it to the medulla oblongata and spinal marrow."

Now it can scarcely be doubted, at least I cannot doubt, that the inference here drawn from the movements observed in acephalous monsters, and in animals after amputation of their brain, is a wrong inference. They are purely automatic movements, independent of sensation and of the will, and derived from the reflex endowment of the cord: and if this be so, the main foundation of Mr. Mayo's argument is cut away. For my own part, I have no faith in any *shock* except *pressure*. But mere pressure Mr. Mayo repudiates, asserting that in many cases of hemiplegia from cerebral disease there is *no* pressure. And this may be granted: although even in cases of softening, such as he refers to, the mere absence of support in some parts of the brain may lead to the subsidence or settling down of other parts, so as to cause pressure upon the medulla oblongata. I took some pains, in the course of the last lecture, to shew you that pressure is adequate to the production of coma and *general* paralysis; and pressure on a nerve in its course we are sure is capable of occasioning *local* paralysis; and upon the whole, my doctrine of the production of palsy by cerebral hæmorrhage or cerebral disease is, that it is frequently (not always) owing to the benumbing influence of *pressure*. I do not ask you to adopt that opinion; nor do I think the solution of the question is of any primary importance; yet I place the question before you, as the best way of impressing upon your memories the *facts* which it involves.

Now whether Mr. Mayo's notion, that some "*shock*" or "*withering influence*" is transmitted from the injured brain, be true; or whether that which I have proposed to your notice, *viz.* that this influence is no other than the benumbing influence of pressure, be true; either supposition will plausibly account for these facts, *viz.* that "in general hemiplegia from cerebral lesion, the palsy of the leg is (commonly) less complete, and is sooner recovered from, than palsy of the arm," and that when one of these limbs only is affected, it is (commonly) the *arm* alone. The shock, or the pressure, will be most felt in proportion as the part is nearer the origin of the pressure; and less

and less felt in proportion as we recede from the source of the injurious influence.

But, unfortunately, I was obliged to insert the word (*commonly*) in the statement just made of the *facts*; which word Mr. Mayo does not employ. To make either his theory, or the theory of pressure, perfectly satisfactory, either the arm alone should be affected; or the affection of the arm should always accompany and be more intense than, or at any rate not less intense than, the affection of the leg. But this is not the case. Since Mr. Mayo's observations were published, I have met with two or three cases, and pointed them out to him, in which the leg alone, or the leg first, has been palsied, from cerebral disease. And Andral, among 75 cases of cerebral hæmorrhage collected for another purpose, which I shall presently advert to, met with 12, in which the leg only was affected. It is a great pity that these stubborn facts should thus cross and thwart a very pretty theory. Perhaps they may hereafter be found not in reality inconsistent with that theory; but at present they are a sad stumbling-block in the way of our adopting it.

I will venture, in my turn, to suggest a conjecture by which the apparent inconsistencies may perhaps be reconciled. I think the excepted cases may be probably accounted for upon the very theory which Mr. Mayo rejects; viz. that the paralysis proceeds, *in these cases*, from a simple interruption of the nervous influence, a breaking up of the road by which the changes leading to sensation travel in one direction, and the mandates of volition in the other. We may easily conceive that, without any pressure, the conducting fibres of communication between the sensorium and the muscles of the leg may *alone* be torn across, or severed by a process of softening, while the residue of the conducting apparatus is entire: and I think it may be worth your while to note, as future opportunities for observation occur, whether the cases that fall within the general rule are not cases of hæmorrhage, in which there is a palpable cause of pressure; and whether what are now called anomalous cases are not cases of mere *softening*, without any obvious compressing agency.

I must not omit to give you Mr. Mayo's explanation of the *other* difficulties to which I referred—viz. of the paralysis of the muscles of the *face* being on the opposite side from the injury of the brain; and of the hemiplegia being on the opposite side from an injury of the *cerebellum*. And having given you it, I shall leave it, without farther comment, to your consideration.

Mr. Mayo's words are: "Where the decussating fasciculi of the anterior pyramid plunge into the opposite half of the spinal marrow, they are implicated, in a wonderful closeness of inter-texture, with fibres, which,

in their upward course, bend towards the places of origin of the ninth and seventh, and of the eighth and fifth nerves of the palsied side. May it not be supposed that this interlacement may be a sufficient means of communicating the palsy influence to the *ascending* fibres, which are in close relation to the affected cerebral nerves?"

Again, "How is the fact to be accounted for, that hemiplegia of the opposite side is produced by lesion of one hemisphere of the *cerebellum*? I have little doubt that the following explanation of the phænomenon will eventually be proved to be correct. The fibres of the anterior pyramids pass through the pons varolii. The pons varolii consists in great part of filaments which issue from each hemisphere of the cerebellum. These filaments may easily be supposed to convey a depressing influence from the diseased hemisphere. But in their course they come immediately upon the filaments of the anterior pyramid of the same side; and they are so implicated with the latter, with such a singular closeness of reticulation, and often with so much that looks like an actual interchange of filament, that it is far from unlikely that they may transmit to the descending fasciculi of the pyramid a shock (*qu.* a degree of pressure), which may thence be communicated to the same part at which a *cerebral* lesion exerts its paralyzing force."

I will only say farther of this hypothesis, that if the explanation it furnishes of the facts in question be not the best and most satisfactory in the world, it is the best and most satisfactory that we *possess*: and that, at any rate, we may make use of it to bind those facts to our recollection, until some better theory shall be devised.

There is one very curious law asserted, by Andral, in respect to hæmorrhage of the *cerebellum*. If the blood is effused into one side of the cerebellum, and no where else, the palsy that ensues follows the general rule; it takes place in the limbs of the opposite side of the body. But supposing hæmorrhage to take place on one side of the cerebrum, and on the other side of the cerebellum, simultaneously: what then, think you, happens? Doubtless you would expect that there should be palsy on *both* sides of the body. Hemiplegia on the one side, from the effusion into the brain proper; hemiplegia on the other side, from the effusion into the cerebellum: double hemiplegia; that is to say, general palsy. But it is not always so, in fact. The cerebral affection seems to overpower and master that of the cerebellum. Whatever the explanation may be, the palsy has been found to occur on the side opposite to the lesion in the brain proper; and not to occur on the side opposite to the lesion in the little brain. This is a very singular fact, of which Andral relates

four or five examples. But I suspect that they will ultimately take their place among the "anomalous" cases. As facts multiply, the law will, I conjecture, be found to be a different one.

The complex structure of the brain, and the dissimilar consequences that ensue, in different cases, from its injury or disease, lead directly to the belief not only that the organ subserves several distinct functions, but also that separate parts or sections of it hold peculiar and definite relations with other portions of the body. Ingenious men have even attempted to settle these points experimentally. By wounding or removing various portions in succession of the cerebral mass in living animals, and comparing the results, they have endeavoured to assign to each portion its particular province and function. But, to say nothing of the remarkable differences which exist between the cerebral functions in man and in the inferior animals, there is an unavoidable source of fallacy common to all such experiments. We cannot reach the particular spot in the brain upon which the contrived injury is to be inflicted, without penetrating and hurting various other parts; and from these combined injuries (dangerous, indeed, and often fatal in themselves) arise symptoms which the experimenter may erroneously conclude to be characteristic of the lesion originally in his contemplation.

Much more accurate and satisfactory data for the determination of this interesting class of questions, would seem to be furnished by the spontaneous operation of disease, and especially of the disease we are now considering. The injury done to the cerebral substance by the eruption of blood is not less sudden, nor less mechanical, than in the experiments or contrived observations to which I have alluded. It is capable also, in many instances, of exact appreciation in regard to its extent; the parts which lie round the seat of the effusion remain undisturbed; and above all, the organ that is the subject of our observation is the *human brain* itself.

Attempts have accordingly been made to connect particular symptoms with the disorganization of particular parts of the brain. These attempts can boast, as yet, it must be confessed, but little success. Very few, if any, of the conclusions hitherto advanced upon this intricate subject can be relied on. Yet it is proper that you should be informed of them.

Because palsy of the arm is, in general, more complete, and more persistent, than palsy of the leg, it has been maintained that the former, the paralysis of the arm, is to be ascribed to hæmorrhage of the *corpus striatum*, which seems to be *more common* than any other; and upon similar grounds

hæmorrhage of the *optic thalamus* has been supposed to determine paralysis of the *leg*. So much have these distinctions been confided in, that the honour of having first pointed them out has actually, in France, been made a subject of dispute. Now it is plain that one example of the contrary effect of these particular lesions, would suffice to upset the whole theory: but *many* such exceptions have, in fact, been noticed. It was with the view of settling this question that Andral collected and collated the 75 cases of cerebral hæmorrhage to which I lately referred. In each of these 75 cases the clot of blood was sufficiently limited to allow of that case being applied towards the solution of the controverted points.

In 40 of the 75, both the leg and the arm were paralyzed together. And where was the place of the hæmorrhage in these 40 cases? Why, in 21 of them the corpus striatum was the only part injured; and in 19 of them the optic thalamus was the only part injured. Thus you see, according to the theory just explained, in about one-half of these cases the arm alone *should* have been palsied, and in about half the leg alone: whereas both leg and arm were palsied in them all.

Again, in 23 of the 75 cases the palsy was confined to the arm. Therefore, according to the theory, the injury should have been confined to the corpus striatum. What was the fact? Why, in this class of cases also there was as nearly as possible an equal sharing of the injury between the two parts: in 11 of the 23 the corpus striatum alone suffered; in 10 the optic thalamus alone; in two the space between them.

Once more: there were, as I stated before, 12 out of the 75 cases in which the leg alone was palsied. Consequently, in all of these 12, if the theory were sound, there should have been damage of the optic thalamus only. But in 10 of them the mischief was confined to the corpus striatum; in 2 only to the optic thalamus.

Gall had conjectured that the faculty of speech was placed under the governance of the anterior lobe of the brain: and Bouillaud has endeavoured to support that opinion by a number of facts observed in connexion with cerebral hæmorrhage; but Cruveilhier has brought forward several curious instances in which the loss of speech was a prominent symptom, while the disease was *not* found in the anterior lobe, but in some other part of the brain.

Andral, with his accustomed industry, has accumulated evidence upon this point also.

In 37 cases of cerebral hæmorrhage observed by himself or by others, in which the morbid condition occupied one or both of the anterior lobes, the power of speech was abolished 21 times, and unaffected 16 times.

On the other hand, he has collected 14 cases, in which the power of speech was lost, yet no alteration had taken place in the anterior lobes. In 7 of these 14 cases the lesion was situated in the middle lobes; and in the other 7 in the posterior lobes of the brain.

There can be no doubt that there are certain distinct parts of the brain which influence respectively the upper and lower limbs; inasmuch as they are often separately palsied: and since the loss of speech is occasionally the only, or the most prominent symptom, while in other cases the speech is not affected at all, we cannot but believe that this faculty is under the special guidance of some definite part within the cranium. But the facts that I have just been quoting, shew, in the most convincing manner, that we are not able, as yet, to allot these separate functions to their proper spots in the cerebral mass.

Exciting causes.—I dwelt some little time, in a previous lecture, upon the circumstances that give *warning* to the patient, or to his physician, that the former is in danger of being smitten with apoplexy. And the great use of being acquainted with these circumstances, and of looking out for them, consists in the opportunity and the *authority*, which they furnish, for enforcing, upon the person in whom they manifest themselves, the absolute necessity of avoiding all the avoidable *exciting* causes of the disease. But our means of advising him will be very imperfect if we have not carefully considered what these exciting causes are. I propose to devote a few minutes, therefore, to the consideration of the circumstances that are apt to *bring on* the attack. There are many cases of apoplexy in which we cannot trace the operation of *any* such causes: but in many other cases their influence is decidedly marked; and the avoidance of them, while it is important to all who shew a disposition to apoplectic disease, is especially so to those who having once suffered an attack, have reason to dread a *repetition* of it.

In the first place, any thing which is calculated to hurry the circulation, and to increase the force of the heart's action, is likely to operate as an exciting cause of apoplexy: simply by augmenting the momentum of the blood against the sides of the cerebral vessels, which in advanced life are so often diseased and weak. Strong bodily exercise therefore is a thing to be avoided by all persons in whom the predisposition to apoplexy has declared itself. It is of much importance to make patients aware of this; for many persons think, when they labour under uncomfortable bodily feelings of any kind, they may get rid of them by a brisk walk; or by galloping some miles over the country on horseback.

Another dangerous state for such persons arises whenever the free escape of the blood from the head is suddenly obstructed. I have adverted to this before. Certain diseases, chiefly thoracic, which tend to keep the veins of the head inordinately full, rank among the *predisposing* causes of apoplexy. But, upon the very same principle, various conditions, which are temporary only, may operate as *exciting* causes. By what is called "holding the breath," whether upon an inspiration or expiration, the transit of the blood through the lungs is impeded: and the check is felt (through the pulmonary artery, right chambers of the heart, and great veins) in the vessels of the head. And this effect is increased when *straining* is at the same time performed; that is, when a deep breath is taken and retained, while some muscular forcing effort is made.

Under this principle fall a number of bodily acts, which, however harmless in a healthy frame, are not without peril to a person having a predisposition to apoplexy. The motion of the blood in the lungs, and therefore in the head, is checked in the acts of coughing, vomiting, sneezing, laughing, crying, shouting, and so forth. You cannot have looked at a person in a violent paroxysm of coughing without seeing that it produced a determination of blood to the head, or rather a congested state of the veins of the head. The jarring pain in the head which is apt to follow each succession of the cough depends upon this principle: which is often strikingly exemplified in young children labouring under hooping-cough. They turn purple in the face, and become giddy; and not uncommonly ecchymosis of the conjunctiva occurs, giving fearful evidence of what might just as well take place *within* the cranium. It is not very unusual for the whole of the white part of the eye to become suddenly blood-shot in these violent fits of coughing; and convulsions even happen under the like circumstances.

Straining at stool is a common exciting cause of apoplexy in those who are predisposed to it. And this is one of the worst dangers attending costiveness of the bowels in old people: but it is one which it is often in our power effectually to obviate. It is more within our control than a bad cough could be. Any kind of straining indeed is equally perilous. A very good illustration of this danger was recently afforded by a patient of my own. He was attacked with apoplexy on his way to Ascot races; and upon recovering somewhat, was found to be paralytic on one side of the body. He was brought back to town, when I saw him. After some time he regained the power of using the affected limbs to a very considerable extent; so as to be able to walk about, and follow his business, which was that of a job-master, or proprietor of a livery stable. I

cautioned him seriously, *inter alia*, against straining : but I suppose he forgot my caution. For, when getting up one morning, he tugged violently in attempting to pull on a damp boot, and in the midst of his efforts fell back insensible : and from this relapse he never fairly recovered.

To the same principle are to be referred a variety of things which a patient, in danger of this disease, must most carefully avoid. Lifting heavy weights ; leaping ; striking a hard blow ; playing on wind instruments ; even long and loud talking. Dr. Abercrombie relates two instances of fatal apoplexy brought on (as it would seem) by a sustained exertion of the voice : one of the attacks happened to a clergyman during the delivery of his sermon ; the other to a literary man while speaking in a public assembly. In both cases a large quantity of blood was found extravasated within and upon the brain. Dr. James Gregory used to mention a patient of his, an officer in the army, who had apoplexy, and in whom the attack had been preceded by pains of the head, and giddiness, upon his giving the word of command, and particularly when dwelling upon the last sound ; that is, when he made a long expiration. Precisely of the same kind is a case told by Van Swieten, of a singer who was obliged at length to abandon her vocation by reason of gradually increasing vertigo whenever she had to hold a high note. Violent emotion is another exciting cause. Large fires, crowded rooms, the heat even of the sun, will tend to produce apoplexy, and therefore ought to be shunned by those who have a tendency to that disease. The warm bath is not without hazard to such persons. This is so well known, I understand, at Bath, that the physicians there will not allow paralytic patients, in whom the paralysis has been connected with apoplexy—hemiplegic patients, for example—to go into their hot baths. The excitement of drunkenness, and the venereal excitement, are not uncommon causes of apoplexy, especially in old persons. I had a man of middle age under my care during the spring of 1837, in whom a most awful attack of apoplexy came on under circumstances just as I have now referred to. He had dined at a large festive party, and afterwards accompanied a woman with whom he was acquainted to a brothel ; and he was struck with palsy during the act of intercourse. He was long unable to speak ; and he still remains, and probably will ever remain, a cripple ; imperfectly hemiplegic.

I have been since consulted upon the case of an old gentleman residing in France, in whom an attempt at sexual connexion was attended with similar consequences.

“ The Gods are just, and of our pleasant vices
Make instruments to scourge us.”

Posture again has no small effect upon

apoplectic people. Giddiness, and some degree of confusion of thought, are apt to be occasioned in most persons by long stooping. There is one peculiar posture or position mentioned by Dr. Fothergill as being very unsafe, especially for short-necked persons—viz. that position which is assumed when we turn the head to look backwards for any length of time without turning the rest of the body ; in fact, a twisting of the neck. In this action the jugular veins are more or less obstructed. He gives an account of a man who was seized with apoplexy as he was crossing the Thames in an open boat ; he having kept his eye fixed upon a particular ship until, and after, he had been rowed past her. On the very same principle tight ligatures worn about the neck, and compressing the jugular veins, may bring on apoplexy ; the wearing a tight neckcloth, for example. A continental writer informs us that a Swedish officer, who was desirous that his men should look well in the face, caused them to wear tight stocks ; and the consequence was that in a short time a great many in that regiment died of apoplexy. Dr. Abercrombie quotes from Zitzilius the case of a boy who had drawn his neckcloth remarkably tight, and was whipping a top, stooping and rising alternately. After a short time he fell down apoplectic. The neckcloth being loosened, and blood drawn from the jugular vein, he speedily recovered.

There is one very powerful exciting cause of apoplexy, in those predisposed to it, which I need only refer to now, because the facts that have been observed in proof of its agency were fully detailed in a former part of the course ; I mean exposure to cold. You will recollect my telling you that the number of deaths in London from apoplexy and palsy in the month of January 1795, which was a bitterly cold month, very much exceeded the number in the month of January 1796, which was a remarkably mild month. The cold operates in two ways, in the production of apoplexy. In the first place it drives the blood from the surface, and accumulates it in the large vessels of the interior of the body, and so increases the stress upon the cerebral arteries. And in the second place, the cold has a great influence in causing or aggravating affections of the *chest* ; and the return of the venous blood from the head is impeded, in the manner just now explained, by fits of coughing and obstructed respiration.

This influence of external cold, and probably certain barometric conditions also of the atmosphere, help to explain, what I am sure I have several times had experience of, namely, the epidemic prevalence, now and then, of apoplectic seizures.

There is an alleged *exciting* cause of cerebral hæmorrhage, which I think it the more

necessary to consider, because I believe that very erroneous notions prevail about it, even among pathologists of eminence. I allude to the imputed dependence of cerebral hæmorrhage upon hypertrophy of the left ventricle of the heart. It has been supposed that the powerful contractions of a ventricle thus morbidly strong may drive forwards the blood with such unusual force, as to strain and burst the cerebral arteries. Dr. Hope, in his very complete work upon Diseases of the Heart, uses these words:—"Instances of apoplexy supervening upon hypertrophy have been so frequently noticed, that the relation of the two, *as cause and effect*, is one of the best established doctrines of modern pathology." And similar opinions are entertained by the most distinguished of the French writers on this subject; Andral, Bouillaud, Cruveilhier. I think they are all wrong: or that at least they state their proposition much too broadly, and generally.

I fully admit, no less from my own observation than upon the testimony of others, the frequent coincidence of hæmorrhage of the brain and hypertrophy of the left ventricle of the heart; but I distrust the reasoning which would always connect these events with each other as cause and effect. They may, sometimes, have that relation; but I have long thought that in most cases, if not in all, the coincidence is capable of being explained upon other and more satisfactory principles.

In the first place, hypertrophy of the left ventricle of the heart is very frequently, far more frequently than not, accompanied by other structural changes of that organ: changes which imply some impediment to the circulation; changes which involve or influence its right chambers also. In fact, disease of the right heart is not very often seen, without disease of the left; and one of the commonest forms of alteration to which the left side is liable, is hypertrophy of its ventricle. Now I have already pointed out to you the connexion which sometimes subsists between cerebral hæmorrhage and such disease of the heart as obstructs the ready and regular descent of the blood from the head through the veins. Many of the cases of apoplexy occurring in persons who have previously had cardiac hypertrophy are, I really believe, cases of this kind. The brain affection is dependent, in part, upon disease of the heart, but not upon the preternatural strength of its left ventricle. The heart acts morbidly upon the brain through the veins, and not through the arteries.

But there is another reason for the coincidence; and here the arteries are concerned.

No one can doubt that the momentum, with which the blood reaches the cerebral arteries, in *healthy* persons, under violent bodily exercise or mental excitement, must

often exceed the momentum produced by a hypertrophic heart in the cerebral arteries of persons who are tranquil and at rest. But apoplectic seizures are frequent under the latter circumstances, infrequent under the former. We must look, therefore, for something more than the mere hypertrophy to explain the coincidence. Now (supposing the absence of any check to the flow of blood from the head through the veins) that something is to be found in *disease* of the arterial system.

When the arteries of the brain are ossified, or changed, and rendered brittle in the way I yesterday described, the *commencement of the aorta* also is found, in a great majority of cases, to be the seat of similar alterations; and, often, to be sensibly dilated. Now the mere albuminous deposit beneath its inner tunic must seriously impair the elasticity of the vessel; and in this way the free passage of the blood out of the heart will be impeded; and the dilatation of the aorta at that part will produce the same hindrance more certainly and in a greater measure. Still more effectually and obviously will any contraction of the outlet prove an impediment. It is in consequence of these mechanical obstacles to the free exit of the blood from the left ventricle, that the walls of that chamber, urged to more vigorous contraction, become thicker and more powerful. The hypertrophy is the natural compensation for the morbid state of the aorta; without it the heart would much sooner become unable to propel its contents at all: and the hypertrophy does not often, I fancy, become greater than is needful for its purpose. The strength of the left ventricle, therefore, in such cases, is not a true measure of the force with which the blood is driven into the distant arteries. Quite the contrary. It is a measure of the *difficulty* with which the blood is circulated through the *primary branches*, and therefore through the entire system of the arteries. It indicates the *diminished* force with which the blood is likely to reach the cerebral vessels. And in point of fact you will find in *many* cases of hypertrophy of the left ventricle—I do not say in all, but certainly in very many—you will find the pulse at the wrist to be disproportionately small and feeble. So that, in these cases, instead of regarding the cerebral hæmorrhage as the *effect* of the hypertrophy (acknowledging, as I do, the frequent coexistence of these morbid conditions) I have been accustomed to look upon the apoplexy and hypertrophy as *concomitant effects of the same cause*; viz. of disease pervading the arterial tree. The hypertrophy of the left ventricle is the effect of the diseased condition of the aorta at its mouth; the cerebral hæmorrhage is the effect of the same diseased condition of the arteries in the brain,

When you find each of these lesions, and nothing to retard the venous current, you may, I believe, safely apply this explanation of the occurrence of apoplexy.

Having again referred to the frequent existence of disease in the cerebral arteries as a predisposing cause of hæmorrhage within the brain, I will just point out, before I leave this part of the subject, the light which that fact throws upon the circumstance that sanguineous apoplexy is so peculiarly a disease of advanced life. Earthy concretions in the coats of the arteries are so frequent in the later periods of existence, that they are met with, according to Bichat, in seven individuals out of ten of those who die beyond the age of sixty: and Dr. Baillie considered ossification to be much more common in old persons than a healthy state of the arteries.

Prognosis.—In the account which I have endeavoured to give you of the symptoms of apoplexy, of the different modes in which the attack may commence, and of the various morbid appearances discovered within the cranium in the fatal cases, I have already embodied almost all that can be stated, with any confidence, respecting the special diagnosis and the prognosis of the disease. The one of these follows the other: the exact diagnosis being known, the prognosis is seldom difficult. By the diagnosis, however, I do not now mean simply the recognition of the disease as a case of apoplexy: of that *general* diagnosis, of the means of distinguishing the coma of apoplexy from the coma caused by opium or alcohol, I told you all that I know in a former lecture. But I use the term diagnosis now in a stricter sense, and in reference to the distinctions that exist between *one case* of apoplexy and *another*; and I say that in proportion to the accuracy with which we may be capable of determining the precise condition of the contents of the skull, will be the facility of predicting the issue of the complaint. Let me remind you, then, that when a patient suddenly becomes apoplectic, we cannot tell whether there be effusion of blood, or effusion of serum, or no effusion at all within the cranium: and therefore the diagnosis must be precarious and uncertain. If, after the use of suitable remedies, the coma persists for many hours, the prognosis becomes worse. In those cases which begin with pain of head, faintness, and nausea, and which pass on to coma, the prognosis is positively bad; for the diagnosis is easy, and we are tolerably certain that a blood-vessel has given way, and that a large quantity of blood has ploughed up the substance of the brain. In the paralytic cases also, if coma supervenes, the prognosis is gloomy: but frequently coma does not supervene, and then our prognosis, as far

as life is concerned, may be pronounced favourable.

Among the symptoms that belong to the apoplectic condition itself, there are some which experience has selected as being most especially of evil omen: and it is well worth your while to remark that these discouraging signs relate, almost all of them, to the automatic functions of the cranio-spinal axis. The open, fixed, unwinking eye; the explosive flapping of the cheeks in expiration; the inability to swallow; the slow, sighing, interrupted breathing; the loosening of the sphincter muscles of the bladder and anus; these are fatal symptoms, and these all depend upon the excito-motory portion of the nervous system. Perhaps the profuse sweat that so often attends the process of dissolution may be referred to the same source; the whole tone of the various tissues being lost or relaxed. I would not say that no one of these symptoms is ever recovered from: but I may say that of twenty patients in whom such phenomena occur nineteen will die.

Now symptoms of this kind may be expected to arise, if there be hæmorrhage in or near the medulla oblongata; or if there be mischief so extensive in the brain as to cause pressure upon the medulla oblongata. We should reason out the likelihood that such symptoms would be of bad augury. But the fact that they are so was ascertained long before the theory which accounts for them was devised. The fact is independent of the theory, and for that reason helps wonderfully to confirm it.

Treatment of apoplexy.—The older writers entertained some very false notions in respect to the distinction between sanguineous and serous apoplexy. They laid it down that apoplexy resulting from extravasation of *blood* within the cranium was denoted by flushing of the face, and strength of the pulse; and that it was a disease of persons in the vigour of life: while apoplexy resulting from the effusion of serum was marked by paleness of the countenance, and weakness of the pulse; and occurred in the old and the infirm: and they directed their practice according to this distinction. After what has already been said, I need not tell you that this classification of apoplexies could not have been founded upon the actual observation of disease: and that our treatment, now a days, is not regulated by any such erroneous theory.

Nevertheless, I do not mean altogether to praise the modern practice in apoplexy; for it is often one of mere routine. Practitioners are very apt, in this as in other instances, to be guided in their choice of remedies by the *name* of the disease, and to treat all cases of apoplexy alike. I remember being

much amused by the perplexity which a friend of mine once told me he had felt on being summoned by letter many miles into the country to see a gentleman who had been struck with apoplexy. As he posted down he earnestly revolved in his mind what he might be able to advise when he should reach the house of sickness. He felt confident that the patient must already have been copiously bled; cupped, or leeches; blistered; and thoroughly dosed with calomel, senna, and croton oil. Mustard poultices had doubtless been applied to his legs. My friend was distressed to think that while much would be expected, nothing would be left for him to do worthy of so long a journey, and so heavy an expense to the patient. A clyster of turpentine might yet, perhaps, be an untried expedient. His cogitations were cut short, however, and his cares relieved, by an express which met him half-way on the road to announce that the patient was dead. Now this is the routine of which I speak: most proper in many cases; unnecessary in others; pernicious in some. There are persons who seem to think that they have not done their patient justice if any part of this active intermeddling has been omitted. Others regard depletion as being worse than useless, and trust entirely to stimulants and cordials. These are still more dangerous *routiniers* than the others: but they are fewer in number.

Our practice would indeed be much easier than it is, if we could thus make one plan fit all cases which are, nominally, the same. But I need not, now, tell you that diseases alike in name—aye, and alike in their essential nature—are often widely different in their circumstances. I formerly explained to you that certain symptoms tell us what the disease is; but that we are often obliged to look to other symptoms, which may tell us what we are to do. I know of no rule so likely to guide you aright as that laid down generally by Cullen, of *obviating the tendency to death*. You must examine and judge to which of the several modes of dying there may be any obvious approach. If the tendency be, as in cases of apoplexy it mostly is, to death by *coma*, then blood-letting and the evacuating system will be requisite. If, on the other hand, the tendency be to death by *syncope*, you must withhold the lancet, and even have recourse to stimulating and restorative measures. Now the distinction between these modes of dying is to be made by attending to the state, not so much of the nervous, as of the sanguiferous system. Insensibility and unconsciousness are common both to syncope and to coma: and cases which fall under the class of apoplexies, and which we cannot separate from that class, are sometimes really more like cases of concussion than any thing else; the shock having

been of internal instead of external origin. If the pulse be full, or hard, or thrilling (sometimes it feels like a tense vibrating rope), or if there are obvious external signs of plethora of the head, you must abstract blood. You are not to refrain from bleeding the patient because he is pale, if his pulse warrants it: nor may you omit taking blood if the head and face be turgid, although the pulse be small; for that smallness may depend upon organic disease of the heart.

On the contrary, if his skin is pale and cold, and his pulse feeble or flickering, you would probably ensure your patient's death, or determine the accession of palsy, if you withdrew from the failing heart and blood-vessels a portion of their natural stimulus. I can only invite your attention to these broad features of distinction. Being once taught to look for and attend to them, your own judgment must instruct you as to what may be needful in particular cases. To this, as to most other diseases, the remark of Boerhaave is strictly applicable, who declares that he knows of nothing which can be called a *remedy*, "*quin solo tempestivo usu tale fiat.*"

Having made up your mind as to the general indications of treatment, you will pursue them steadily in detail. If the patient to whom you are summoned be stupid and drowsy rather than faint, and his pulse and appearance warrant the conclusion of plethora capitis, the first thing to be done is to place him in a semi-recumbent position, with his head raised; to loosen any tight parts of his dress, especially his neck-cloth and shirt collar, and whatever might press upon the *neck*; and then as quickly as possible to bleed him from the arm. We know that in some cases the apoplectic state occurs, when as yet no injury has been done to the brain; no effusion, no laceration of its texture; and we may hope, by timely and vigorous measures, to *prevent* these terrible evils. We never can be sure that there is blood extravasated in such cases, and we must act, in the first instance, upon the presumption that there is not. We are especially encouraged to take away a considerable quantity of blood by venesection when we perceive external signs that the vessels of the head are full; redness and turgescence of the face, throbbing and prominence of the temporal arteries, distension of the superficial veins of the neck and forehead. Our object is to take off the strain upon the internal vessels, by bleeding in such a manner and to such an amount as shall produce a decided effect upon the general circulation. Sometimes the good effect of the bleeding is very marked indeed, so that no doubt of its propriety can be entertained; the patient being so insensible as not to feel the puncture of the lancet, and yet

emerging from his coma while the blood is still flowing. It is seldom, however, that we can expect such manifest and immediate melioration as this.

After one *sufficient* bleeding from the arm, the vessels of the head may be farther relieved by cupping the nape of the neck, or the temples; and venesection may be repeated if the condition of the pulse, and of the symptoms generally, should require its repetition. It is seldomer, however, in cases of apoplexy than in cases of acute inflammation, that a second or third recourse to the lancet becomes advisable: unless, indeed, the first blood-letting has been mismanaged. Enough blood must be taken, in the first instance, to produce some evident effect; and therefore no precise rules can be laid down in regard to the absolute quantity to be drawn; nor can we make any estimate beforehand as to the whole amount of blood which it may be necessary to remove.

Even if we could be sure that a blood-vessel had given way, and blood was already poured out upon the brain, there are good reasons why (no adverse circumstances withstanding) we ought to bleed our patient largely, and at once. I will enumerate briefly the benefits we seek to obtain by the abstraction of blood in such cases.

1. The effusion from the ruptured artery may be slowly going on. Bleeding from a vein, so as to make a sensible impression on the general circulation, will diminish the stress upon the cerebral arteries, and so tend to put a stop to the hæmorrhage. Both of these two objects are of primary importance.

2. By early and free bleeding we lessen the chance of inflammation supervening upon the mechanical injury done to the brain by the sudden tearing and contusion of its texture by the effused blood; and

3. We thereby bring the system into the most favourable condition for the rapid absorption of the extravasated blood, and for expediting the patient's recovery from those symptoms which depend upon the presence of the clot in the brain.

But although, in that form of disease which we are now considering, bleeding is our sheet anchor, it may be carried too far, or repeated too often. We must not lose sight of the fact that many of these patients are old, and will not survive over depletion; and that if they survive at all, they will need all the strength that we dare suffer them to retain for carrying on the vital actions, when the chief instrument of the most important of the animal functions is so greatly damaged: nor of the fact that if there be blood extravasated, we cannot touch it, except indirectly, by the abstraction of more blood from the arm: nor of the fact that a patient may be bled into convulsions, and fatal syncope. In short, after the first free bleeding, you must

be guided by the special circumstances of the case, and particularly by the pulse. The woman at present in the Middlesex Hospital, with paralysis of the limbs on one side, and of the face on the other, attributes her palsy (erroneously most likely) to her having been cupped. She had had a blow some weeks before, and suffered headache from that time. At length she was cupped, from the neighbourhood of the head; and the next morning she was paralytic. This might have been an accidental coincidence. But I remember being sent for a few years ago to see a patient, at Greenwich, who had already three physicians about him, and was apparently in danger of apoplexy, of which he had for some time experienced distinct warnings. The three physicians had agreed that he ought to be cupped from the back of the neck; to which I assented; and while blood was being rapidly extracted in that manner, he became all at once hemiplegic. Similar cases have been noticed by other persons. Therefore we are not to bleed without measure or discretion.

The pulse may be small, and the arterial action feeble, while yet the veins are turgid, and the capillaries of the head and face loaded with blood. Changes may have occurred in the heart, such as to obstruct the stream which it is its healthy office to transmit. These are cases to which the local abstraction of blood from the head by leeches and cupping glasses is peculiarly adapted.

Again, the whole state of the patient may approximate more or less nearly to the state of syncope; the pulse being weak, the aspect pinched and bloodless, and the skin cool. In this condition, no good, but much harm, is to be expected from blood-letting of any kind. You will do better to apply warmth, cautiously, to the surface, and cautiously to administer what are called diffusible stimuli, of which the preparations of ammonia afford the most eligible forms. Five grains of the sesquicarbonate, or half a drachm of sal volatile, mixed with camphor julep, are ordinary doses. Meanwhile you must carefully watch for symptoms of reaction.

In more ambiguous cases, when you scarcely can tell which way the balance inclines, I would advise you to wait the effect of the next remedy I have to mention; viz. purgatives, about giving which you need not entertain the same doubt and hesitation.

Purgative medicines are of signal service in apoplexy. They empty the intestines, which are oftentimes loaded, and which by distending the abdomen have perhaps occasioned undue pressure against the diaphragm, embarrassed the breathing, and through it the cerebral circulation. Another very important purpose of hard purging, which I have frequently pointed out before, is the pro-

ducing of copious watery discharges from the bowels; whereby the blood vessels are drained, and the tendency of blood to the head especially relieved. If the patient can still swallow, you may give him half a scruple of calomel, and follow it up by a black dose. If the power of deglutition be lost, the croton oil becomes a most valuable remedy. Dr. Abercrombie suggests that it may be conveniently introduced into the stomach, suspended in thick gruel or mucilage, by means of an elastic gum tube. But really this is not necessary. If two or three drops of the oil be put upon the tongue, as far back as is possible, it will produce its specific effect very readily and well. But we are not to wait for the operation of aperients given by the mouth. Strong purgative and stimulating enemata must be thrown into the rectum: half an ounce, or six drachms, of turpentine, suspended, by the help of the yolk of an egg, in gruel or warm water. We very often witness decided signs of amendment upon the free operation of a purgative. I may mention one instance of this while it is fresh in my recollection. I was asked, a few evenings ago by a medical friend, to see an old gentleman, a patient of his. I found him in bed, comatose, but capable of being roused when loudly spoken to: but he presently fell off again into stupor. His respiration was peculiar. For a minute or two he would breathe, snoring strongly; then the breathing would cease altogether for half a minute or thereabouts; and then the stertorous respiration recommenced; and so on alternately.

He had been found by his servant on the floor, nearly insensible, in the morning, having fallen either out of, or upon rising from, his bed. He had very properly been cupped; and calomel and aperient medicine had been given: but the coma had been growing more profound all the afternoon. His bowels had been but scantily moved; and the fæces and urine were passed as he lay. His extremities were coldish. The pulse was neither full nor strong.

I learned that for four or five years he had had some very significant warnings; and within that period had suffered one or two slight apoplectic seizures, which had left him with impaired mind and memory.

I recommended blisters behind the ears, and two drops of croton oil with two drachms of castor oil, in a draught. The next morning I expected to hear that he was dead; but I found him quite conscious, speaking somewhat inarticulately, with the right side of his face chapfallen and inexpressive. There seemed no particular weakness of the corresponding extremities. The oils had been followed by copious evacuations from the bowels. The day afterwards he was

sitting up, and so well, that I took my leave.

In combination with blood-letting and purgatives, cold lotions to the head are often found useful in this disease, especially if its surface is hot. I need not trouble you by rehearsing the modes in which the application of this remedy may be managed. Blisters near or upon, the head, are also frequently of service, after due abstraction of blood, in rousing the patient from his state of coma.

Formerly, at the suggestion, I fancy, of Dr. Fothergill, it was much the fashion to give an *emetic* in the outset of the treatment of apoplexy. But we can scarcely imagine that this was not a hazardous measure; and almost sure to do harm if there was already any extravasation of blood. I believe this practice to be now obsolete; and that very circumstance is a proof that it could not have been attended with much success. I merely mention it to protest against it.

When the immediate danger has passed by, and paralysis remains, we are not to be over busy. If the palsy is to get gradually well, it must be by virtue of time, and the resources of nature. To young and strong persons I should, under such circumstances, give small and repeated doses of mercury: and in all cases I should prescribe aperient medicines, so as to keep the bowels freely open twice or thrice a day; enjoin perfect quiet; and put the patient upon very short commons. Diuretics are also very proper when the urine is not plentiful without them.

You will often have to contend against the ignorance and impatience of patients, and their friends, on these occasions. They think that *weakness* is to be remedied by *strengthening* food; by meat and drink, and tonic medicines: or if they are not so foolish as this, they will want to be electrified, or to be put into a warm bath. Certainly in the earlier states of the palsy that remains after apoplexy, none of those measures ought to be *permitted*. If after some time, when all febrile action has ceased, the palsy seems stationary, it may be warrantable and right to attempt to stimulate the torpid nerves, and to accelerate the acquirement of power by the mind over the muscular contractions: but when any means for attaining these objects are employed at all, they must be used with the utmost caution; for they are much more likely to stimulate the vascular system, and so to do harm; or even to renew the apoplectic attack. Electricity, and strychnia, are the remedies most relied upon. In cases of long-standing palsy, it may also, no doubt, be of use to try to awaken, by stimulating frictions, the dormant powers of the muscles, which by protracted inaction become *forgetful*, as it were, rather than *inca-*

pable, of their natural office : and in these old cases we sometimes are able to benefit our patient's condition by the cautious exhibition of some of the preparations of iron.

AN ESSAY
ON THE
SOURCES OF TYPHUS OR CONTINUED FEVERS.

BY JOSEPH BELL, Surgeon, Barrhead.

[Continued from page 98.]

TYPHUS is not the only febrile contagious disease which originates independent of fever, and propagates itself by this principle. We have that dreadful malady, puerperal fever, and also erysipelas, both originating from contagion in some instances, and arising independently of it in others. We perceive, then, that though there cannot exist a doubt, on the one hand, but that contagion is a source of typhus, yet on the other we see that it is highly erroneous—opposed to both analogy and fact, to maintain that it cannot be originated from any other source.

Let us proceed to examine the other sources from which it is said to arise.

1st. *Malaria*.—In the reports on the “Sanatory State of the Labouring Classes in and about London,” the doctrine of the origin of fever is strongly maintained. The following is the conclusion which Drs. Arnott and Key have formed from the reports of the various district surgeons :—“ Besides the malaria arising where nature is uncultivated, we find that wherever men congregate, and bring together the quantities of vegetable and animal substances which constitute their food, in the preparation of which there is much refuse, or where the excrementitious matters from their own bodies (being the matter of their food again rejected, and in another form) are allowed to accumulate, there is produced another malaria, often as destructive to life as the most active which dwells in the Indian jungle. The fevers called typhus, putrid, malignant, jail, hospital, ship fever, &c. are the products of this malaria, and, when once induced, the bodies of persons affected give out a contagious malaria, often more quickly operative

on other persons than the original cause.”

Dr. Southwood Smith, in Supplement No. 2 of the Reports, states that “the exhalations which accumulate in close, ill-ventilated, and crowded apartments, in the confined situations of densely populated cities, where no attention is paid to the removal of putrefying and excrementitious substances, consist chiefly of animal matter; such exhalations contain a poison which produces continued fever, of the typhoid character.”

Such, then, are the opinions of these physicians, founded, as has been already mentioned, on the reports of the district surgeons of London. But, as it has been justly observed, we live in a discriminating age—an era in which we are unwilling to adopt either the conclusions or opinions of others, without investigating the facts from which these conclusions are deduced, as well as the process of reasoning employed in their deduction. Let us take a brief survey of the reports :—

The district surgeon of Lambeth, after stating that fever prevailed to a considerable extent in that parish, observes, “The primary cause of this infection I believe to be the malaria, or effluvia, arising from the state of the drains, or stagnant filth: the heat of the sun, acting upon the mud, sends forth this kind of malaria, which, impregnating the air, is the first cause of fever.”

Mr. J. Appleton mentions, that, in a part of the district of which he has medical charge, there is a large open ditch, filled with filth, and contaminating the atmosphere in and around the dwellings of the poor. He also observes, “that the majority of the cases of sickness occurring in this district are typhus and continued fevers. . . . Considering the privations many of them undergo with respect to food, and their intemperate use of ardent spirits; that they are huddled together in ill-ventilated rooms; and that this place is the resort of Irish lodgers, who are travelling the country, it is exceedingly difficult to give an opinion how far these cases of fever have had their origin in states of the atmosphere equally affecting the crowded parts of the metropolis, and how far they have originated in the local causes above named.”

We are told by another surgeon that, in his district (comprising Homerton and Mare Street, of the Hackney Union), fever of a typhoid character is seldom absent. This, he thinks, arises from "an obstruction by a dam to a mill," which allows "a large accumulation of decaying and other matter of a deleterious nature, likely to cause an atmosphere not at all congenial to health, which, aided by, I am sorry to say, the innate want of cleanliness and care on the part of the poor, frequently gives rise to fevers of this description."

At page 19 of the Report, Mr. Foote states that, "two years past a fever raged at Red-Hill, which I attributed to the lodgment of the filth from privies, which I had removed at the time; and the same thing occurred at the Hyde, the fever prevailing there being of the typhoid type; and I consider that, unless the ditch is cleaned, the same kind of fever will prevail again; and also at the Marsh, in the parish of Great Stanmore, typhus fever lately prevailed amongst the poor."

Dr. Bowling states that, "at Hammersmith, fever has always prevailed at certain seasons among the poor, attributable in a great measure to miasma, produced by a quantity of water which had been left stagnant on the surface of the earth after brickmaking, and which, in process of time, had become full of vegetable matter."

Another report speaks thus, at page 21:—"Fever has been most severe in those courts and alleys where there is no circulation of air, such as, for instance, Johnson's Change, in Rosemary Lane, in which there are about twenty houses, in almost every one of which fever has prevailed. The disease first made its appearance there in August last." This is ascribed to the "intolerable nuisance of the overflowing of a cesspool or privy."

Another surgeon, at page 22, makes the following observations:—"The last six or seven weeks we have been called on to attend many cases of typhus fever, of a very malignant character, chiefly attacking the labouring classes residing in the dirty and most unhealthy portions of our locality. The majority attacked were those who were unable to procure adequate nutriment, from want of employment during the last inclement weather." He ascribes this prevalence of the disease to "the ab-

sence of cleanliness, crowded state of the houses, intemperance, accumulation of filth, slaughter-houses, sewers,—all these circumstances constitute the constant source of the generation of contagion."

Such, then, is the most recent evidence adduced in support of the origin of fever from malaria. The facts, however, merely prove that which is admitted by every person conversant with the practice of medicine, namely, that among the inhabitants of the ill-ventilated and filthy localities which are met with in large towns, fever prevails to an alarming extent—an extent wholly disproportionate to that which occurs among the better classes of society, who live in more airy and clean dwellings. I cannot conceive how the facts stated in the reports can be made to support any other inference. In the whole series of opinions and statements, there is no proof of a single case of typhus having originated from malaria. It is true Dr. Southwood Smith states that the formation of a common sewer, the filling up of a ditch, the removal of stagnant water, and the drainage of houses, have rendered a district healthy, from which, before such measures were adopted, fever was never absent. "This is strikingly exemplified," he further observes, "in the present healthfulness of the upper part of the Hackney Road, in which an excellent common sewer has been recently made, the neighbourhood of which is now well drained." (P. 36.) He further adduces a few similar illustrations. These circumstances, however, cannot affect the argument very much, when we take into consideration that these improvements were only of recent date, and that fever is very apt to shift its seat during epidemics. It should also be borne in mind, that improper ventilation is a very great cause of the propagation of contagion; that the poor creatures who dwell in these wretched abodes of filth and misery are in a condition highly favourable to the attack of typhus. Contagion having gained access to such hovels, will be propagated to a most fearful extent, from the number of causes which exist to favour its spread, and to increase its virulence. Hence contagion is just as likely to be the source from which fever originates in such instances as is malaria. But even putting these considerations alto-

gether out of view, there are many facts which strongly oppose the doctrine of the malarious origin of typhus. We have many instances of people exposed to the influence of malaria, of a much more intense and powerful nature than any effluvia which can possibly occur in our large towns. These people are not only in the enjoyment of good health, but are scarcely ever attacked with typhus fever.

I will merely remind you of the many facts detailed by Drs. Chisholm and Bancroft, in their writings on the subject—facts which afford powerful evidence against malaria being a source of fever. My time will only permit me to allude to the investigations of a more recent writer, now no more—I mean Parent-Duchatelet—a man of the most comprehensive intellect. He investigated this very subject with a degree of discrimination never surpassed.

At Montfaucon, in France, there is one of the most offensive manufactories that could possibly exist—a manufactory where effluvia of the most repulsive nature is daily elaborated, rendering the atmosphere insupportable for thousands of yards around the place. In this manufactory there are 12,775 horses slaughtered annually. The basins of this reservoir contain 32,800 yards of surface, and upwards of twelve acres occupied by dry matters. To this place, besides the twelve thousand horses, there are brought daily 240 cubic yards of human filth from the privies, and thirty thousand bodies of small animals, all of which are left to rot on its surface.

These animals are flayed up, and undergo various processes: the intestines manufactured into gut; some parts are made into manure; from others maggots are bred to feed poultry. Such are the disgusting operations which are carried on in this place. “In reality,” observes Duchatelet, “nothing can be compared to the disgusting effluvia produced by these establishments.” Let one figure to himself what may be produced by the decomposition of heaps of flesh and intestines, abandoned for weeks and months in the open air, and to the heat of the sun, to spontaneous putrefaction; let him endeavour to conceive the nature of the gases which can arise from the heaps of carcases that remain covered with much of the soft parts;

let him further add the emanations furnished by a soil which for years has been drenched with the blood and fluids of animals—those which arise from the blood itself, which in both the yards lies on the pavement without being able to make its escape—those of the kennels of the gut spinners and dryers in the neighbourhood; let one multiply as much as he pleases the degrees of stench, by comparing it to that which every one of us has been enabled to feel on passing near the bodies of animals in decomposition, which it was necessary to encounter, and but a feeble idea shall be formed of the truly offensive odour which emanates from this sink, the most offensive that it is possible to imagine.

Any malaria which can possibly arise from uncovered drains, crowded apartments, slaughter-houses, sewers, must sink into perfect insignificance when compared with that which proceeds from this awfully disgusting manufactory. If malaria be at all a source of typhus fever, the disease must here exercise the most dreadful malignancy. We have the evidence not only of Duchatelet, but also of Deyeux, Parmentier, Pariset, Damoiseau, and Huzard, that typhus fever is almost unknown among the workmen, who enjoy excellent health—even those who remain constantly in the tainted atmosphere. The three first-named gentlemen tell us that they found some of the workers in the best of health, who had not been off the premises for a year, and who slept in a place which they found impossible to enter, on account of the intensity of the fœtor exhaled.

Duchatelet saw many of the workmen sixty and seventy years of age stout and active. “We have taken exact notes,” says he, “respecting their fathers and mothers, and have learnt that they all died at a very advanced age, and almost always exempt from the infirmities of old age. He also found that new workmen were not subject to illness more than those who had been employed for years. Further, he states that those who are employed in the manufactory of a kind of manure called poridette, which is prepared from the filth procured at the privies, and is nothing more than dried ordure, enjoy the same immunity from typhus fever. In the report on the sanatory condition

of the labouring classes of London, we have heard the cause of fever ascribed to the overflowing of a cess-pool, the accumulation of animal and vegetable matter in a mill-pond; but in Paris Duchatelet informs us that the Bieore, a stream that runs through that city, is nearly dry in summer, and sends forth exhalations insupportable to those who pass in the vicinity: the mud, which is entirely composed of the remains of animal substances, swells when it becomes dry, puffs up, and bursts, like fermenting dough. The exhalations arising from this place are so intense that meat cannot be preserved more than a few hours during summer; and yet there are no typhus, putrid, or malignant fevers prevalent, among the thirty thousand individuals who are densely crowded together, in the part of the city through which this bed of putrescency extends. Duchatelet found the inhabitants of this place as healthy as those of the other parts of the city; and ascertained that they were not more liable to any serious disease than those who resided in other parts of Paris. If time permitted, many facts similar to those narrated might be adduced, proving the doctrine of the malarious origin of fever to rest on very slight foundation. Any evidence which has been brought forward by the advocates of this doctrine is entirely of a presumptive character, which, in my opinion, is completely overturned by the facts which I have quoted. There is, however, one circumstance which we must not overlook, namely that the condition of the workmen engaged at the disgusting employment to which I have alluded, and that of the people exposed to malaria in large towns, is not strictly analogous; the former are well fed, comfortably lodged and clothed, have plenty of animal food, though it be horseflesh; whereas the latter are badly fed—I should rather say half starved—miserably lodged; badly clothed, if clothed at all; and often of intemperate and abandoned habits. They are consequently not capable of sustaining the agency of any deleterious principle so well as people in the condition of the Montfaucon workmen. Hence it may be argued that malaria may exercise its malignant influence on the one, and not on the other. I think that this should rather lead us to consider a source of fever which the advocates

of malaria have either overlooked, or slightly glanced at, viz. privation, or, in other words, exposure to cold, want of food, and other necessities of life. This brings us to another part of our subject—the origin of typhus fever from privation.

We have seen, from the “Report on the Sanatory State of the Labouring Classes of London,” that fever was almost entirely confined to those parts of the city inhabited by the poor; that want of proper nutriment held a prominent feature in the character of those affected by the disease.

Poverty prevailed to a most alarming extent in Ireland during the years 1817, 18, and 19, when that country suffered so much from epidemic fevers. The harvests were bad, and the produce of the land was not properly matured, so that food was very deficient in quantity, and of unwholesome quality.

Drs. Barker and Cheyne state that from the year 1721 to 1728 there was scarcely a case of fever to be met with in Ireland. “After the latter year three bad harvests occurred in succession; provisions rose to an extravagant price. . . . From 1728 fever gained ground, and continued to be epidemical to 1732. The season following 1739 was one of great severity. Fever, which had been frequent, again became epidemical; it did not cease in the winter, and increased most alarmingly in 1741. Dr. O’Connell states that the mortality during this epidemic was 80,000. . . . ‘The year 1800 was nearly as unfavourable to the fruits of the earth as 1799.’ Great scarcity prevailed. The price of bread and potatoes, both of bad quality, together with every necessary of life, was raised beyond all precedent. In the autumn and winter of 1800 the inhabitants of this kingdom suffered from a contagious fever, in which the troops still continued to participate. . . . The epidemic had reached to the most alarming extent at the commencement of the autumn of 1801. Shortly after this, however, it began to decline, “but not before the good effects of an unusually abundant harvest, in again furnishing provisions of all kinds to the poor at a moderate rate, had been felt.” In 1816, the harvest proved exceedingly bad. It was not only defective, but late. Much corn remained uncut, and a greater portion was only cut in Octo-

ber and November of that which was saved. The potatoe crop was a failure. Besides these calamities, fuel was scarcely to be obtained; and trees were stripped of their leaves and bark by the starving population for food; whilst the branches were used for fuel. The following year, 1817, was equally unpropitious; nor was 1818 much better. To these distressing circumstances were added the evils of commercial distress—extensive failures in trade, and lowering of wages. The following extract from Dr. Gratten will show the effects of this awful state of privation:—"The present epidemic," says he, "is principally to be referred to the miserable condition of the poorer classes in this kingdom; and so long as their state shall continue unimproved, so long will fever prevail; probably not to its present extent, but certainly to an extent sufficient to render it at all times a great national affliction. He also observes at another place, "Next to contagion I consider a distressed state of the general population of any particular district the most common and most extensive source of typhoid fever, whether this has been the result of war, or been produced by the more gradual progress of domestic misfortune."

Professor Alison, in his valuable work "On the Management of the Poor in Scotland," after describing (in a very graphic manner) the shamefully destitute condition of the poor in Scotland, alludes to the miserable state of the people in Ireland during the years 1817-18-19, as the source of the epidemic which ravaged Ireland at that period. He observes: "That it is always in persons suffering, or who have lately suffered, similar privations and sufferings, and the mental depression and despondency which naturally attend them, that continued fever becomes extremely prevalent, is fully established by the history of all considerable epidemics. The elaborate work of Drs. Cheyne and Barker shows that this has been strictly true of all the great epidemics which have appeared in Ireland since 1700; each of them lasting fully two years, viz. in 1708, 1720, and 1731, in 1740-1, (after the great frost in 1740), in 1800-1, after the rebellion; the transference of the seat of government to London, and the scarcity of 1799, 1800, and again in 1817, after the tran-

sition from a state of war to a state of peace, and the scarcity of 1816-17.

That work contains reports from the most eminent physicians in all parts of Ireland on that great epidemic, all agreeing in the statement that the poor were the greatest sufferers; and the fever seemed to rage among them in a degree proportionate to the privations they had endured. In Ireland accordingly, at least during the present century, as the condition of the poor has been decidedly worse than either in England or Scotland, so contagious fever has never ceased to be more generally prevalent. The same observation applies to the epidemic fever in London after the scarcity of 1800, (the last great epidemic which has occurred there) to the great continental fever of 1813-14, which followed the track of the French army retreating from Russia, but never made much progress in the victorious allied army; to the epidemic fever of 1817 in Italy, consequent on the scarce year of 1816; to the epidemic which affected the British army in Holland, after the disastrous retreat from Flanders in 1794; in Portugal, after that from Bruges in 1812; and to that which nearly decimated the British Legion at Vittoria in 1836."

[To be continued.]

FEMORAL HERNIA IN THE MALE.

To the Editor of the Medical Gazette.

SIR,

IF you think the following case and remarks sufficiently interesting for publication in your excellent periodical, I shall feel obliged by their insertion.

I am, sir,

Your obedient servant,

T. C. BUCHANAN.

Gloucester, March 31, 1841.

Thomas Price, labourer, æt. 65, who had been under my care a short time previously for an injury of the eye, was admitted into the Gloucester Infirmary on the evening of April 19th, 1840. He stated that during a fit of coughing, on the 17th, a swelling took place suddenly in his left groin; that vomiting immediately followed, and had continued at intervals ever since; that he had suffered a great deal of pain in the

bowels, but had not been able to procure any evacuation from them. On examination, a small enterocele was perceptible in the left groin, but it was difficult to determine whether its neck was above or below Poupart's ligament, on account of the margin of that ligament being very imperfectly defined. The parts in the groin were not so lax as they usually are; and although the neck of the tumor appeared to be situated deeper than in a case of inguinal hernia, and rather to the outside of the spinous process of the os pubis, yet manual examination did not entirely relieve my mind from doubt as to whether the hernia emerged from the abdomen above or below Poupart's ligament; whether, in short, it was inguinal or femoral. The abdomen was tympanitic, and painful on pressure: vomiting occurred occasionally. The pulse was 80, and easily compressible. After waiting for about two hours for the application of cold to the tumor, the administration of a purgative enema, the passing of the œsophagus tube, and the employment of the taxis, which was tried unsuccessfully, it was deemed expedient to perform the operation, as the tension of the tumor, and of the surrounding parts, precluded the probability of returning the hernia without it. An oblique incision was made over the tumor, and an enlarged gland situated in front of it was divided; the dissection was then proceeded with, and the sac, which did not contain any fluid, was cautiously opened. The intestine was then clearly found to emerge from the femoral ring; and the stricture, which was tight, and situated in the front part of it, was divided upwards by a probe-pointed bistoury passed along the groove of a director. The intestine, of a deep chocolate colour, was easily returned into the abdomen; and two sutures, with plaster, were employed to close the wound.

April 20th, 11 A.M.—Has not slept; no vomiting, but the bowels have not been opened; abdomen tense and tender; pulse 80, and compressible; tongue white.

Appl. Hirud. xvij. abdom. Sumat Hydr. Chlor. gr. iij. statim. R Magn. Sulph. ʒj.; Aq. Menth. Pip. ʒviiij. S. ʒj. omni horâ. Tea and arrow root.

10 P.M.—Bowels have been opened once freely, but he has since vomited; 698.—XXVIII.

abdomen rather less tender on pressure; tongue loaded with a whitish fur; pulse 84, and compressible; cough troublesome.

Omittr. Mist. S. Hydr. Chlor. et Ext. Hyos. aa. gr. ij. 3tiis horis. Fetus abdom.

21st, 11 A.M.—Has slept at intervals, altogether about two hours; has vomited twice, and now feels sick; abdomen rather less tense and tender: about two hours since has had a purgative enema, which has not yet returned; pulse 80.

Rep. Pil.

10 P.M.—Has been so sick as to refuse taking the pills, of which he has only had three; bowels have been opened, but not freely; pulse 80; tongue furred; abdomen rather less tender, but quite as tense; he still feels sick, but wishes for a little broth, which was allowed him.

Rep. Pil. addit. Ext. Coloc. C. gr. ij. cuique. The wound was dressed in the morning, and looked well.

22d, 2 P.M.—Has slept a little in the night, and this morning a purgative injection was given, which produced a pretty copious evacuation; he was sick in the night, and has just now vomited. Abdomen slightly tender, but still tense and tympanitic; pulse 78, rather full; tongue furred; cough distresses him.

Appl. Empl. Lyttæ Epigast. Rep. Pil. addit. Aloes, gr. j. cuique dosi. To have an injection in the evening if the bowels are not previously opened.

10 P.M.—Has had an injection, and the bowels have acted pretty freely; the vomiting has ceased: he has taken a pint and a half of broth during the day, and has slept an hour at a time; feels better; pulse 84.

To take one pill in the night. Cont. Fetus.

23d, 11 A.M.—Bowels have been twice freely opened in the night; abdomen soft, and not tender to the touch; pulse 72; tongue moist; feels comfortable. He appears to be now going on well, after having taken

Hydr. Chlor. gr. xxvij.; Ext. Hyos. gtt. xxiv.; Ext. Coloc. c. gtt. xvj. et Aloes, gtt. iv.

He was discharged, May 21st, quite well from the hernia, but still troubled with his cough.

REMARKS.—Although, in the pre-
L

ceding case, it was difficult to decide positively, by manual examination, whether the hernia was or was not femoral, yet I should have felt less hesitation in doing so, if the belief of the rarity of femoral hernia in men had not been so strongly impressed on my mind: more so than the observation of our great authority, the late Sir A. Cooper, warrants. I perceive, indeed, from the last GAZETTE, that Professor Cooper apprehends that crural hernia in the male is more frequent than the illustrious Scarpa supposed; and as a knowledge of its comparative frequency, with the more usual form, may prove of some utility in practice, and can only be acquired by extensive experience, I was glad to find the Professor's attention had been directed to the subject.

It may seem strange that aloes was given to the patient, but as the tympanitic state of the bowels at the time of its exhibition appeared to me to arise more from atony than inflammation, as we sometimes see in cases of colic, I did not think aloes inadmissible.

ON HIP DISEASE AND LUMBAR ABSCESS.

BY WM. OLIVER CHALK, ESQ.

Resident Surgeon to the Royal Sea-Bathing Infirmary, Margate.

THERE is another circumstance attendant on the position of the limb in psoas abscess which tends greatly to obscure the diagnosis, especially when the first stage is somewhat advanced: it is the unusual prominence of the great trochanter, produced by crossing the limb affected over on the sound one. This appearance so much resembles disease of the articulation*, that it is often extremely difficult to form a right judgment; yet, generally speaking, this projection of the trochanter may be observed long before the matter quits the pelvis, and the absence of pain on percussion over the articulation will form the best means of distinction. It is, however, absolutely necessary to satisfy ourselves, if possible, on the point, since it involves a question of practice; for

* The manner in which partial or complete dislocation of the head of the femur takes place from position has already been described in the cases of J. G. Butter and W. H. Elsworthy, at page 26 of the MED. GAZ. March 26th.

if there be consecutive disease of the joint, the patient (independently of any other consideration) must not be permitted to move about as freely, as if the psoas muscle only were affected. These consecutive affections of the articulation are commonly fatal, especially among children—a fact which cannot excite our surprise, when we consider the extent of structure involved, the great destruction of parts that ensues, and the desperate severity of the local and constitutional symptoms. I do not remember to have seen a single instance of recovery under these circumstances. The case of M. A. Upton, æt. 6½, now under treatment, is one in which disease of the articulation exists, in conjunction with lumbar abscess, and seems to all appearance as if it would ultimately do well. The affection of the articulation, however, cannot be considered as consecutive in this instance, as it arose from a fall, whilst the former disease was only in its incipient stage. The following is the history of the case:—In the autumn of 1838, she suffered from a severe attack of scarlet fever: soon after her recovery she was observed to stoop, particularly when walking. January 6th, 1840, she began to complain of pain in the knee, which came on at night, and was always worse in the recumbent posture; she lay with the thigh flexed on the pelvis, and rested the limb affected on the sound one. Whenever any one attempted to move her, she screamed with pain. At this period surgical advice was obtained. Leeches, blisters, and an issue, were applied in the vicinity of the articulation; she was not allowed to move about; a suitable medical treatment was adopted; and, after a time, an amelioration of the symptoms ensued. She was admitted, as an in-door patient, May 14th. She did not then complain of pain in the hip or knee; nor was there any on flexion; but it was produced by extension, and rotation outwards, in the latter articulation. She limped very much; her health was delicate; she complained of pain on pressure in epigastrium; the pulse was quickened, and she suffered under febrile accessions; tongue furred; alvine excretions unhealthy. The hepatic treatment was pursued, and her general health soon improved. As she did not walk any

better, a pair of crutches was ordered. From this time she went on remarkably well, and, from having been a spare delicate child, had become strong and healthy, and had gained flesh. She suffered no pain, and seemed to be rapidly recovering. In the month of August, she unfortunately slipped whilst walking on her crutches, and in falling struck the hip and knee: from this time (although suitable remedial means were adopted) she began to experience severe pain in both articulations, flexion, extension, rotation, &c. considerably increasing it; she was entirely confined to her bed, and no apparent amelioration in her symptoms took place up to the period of her departure, October 6th. I saw her again in the November following. She was then suffering from hectic fever and constant cough, with profuse muco-purulent expectoration. The hip-joint was acutely sensitive; the slightest percussion over the trochanter caused excessive pain, as did any attempt at movement; there was a hard circumscribed fulness of the nates. Three leeches were applied to the epigastrium, and a bread and water poultice for an hour, to promote the bleeding. Aperients of calomel and jalap were given once or twice a week, together with occasional enemata of warm water. To alleviate the cough, which was very troublesome, I prescribed the following mixture:—

℞ Creasoti, ℥xvj.†; Tinct. Card. Co. ʒiij.; Essen. Pap. Alb. ʒj.; Sacchar. Alb. ʒij.; Aquæ Destillatæ, ʒviiss. Mft. Mist. sum. cochl. duo ampla ter quotidie aut urgenti tusse.

I am induced to give the formula, as I have frequently found it of essential service in allaying the cough of patients in the latter stage of phthisis as well as in hooping-cough. A lotion, consisting of Iodine, two grains, and four grains of Iodide of Potass, dissolved in a pint of distilled water, was constantly applied to the articulation. In the course of a fortnight or three weeks the hectic symptoms disappeared, the hip became less painful, the cough entirely sub-

sided, and it has not since returned. As soon as the hectic fever was subdued she took Decoct. Cinchonæ, ʒiss. three times daily: whenever the constitutional effects of iodine became at all apparent, the lotion was discontinued for a short period.

I have just now examined her (April 9th): she is able, and has been for three weeks past, to move about on crutches; the limb is shortened, the toe scarcely touches the ground when in the erect posture; the circumscribed hardness of the nates has increased: she complains of pain on flexion and percussion. Her general health is greatly improved.

It occasionally happens that lumbar abscess forms on both sides of the spine; under these circumstances the patient's suffering is extreme, and the disease is commonly fatal. I can only call to mind a single instance to the contrary; it was the case of John Dimblebee, admitted in the seasons of 1828-29. The spine was not affected; the complaint had existed for several years, and, although there was considerable contraction about the hip and knee-joints, his recovery was complete. I saw him two years afterwards in perfect health, and scarce any trace of the disease existed. In the case of Jane Barnes, admitted May 12th, 1836 (where the complaint terminated fatally after she left the infirmary), the deformity produced by the exit of matter in the vicinity of both articulations of the hip, was indescribable, and her sufferings intense. As the exciting causes of psoas abscess differ in no respect from those of hip disease,* it will be unnecessary to enumerate them here. The general treatment is likewise the same, and need not be repeated.

I will, however, take this opportunity of adverting to a point of great practical utility, namely the necessity of relieving the lower intestines by enemata, as the peculiarity of the local disease renders the patient liable to suffer whenever accumulations take place in the colon. The presence of scybala is often productive of severe pain—a circumstance which is readily accounted for in the relative situation of the psoas muscle and colon, and one that should be carefully borne in mind throughout the whole course of the disorder. The fol-

† To form this mixture the Creasote should be mixed with Tr. Cardam. Co. and Sacch. Alb.; the distilled water should be added by degrees, and repeatedly shaken until a clear mixture is formed. The quantity of creasote may be increased if necessary.

The Essen. Papav. Alb. is a sort of fluid extract prepared by Messrs. Westwood, of No. 16, Newgate-street.

* Vide Medical Gazette, March 26, p. 28.

lowing case is highly interesting, and shows the necessity of attention (if proof were wanting) to this particular point.

John Cliver, æt. thirty-three, admitted May 9th, 1839, suffering under psoas abscess on the right side (which had found an exit in the groin), consequent on a kick from a cow about nine months previous: he was a shepherd. The discharge was rather copious, and of an unhealthy character: his general health was greatly impaired; he was labouring under hepatitis. The bowels were costive; he complained of great thirst; the skin was hot, and his pulse accelerated. He had great pain between the shoulders, and suffered acutely from pressure in epigastrio. He had frequent palpitations of the heart: the abdominal aorta was observed to pulsate violently at times. He occasionally complained of pain in both iliac regions, especially in the right: whenever this was the case, the discharge from the abscess increased, and the bowels were more inactive. Upon observing this I constantly relieved the symptom by aperients, or enemata. It is unnecessary to follow out the treatment, which extended over a period of upwards of four months, at the termination of which the abscess had nearly healed, but had formed another small opening, just above the anterior superior spinous process of the ilium: the discharge was very slight. He had so far recovered his general health and strength as to be able to walk with a crutch and stick, and was about to return home. On Sept. 29th he was seized with violent spasms at 11 o'clock in the morning. Antispasmodics were administered. Hot bottles of water were applied to the feet and pit of the stomach: hot brandy and water was given at intervals. The patient was greatly alarmed, and frequently expressed his conviction that if the pain continued he should die. The spasms soon subsided under this treatment, and he experienced no return of them until 3 o'clock P.M., when I was called to see him. He was suffering extreme agony: his countenance was exceedingly pallid; the surface of the body was covered by a profuse and cold perspiration; his breathing was accelerated; the pulse rather full and quickened: he repeatedly asserted that the pain was too

great to bear, and that he should die. I endeavoured to encourage him, and ordered a repetition of the remedies above described. I left his bedside for the purpose of procuring him a draught; but although three minutes could not have elapsed from the time of my absence until my return, he expired during the interval. A post-mortem examination was made 23 hours after death; every viscus of the body was carefully examined, and found in a healthy state, with the exception of the liver, which was engorged, and presented in some places a slight nutmeg appearance. There was an abscess occupying the right psoas muscle, which descending had formed an orifice just above Poupart's ligament, and another near to the anterior superior spinous process of the ilium: the sac was nearly obliterated, and the walls of the abscess were semi-cartilaginous, and several lines in thickness. The caput coli was very closely adherent to the upper surface of the sac by strong adhesions. It was at this point that accumulation had constantly occurred during life.

In lumbar abscess it is of the greatest importance to recognise the incipient symptoms, for if the disease be independent of vertebral caries, or unconnected (as is often the case) with any organic changes of the liver or respiratory organs, we shall generally prove successful in relieving them, or at least in modifying and restraining the suppurative process; and if we cannot altogether arrest the disease, it may be rendered far less formidable by a careful adoption of the constitutional treatment already described. Provided the pain be not excessive, the patient should be made to walk on crutches as early as possible: great care and attention being paid to have these of a proper length*, (as in morbus coxæ); thus avoiding the contraction which commonly takes place about the articulations of the hip and knee, upon which the shortening of the limb in these cases depends. It is the opinion of many of our most able and experienced surgeons, that when vertebral caries exists, the patient should be confined to the recumbent posture on a bed or couch†. I cannot say that I ever

* The patients themselves, particularly children, if left to their own choice, will almost always select crutches which are too short.

† The prone couch has been used of late years,

found the practice successful, and it seems to me open to the following objections:—

1st. The mischief to the general health from want of exercise.

2dly. The loss of muscular power, which is always great under such circumstances.

3dly. The limb (in spite of every effort to the contrary) acquires a bad position, and, as we have already seen, the pain is always more urgent in the recumbent posture.

It has been asserted by those who advocate the system of rest in scrofulous caries of the spinal column, that by allowing the patient to move about in the erect position, the superincumbent weight of the body is hurtful; that the caries is increased by it, and the reparative process retarded. This mode of reasoning, however, seems to me fallacious. I think that too much importance has been assigned to the effect of superincumbent weight; for it may reasonably be asked of what use are the muscles unless to support the spine and to retain it in its natural position, and by their influence to sustain the weight of the body, as well as to minister to the locomotive functions. So long as the strumous diathesis remains unremedied, the reparative organs will be wanting; this must be at once apparent, if we admit that an impaired state of the nutritive functions (of which there can be no doubt) is essential to the formation of scrofulous habit; and, therefore, that whatever tends to alleviate it will best secure the well-doing of the local malady; but whether or not the argument be correct, long experience has taught me that provided the general symptoms be combated successfully, amelioration of the local mischief will most certainly follow. This circumstance should never be overlooked or forgotten in entering on the use of topical remedies.

The only objection that can be made, in my opinion, to allowing the patients to move about during the earlier stage, is the uncertainty of gait that accompanies it, especially among children, which renders them liable to falls, and consequently to an aggravation of the disorder; this, however, is in a great

measure remedied by the use of crutches. The exercise thus obtained is so beneficial to the general health and position of the limb, that it amply compensates for the risk incurred.

The case of Mary A. Heard, æt. 28, admitted July 13th, 1836, suffering under incipient lumbar abscess, is a good illustration of the benefit derived from crutches, as regards the position of the limb; even under unfavourable circumstances. The case had been treated as an affection of the hip. The articulations of the hip and knee were greatly contracted. This state was rendered more permanent by a high shoe which she had worn for some time. She complained of pain in the hip and knee, particularly at night. She had great tenderness in the region of the liver. The digestive functions were much deranged. These symptoms were accompanied by a teasing cough.

I reported the case to Dr. Dennison, under whose care she was admitted, and with his concurrence she was placed under the hepatic treatment. The high shoe was removed, and she was ordered to walk on crutches. She underwent a course of warm bathing, &c. and by Sept. 3d was discharged cured, having entirely recovered the use of the limb.

The use of counter-irritants, such as moxæ issues, &c. at any period of the disease, is open to the same objection as in morbus coxæ; they are only so many means of aggravating the patient's suffering. I had an opportunity last summer of seeing a youth of 19 years of age who was suffering under lumbar abscess in its suppurative stage, and which had found exit at the side of the spine: the discharge was profuse. On the opposite side was placed a deep issue about two inches in length, which was in a state of great irritation. He was in the last stage of phthisis. Could any reasonable hope be entertained from such treatment, and under such circumstances? Yet such cases are of constant occurrence. The warm salt-water and douche baths will be found useful during the incipient symptoms: the latter should not be used if there be great pain. When once the abscess has made its exit from the pelvis, and begins to form on the inner side of the thigh, or beneath the gluteal muscles, our attention should be more than ever directed to the care of the patient's

and I am informed in some instances with benefit to the disease; but, from cases that have come under my observation, it has not only been of no use, but exceedingly prejudicial.

general health. The ioduretted lotions applied over the tumor will now be of use, especially if there be any heat of the parts. The ointments of iodine and iodine of potass, and iodide of mercury also, form excellent applications; but they must not be used too freely: by these means we may be enabled to moderate the secretion of pus, and secure a more rapid recovery when the abscess opens. I have remarked elsewhere that early opening of the abscess is prejudicial; and that it is always advisable to allow it to break of itself. This rule applies especially to accumulations of pus in the psoas muscle. I have sometimes had occasion to observe that, when the sac has been large, the quantity of matter secreted has varied according to the state of the patient's general health. If any circumstance tended to derange this, an increase was generally perceptible; but when it was remedied, an obvious diminution in quantity took place. I was for some time inclined to believe myself in error in this respect, but repeated and careful observation has assured me of the fact. The following cases shew to what extent pus may be secreted and reabsorbed, under careful attention to the general health.

Jane Redding, æt. 17, admitted Aug. 30, 1837, suffering from psoas abscess on the right side, and an ulcerated state of the foot, with caries of one of the phalanges. The accumulation of pus was enormous, and occupied the inner part of the thigh, which was nearly as large round as her body; the sac could not contain much less (upon a moderate calculation) than a gallon. She complained of pain in the hip and knee, and suffered severely when she attempted to move about. She was labouring under hectic symptoms. The usual treatment was adopted. She left October 16th. Her general health was greatly improved, and the abscess considerably diminished in size. She returned to the infirmary May 18, 1838; her general health had continued good during the winter; the sac had decreased, and the foot was healed. She returned home October 16, in excellent health: the abscess now formed a circumscribed tumor of some size at the upper and inner side of the thigh, which seemed as if it would be ultimately absorbed.

Thomas Brenchly, æt. 23, admitted

May 19 1838, suffering from posterior curvature of the spine, with consequent psoas abscess, which had descended beneath Poupert's ligament on the inner side of the right thigh, forming a large tumor. He complained of great tenderness on pressure in the epigastrium; the skin was yellow, and the conjunctiva of either eye suffused with bile: the local affection originated in a sprain four years previous. The hepatic treatment was adopted, and by the time he left the infirmary, October 9th, the tumor had entirely disappeared, and I could not, by the most careful examination, detect the slightest fluctuation. His general health had undergone great improvement.

Thomas Whitehead, æt. 24, admitted May 13th, 1840, suffering from an accumulation of matter on the upper and inner side of the left thigh consequent on psoas abscess. He had been thus affected eighteen months. Fluctuation was very perceptible low down on the inner side of the thigh: his general health was not much impaired. He left the infirmary October 23d, greatly improved in every respect. The accumulation was scarcely observable at the time of his departure.

W. O., æt. 8, admitted as an out-door patient May 22d, 1840, labouring under psoas abscess in its incipient stage. He complained of great pain in the hip and knee, which was worse at night. He was suffering from hepatic congestion; the tongue was furred, pulse quick and small, alvine excretions irregular. Not long after his arrival I observed a fullness on the upper and inner side of the right thigh, which soon increased, and proved to be an accumulation of pus. The pelvis was much canted up, and thus gave a greater appearance of lengthening to the limb than usual. The usual plan of treatment was followed, and he left the infirmary October 23d, greatly improved in his health. The abscess had been entirely absorbed. I have twice had an opportunity of seeing him during the winter, and he remains quite well. Other cases might be related bearing on this point, but it appears to me that the foregoing are sufficiently demonstrative of the inexpediency of early opening; for if nature will promote a cure, it is far better than the employment of artificial means; whether matter be liberated from a chronic abscess of a large size

by incision, or an outlet form of itself, it should never be forgotten that this is always fraught with more or less risk to the patient. In the latter instance, however, there is assuredly less than in the former. Provided we continue our attention to the general health, the longer the abscess is in forming an exit, the more probability will there be (generally speaking) of a happy result.

[To be concluded in our next.]

CLEFT PALATE.

To the Editor of the Medical Gazette.

SIR,

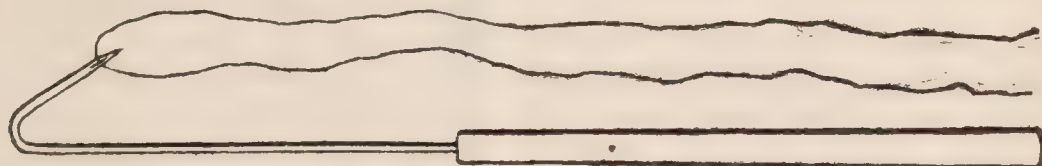
I TAKE the liberty of sending you an account of a case of cleft palate, in which an operation was performed, with as much success as could have been anticipated, when the large size of the interval between the edges of the tissue be taken into consideration. The brief narration of such cases is not unattended with advantage, and the means of dissemination of the result of experience in the treatment of many of the ordinary forms of surgical disease, through such publications as your own, lead one to record individual examples of success, and which cannot well be detailed and given to the public in any other manner.

A little boy, 15 years of age, was born with the defect alluded to, accompanied with hare-lip; the palatine processes of the superior maxillary and palate

bones of the left side being also deficient, nearly as far forwards as the alveolar process. The ordinary operation for hare-lip was performed at the London Hospital during infancy.

I was induced, at the request of the parents, to make a farther effort to rectify an evil so detrimental to him, from his consequent defective pronunciation.

The ordinary steps of the operation differ but slightly from those frequently adopted; but a slight detail of them may not be uninteresting. The extremity of each half of the uvula was successively seized with Asseldini's forceps, and with a spear-pointed knife a slice of the edge of each half of the fissured soft palate removed. The instrument next employed was one of extreme simplicity, and which answered perfectly. It consisted of a simple silver style, of about three inches in length, fixed firmly in an ebony handle, and bent at its extremity to the curve represented in the drawing: at the end of this, a steel point, with an eye capable of holding a moderate-sized ligature, is affixed. The advantage of its being made of silver is, that it is capable of being bent to any curve, to suit the necessities of the case. There was no difficulty whatever in passing the end of the instrument, armed with the ligature, through the right flap of the velum, the point of the instrument being carried from behind forwards, so as to be seen projecting into the mouth, and with a long pair of elastic forceps the ligature was laid hold of on the *concave* side of the needle, and drawn forwards; the instrument was then withdrawn, the



ligature of course being still held in its eye, and was then turned to the opposite side, and made to traverse in the same manner—namely, from behind forwards—the left flap. The object was now to disengage the ligature completely from the needle, which could only be done by seizing it on the *convex* side; a point of no great difficulty to accomplish. The needle was then entirely withdrawn. As many ligatures as the surgeon thinks

fit may in this manner be employed. In the case now under consideration, three, which is the usual number, were used; and the parts were brought together by the indices fingers. As there was considerable stretching of the soft palate, from the immense size of the aperture, a longitudinal incision was made on each side through the palate, after the method advised by Dieffenbach, and the tension was thereby most materially diminished.

The union was exceedingly good, but not quite perfect.

An artificial palate can, I have no doubt, now be worn; but it is found necessary to attach this to the teeth; and as the canine teeth are gradually descending, Mr. Craigie, into whose hands he has now fallen, is waiting until they are so far advanced as to become fixed objects for its attachment.

The only difficulty, and that is one of a comparatively trifling nature, which can occur in the employment of the needle, arises from the needle, after being withdrawn through the second flap, being found still implicated in the ligature; and all that is requisite to rectify this error is, either to remove the ligature on one side, and re-introduce it, or to draw it through the mouth, cut out the needle, and tie a knot in the ligature, which will, on the securing of the ligature, be found behind the wound, and which would afterwards readily drop out when, after adhesion had taken place, the anterior knot was cut away. This accident occurred in the introduction of one of the ligatures, but occasioned no inconvenience.—I am, sir,

Your obedient servant,

JOHN ADAMS.

31, New Broad Street,
April 1841.

REMARKS ON MR. T. B. CURLING'S
NEW METHOD OF
CURING PARALYTIC PTOSIS BY
OPERATION.

To the Editor of the Medical Gazette.

SIR,

IN your journal of the 26th instant, Mr. Curling has favoured the profession with the details of an operation successfully performed for the cure of paralytic ptosis. A somewhat similar operation, he informs us, was undertaken by Mr. Hunt, of Manchester, in a case of traumatic ptosis of one lid consequent upon a division or laceration of the levator palpebræ muscle; and he seems to consider it equally applicable to the one form of the affection as the other. The subject of the operation, in Mr. Curling's case, was a gentleman affected with double ptosis and divergent strabismus from palsy of the muscles supplied by the third

pair of nerves; and his condition, which is so well described by Mr. Curling, was such as fully to warrant the interference of a surgeon. But, sir, in admitting the propriety of operating on one eye-lid when both are affected, is a surgeon justified in having recourse to the operation when one only is paralyzed?—a case of far more frequent occurrence. I think not. When palsy affects the muscles supplied by the third pair of nerves, the iris also participates in the same condition, and the pupil is dilated; and where the paralysis is as complete as in the case described by Mr. Curling, largely dilated, and incapable of extraction under the strongest stimulus of light, I have ever regarded this conjunction of the dilated pupil, with palsy of the upper lid, as a most admirable and provident contrivance for the protection of the retina. Had the orbicularis palpebræ, instead of the levator palpebræ, received its nervous influence from the same source as the iris and the three recti muscles, what would have been the condition of a patient in whom these parts were paralysed?—unable to close the lids or to roll the eye under them, (a movement which, in part, supplies the defect of the patent state of the eye-lids in cases of paralysis of the portio dura) the pupil dilated, immobile, and incapable of excluding a too great abundance or intensity of light: either corneitis, with its attendant consequences, ulceration or sloughing of the tonic, and escape of the humours, would have resulted, or retinitis, terminating in total blindness, must have ensued. It behoves us therefore, in recommending an operation for the alleviation of present distress, not to lose sight of probable ulterior consequences, and to be careful that, in having recourse to analogy as our guide, we do not overlook those apparently minor circumstances in which the analogy does not hold, but which must be taken into account, before we can confidently predict, or rationally hope for, success. These remarks are not made in a spirit of criticism, but are intended rather as a caution to those more zealous than discreet members of our profession, who, in their eager scramble for cases to operate on, pay too little regard to their own inexperience or their patients' welfare. Fortunately cases of paralytic ptosis are much more

rare than those of squinting and stammering, but should some unfortunate victim of this disease fall into the hands of an inexperienced operator, and too large a portion of the upper lid be excised, either ectropium would be produced, or the remaining portion of the lid might be not sufficient to cover the eyeball, unless by strong contraction of the orbicularis palpebræ. In either case the probable result would be corneitis and its consequences, from inability of the muscles to roll the eye under the upper lid; or retinitis, from inability of the pupil to contract and exclude too great a degree of light.

I am, sir,
Your obedient servant,
C. HOLTHOUSE,
Surgeon to the St. Pancras General
Dispensary.

13, Keppel Street, Russell Square,
March 31, 1841.

MEDICAL GAZETTE.

Friday, April 16, 1841.

"Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri; potestas modo veniendi in
publicum sit, dicendi periculum non recuso."
CICERO.

IMPROVEMENT OF MADHOUSES.

THE division of labour has often been panegyricized by political economists; and it is observed by Adam Smith that the number of hands employed in the making a pin, contribute not only to the excellence but to the quickness of the work. In fact, by this parcelling out of the manufacture among many artificers, more and better pins are made, than if each man attempted to make entire pins. Just so in the reform of church and state, the business is undertaken with the greatest vigour by those who confine themselves to one branch of the subject; who, as it were, keep for ever at the head of the pin, careless of the silvering or pointing. Dr. Caleb Crowther, on whose opinions we commented a few years ago*, appears to be one of

these reformers, who are chiefly occupied by a single topic; and though it must be confessed that his zeal sometimes outruns his discretion, yet few can read his censures without deriving some useful hints from the perusal. Dr. Crowther, indeed, as we showed in our former article, is one of the angriest of critics. He reminds one of the description of that fierce satirist, whose pen was like a drawn sword, and whose invectives produced the most violent emotions of terror in the breasts of those who were conscious that they had deserved them*.

Like every eager reformer, he not only shows with much vehemence that things are wrong, but is satisfied that he could set them all quite right; whereas, though it is tolerably easy to point out the incongruity and incoherence of all human systems, we doubt if even Caleb Crowther himself could substitute perfection in their stead. Nothing is more easy, for example, than to show, as he did in his former essay, that there are defects in the present method of electing the medical directors of asylums. But, as we then answered, there are obvious objections to the plan proposed by Dr. Crowther, of appointing them by a Minister of Health, assisted by a medical board. Elect them, if you please, by the votes of five hundred governors, or by a medical jury and a *concours*, and objections—good sound objections—will not be wanting. Moreover, without believing, as Dr. Crowther does, that the most ignorant men will often produce the best testimonials, we can easily understand that the man who is theoretically the most fit, may not, in vul-

* *Ense velut stricto quoties Lucilius ardens
Infremuit, rubet auditor, cui frigida mens est
Criminibus, tacitâ sudant præcordia culpâ.*

When angry Crowther, as with flaming sword,
Rages, his hearers dread each bitter word;
Recorded crime the guilty heart affrights,
And sad mad-doctors tremble as he writes.

gar phrase, *work* the establishment the best.

Again, in his present essay*, he declares against the luxuriousness of directors of asylums, and sees much mischief in their dinner-parties. A feast of three courses occupies the servants of the institution, and detains them from their regular offices; and on one occasion a director "on hospitable thoughts intent," when summoned to a maniacal patient, did not go to see him till he had broken two hundred and thirty-six squares of glass. It is quite clear that it is arithmetically and morally possible to make a director so rich that he shall be too great a man for his place; whether this has ever been done, and especially whether it is the case at the West Riding Asylum, on which Dr. Crowther's heaviest censures are showered, we know not. We should be glad to hear what his salary is. A well-paid medical officer in any public institution is so singular an exception, that the whole of this rare class existing in England might be counted on one's fingers; and to hear of such an instance is as refreshing as to see a warm colour after long gazing on cold tints. But though it would be easy enough to avoid this pleasant fault, and give the resident officer of an asylum a salary of sufficient scantiness, in any degree from the bare subsistence of an assistant-surgeon in the army, down to the pretext of pay bestowed on a Union officer, or the avowed nothing given to a dispensary doctor, yet this frugal plan has its disadvantages, and is as unjust to the patients as it is to the physician. Instead of procuring the cheerful services of a man of talent, who felt that he had at length obtained something like a reward for the labours of a life-time,

we should be liable to a continual alternation of the dull and the disappointed; the former unable, and the latter hardly willing, to perform the duties of their office efficiently. When a medical place is accepted with little pleasure, and vacated with great alacrity, *delirant reges, plectuntur Achivi*—it is the rulers of the institution who err, but the poor patients who suffer.

Again, the Director of the West Riding Asylum, according to Dr. Crowther, makes presents of the produce of the garden to his friends, and this is forbidden by the printed rules of the Institution, though perhaps countenanced by the tacit permission of the visiting magistrates. The rules also say, that "any officer or servant found making a perquisite of any kind whatever, will be instantly dismissed." It certainly would seem desirable, either to alter the statutes, or make the practice agree with them; for as the rules direct the vegetables not consumed in the Asylum to be sold for its benefit, each cucumber presented to the neighbours is a loss, though a small one. It might be said, indeed, that the question was, whether these infractions of the law were so frequent as to render it advisable for the authorities to interfere; or whether they were so rare and minute as to make interference inexpedient, and almost ludicrous; while to this it might be answered, that perquisites, like fame, are *parva metu primo*, but often rise to great heights from modest beginnings. So that it is one of those practical questions of constant recurrence, where the difficulty is to draw a line between small and great, and decide with mathematical exactness where the important begins. Who, for instance, would grudge a visitor to an hospital a dose of peppermint and rhubarb; yet who would allow him to be constantly supplied with medicine gratis; and who can shew where his

* Observations on the Management of Mad-houses. Part the Second. By Caleb Crowther, M.D. 1841.

demands began to be unreasonable? On the whole, however, we like adherence to the most rigorous scrupulosity, and cannot, therefore, but approve of Dr. Crowther, who, when physician to the institution, "declined accepting from the matron a present of fruit, produced in the garden belonging to the Asylum, because I conceived that she had no right to give, nor I to receive, public property."

Perhaps the neatest way of getting out of this scrape would be to allow the Director the disposal of the vegetables not consumed in the Asylum, as a sort of increase to his salary. Another accusation which Dr. Crowther brings against the Wakefield Asylum, is founded on a remarkable case; so remarkable, indeed, that the desire of communicating it to the public may possibly have been the chief motive for writing this second essay.

Susannah Roginson, having become deranged from disappointment in love, was admitted into the asylum on the 19th of January, 1828, at the age of twenty-three. On the 1st of April, 1836, a dead child was found in her bed, and on the 18th of the following month she died herself. Susannah Roginson had long been in a state of fatuity, and never left the ward. In the day-time she was confined in a chair by straps, and at night in her bed. She was insensible to the calls of nature, and, at the time of the discovery of the child, was greatly emaciated, and of disgusting personal appearance. Was this poor creature the mother of the child? Dr. Crowther is almost sure she was; we think it at least as likely that she was not, and that some other woman wished to conceal her frailty by laying its evidence in Susannah's bed. The visiting physician, says Dr. Crowther, was not made acquainted with the incident, while the nurses and servants were

pledged to secrecy; but the fact was communicated to some of the visiting justices. Instead of this, says Dr. Crowther, the director ought to have sent for one of the surgeons to the Asylum, who should have examined Susannah Roginson. So far, so good; but when he adds that if she had turned out not to be the mother, the matron and housekeeper ought to have examined all the nurses and female servants capable of bearing children, we must differ from him. Such an examination of a host of women on mere speculation, seems calculated to do any thing but effect Dr. Crowther's object of curbing indecency, and preventing the Asylum from becoming a scene of licentiousness. This examination can only be permissible when there are the strongest grounds for suspicion against the individual. This affair came to Dr. Crowther's ears about three years and a half after it had happened, on which he sent an account of it to the Secretary of State, and in due time received an answer stating that Lord Normanby did not "think it necessary to take any further proceedings on the subject."

After the receipt of this letter the visiting justices held an inquiry, which also terminated in nothing. The director stated to them, truly enough, "that such mischances could not at all times be prevented in an institution of this kind, by the utmost possible vigilance;" and the justices were probably influenced by the length of time which had elapsed since the incident.

The diet of the patients was changed for the worse, says the censor of Wakefield, by the substitution of boiled rice and onions for the regular Saturday's dinner of four ounces of coarse beef made into Irish stew. The excuse assigned was, that old potatoes cannot be procured at certain seasons of the year, and that the dish is not good

when made with new ones. Dr. Crowther does not think this a good reason, or the true one; however, the old Saturday's dinner has been restored.

As for the credibility of the servants of a madhouse, Dr. Crowther believes it easy to ascertain their moral character, and, consequently, the value of their evidence, through the medium of daily visitors. These last he thinks, and probably with truth, are a great check on the maladministration of hospitals and asylums. It is the business of a daily visitor to prevent fraud, waste, or neglect; to insure cleanliness; to examine all the provisions in the house, cooked or uncooked, and see that the patients get a proper quantity of good and wholesome food; and to notice the misconduct of officers or servants. If all the patients of a madhouse are sent to bed at six o'clock, owing to the absence of keeper, director, and matron, the unexpected visit of such an inspector will detect the negligence. Nay, his soothing converse contributes to the cure of the insane; for the patient often fancies that the nurse, director, and physician, are in a combination against him, and is tranquillized by the assurances of one who has no interest but in promoting his welfare.

Last August Dr. Crowther visited four of our great lunatic hospitals—Haslar, St. Luke's, Bethlem, and Hanwell. He was pleased with them all, but is enchanted with Hanwell.

In the improvement of lunatic asylums, Dr. Crowther is a useful co-operator; for though he is far too angry to be always in the right, he is far too clear-headed to be always in the wrong.

DEATH FROM AN EXPLOSION OF CARBONIC ACID.

THE daily journals have already made the public aware of the deplorable

event at the School of Pharmacy in Paris, which cost M. Hervy his life. He was preparing some carbonic acid in its liquid form, in Thilorsen's apparatus, which is composed of two cylindrical reservoirs, of about the thickness of two centimetres, with an interior diameter of from eight to ten centimetres. Each of those cylinders had an interior space of about four litres. The carbonic acid was produced by the action of sulphuric acid on the bicarbonate of soda, which were introduced simultaneously into one of the cylinders. A communication was then established between this first cylinder and the other, and the carbonic acid passed into the second cylinder by distillation. In the first cylinder remained a residue, consisting of sulphate of soda, which was taken out to replace it by a new supply, in order to obtain a determinate quantity of the liquid carbonic acid: a quantity which, as we have seen, may be raised to four litres. The first cylinder is supported on two trunnions, which keep it in equilibrium, and allow an extensive degree of oscillation.

It was at the moment when M. Hervy was balancing the cylinder on the trunnions, to effect the mixture of the acid with the soda, that the explosion took place. At this moment there was no one near except M. Hervy and a friend who was assisting him. The latter was thrown down by the explosion, but was not struck by any fragment of the machinery. Not so M. Hervy. The apparatus was on the floor of the room, and the whole effect of the explosion was directed against his legs, which were cruelly mutilated, so as from the commencement to hold out little chance of saving his life. Amputation of one of the limbs was performed next day; and in three days more this unfortunate victim to science had ceased to exist.

The explosion was accompanied by a frightful noise, and considerable injury to the apartment. It is almost unnecessary to add, that this experiment will not again be attempted in connection with the public lectures*.

* Journal de Pharmacie.

SELECT SURGICAL CASES.*

Reported by GORDON BUCK, M.D.,

Surgeon of the New York Hospital.

Convulsions following an old injury of the head; cured by the application of the trephine†.

AUGUST 22d, 1839. In company with the above named gentlemen, I visited Mrs. M'D., aged 35 years, a person in humble circumstances, who was then recovering from a severe attack of convulsions, for which it had been necessary to employ the most active depleting means. She was pale and feeble, but able to sit up, and had been confined for several weeks. Upon careful inquiry, the following particulars of her case were ascertained. Three years previous, while living in the city of New York, she received a blow in the middle of the forehead from a stone, which stunned her for a short time. On recovering her senses, vomiting came on, and continued for several hours. Two or three days after the injury, Dr. Mott saw her, and removed two small portions of bone. The wound continued to suppurate for several months afterwards, and then healed up. About eighteen months after the accident, she was attacked with convulsions, when far advanced in pregnancy with her third child. Bloodletting, with other depleting remedies, relieved her, so that she recovered her usual health, and went her full time. After a respite of several months she was attacked a second time, and relieved by the same means. She was now recovering from the third attack, which had been more violent and protracted than either of the preceding: during the interval that had elapsed between it and the second attack, she had several times suffered from threatening symptoms, such as flushed face, drowsiness, and torpor, of which she had been relieved by bloodletting. She never suffered from headache, nor referred any uneasy sensations to the seat of the wound. During the attacks, her face and all her limbs were alike convulsed. She was evidently agitated by our visit, and her recollection confused; with the assistance of her husband, however, she was able to make out the order of different occurrences. The cicatrix upon her forehead presented the following appearances: it was an inch and a half in length, in the form of a narrow furrow, with its upper half lying in the median line, and its lower a

little to the left of it, and terminating at the eyebrow; it was one-third of an inch in width, one-fourth in depth at its upper half, growing more shallow towards its lower extremity; there was evidently a loss of a portion of the external table of the skull, and the cicatrix of the skin was closely adherent to the bottom of the furrow. The parts were free from tenderness or pain. The patient, as well as her husband, was exceedingly anxious for relief, and willing any operation should be performed that afforded a prospect of benefit.

Our opinion concurring in favour of trephining, Dr. Fuller proceeded to perform the operation. A semi-lunar incision was made across the forehead, with its convexity directed downwards, intersecting the cicatrix near its inferior extremity, and extending an inch on either side of it. From the middle of this, a second incision, two inches in length, ascended along the median line: the angles were dissected up, and the periosteum detached from the bone. The first application of the trephine included about half an inch of the upper extremity of the furrow, and the disc removed appeared to be of unusual thickness, owing to increased deposit of osseous matter upon its inner surface. A second disc was removed, joining the first at its lower edge, and including the remainder of the furrow; the thickness of this disc exceeded that of the first, and was nearly half an inch. It was perforated by a foramen that transmitted a vein from the scalp to the sinus; the surface of the dura mater exposed was of a normal appearance. A moderate oozing of blood took place during the operation; but no ligatures were necessary. A single suture secured the angles of the wound in their proper situations, and the edges were kept in contact by adhesive straps. Compresses of lint, secured by long adhesive straps, completed the dressing. The patient, after the operation, expressed satisfaction at the relief it had afforded; her head, she said, felt altogether different from what it had before. The wound healed kindly by the first intention, and in about a fortnight she resumed her accustomed occupations.

August 31st, 1840. This patient has continued to enjoy good health, without any return of convulsions; though once or twice she has had some threatening indications, which Dr. Welch has relieved by seasonable treatment.

Fracture of the second lumbar vertebra, without paralysis.

This case, as well as the following, is interesting, from the circumstance that there was no paralysis, although fracture of the vertebra existed.

Daniel Wilson, born in Wales, aged about 37 years, of very intemperate habits, was

* From the New York Journal of Medicine and Surgery for October 1840.

† This case occurred in the practice of Dr. A. Welch, of Wethersfield, Connecticut, through whose courtesy I was invited to see it, and with whose approbation, as well as that of Dr. Fuller, of Hartford, who performed the operation, it is now made public.

admitted into the hospital in a state of delirium tremens, which continued several days, until twenty-four hours before death. Nothing could be ascertained respecting the nature of the injury he had received, nor was its extent suspected until he recovered his reason sufficiently to make it known himself, when it was ascertained that several days before his admission he had jumped from a garret window forty feet in height, in a fit of delirium. His right foot and ankle bore marks of contusion, and the extremity of the fibula was broken off. Before he regained his reason, it was impossible to keep him in bed; he was constantly going about the premises. After this, however, he began to complain of great pain, and soreness in moving his body, and as he lay in bed his head was thrown far back. On examining his back, the spinous process of the second lumbar vertebra presented an angular projection; pressure upon it gave no pain, nor did paralysis exist.

On dissection, blood was found extravasated between the folds of the mesentery and into the muscular and cellular tissue, covering the lumbar vertebræ. The body of the second vertebra was broken into fragments, the first and third were uninjured; the membranous sheath, as well as the enclosed cord, were sound.

Fracture of the skull, and of the second lumbar vertebra; without paralysis.

Q. D. was admitted, December 20th, 1839, into Ward No. 4, of the Marine Department. He had been knocked down in the evening by a carriage running against him. Several contusions of the scalp existed, besides a deep wound of an inch in length, in the axilla, from which there was but little hæmorrhage. The following morning, having recovered from intoxication, he complained chiefly of his back, and referred his pain to the lumbar vertebræ. There was no appearance of external injury or displacement; pressure did not much increase the pain. He was able to move in any direction, and even walked about the ward till the day he died; his face was flushed, and at times he was delirious. He was exceedingly averse to allow any thing to be done for him. At eleven o'clock, the third night after the injury, he was seized with a fit, and five hours after died.

Dissection.—There was a crack in the skull, running from the vertex on the left side to the foramen magnum. The whole surface of the right hemisphere of the cerebrum was covered with a layer of coagulated blood, thickest over the inferior surface of the middle lobe, where the convolutions were softened into a pultaceous mass. The veins of the pia mater upon both hemispheres were loaded with blood. The second lumbar

vertebra was fractured across its body. The anterior vertebral ligaments remained entire, excepting on the left side, where a small spicula of bone had obtruded. The cellular and muscular tissues around were infiltrated with extravasated blood.

APOTHECARIES' HALL, IRELAND.

MEDICAL REFORM.

To the Editor of the Medical Gazette.

SIR,

YOUR giving insertion to the enclosed in your valuable journal will oblige,

Your obedient servant,

GEORGE ATKINSON,

Secretary to the Court of Examiners of the Apothecaries' Hall, Dublin.

April 8, 1841.

The Governors and Company of the Apothecaries' Hall of Ireland, coinciding in the opinion generally entertained that a change is required in the constitution of the medical profession in these kingdoms, have taken the subject into their consideration, in the hope of contributing to the removal of some of the difficulties with which it is surrounded, and of laying the foundation of an effectual and salutary reform.

The Governor and Company have been, for a long time, desirous of a general amendment of the laws which regulate medical affairs; but, in consequence of the divisions which have hitherto prevailed among the different presiding bodies, they despaired of effecting any arrangement which would have been commensurate with the wants of the profession as a whole, and had to restrict their attention to such changes as appeared to be called for in their own department. Their views on this subject are embodied in the draft of a bill which they are prepared to submit to the wisdom of the legislature, and, with regard to the nature of which, all that is necessary here to be stated is, that it is in conformity with the principles which they now wish to promulgate as the basis of a more general measure, the period having at length arrived when, through the concurrence of the different medical corporations, a full and comprehensive reform may be expected.

The Governor and Company, in delivering their opinions on so important a subject, wish to premise, that while they give a ready assent to the faulty constitution of the existing corporations, they entirely disbelieve they have operated injuriously upon the public, or that they are incapable of adaptation to the wants of the profession; they must express their regret, therefore, that any bill should have been introduced

into parliament having for its object the supersedence or annihilation of these institutions, which, with all their alleged imperfections, have sedulously administered to the wants of the people, and provided well-educated practitioners in every department of the healing art; and it cannot be too much urged that the grievances complained of have reference principally to the well-being of the profession itself, and that the public are only interested in the changes sought, so far as they are calculated to engender a better spirit of co-operation and harmony in the medical community.

The Governor and Company, however, representing a numerical majority of the practitioners in Ireland, are not the less sensible of the magnitude of the interests at stake, or less disposed to aid in a consummation so greatly to be desired, but while professing to have the same objects in view, they must dissent from a large portion of their brethren who would seek for them in the establishment of a "one faculty of medicine," in place of the tripartite representation which has so long existed; convinced by long experience that excellence in the respective departments can only be attained by sectional separation, and that the appointment of different institutions to preside over the branches of Physic," "Surgery," and "Pharmacy," is founded in wisdom, and advantageous to the public.

The Governor and Company consider that the real grievances under which the profession labours, arise, in the first place, from the irresponsible constitution and unequal privileges of the corporate bodies; and in the second, from the want of uniformity of education and reciprocity of rights among the members of the respective departments in the three kingdoms.

The Governor and Company accordingly propose the following legislative changes:—

1.—A full extension of corporate rights and advantages to the licentiates belonging to each branch of the profession.

2.—The establishment of uniform curricula of study for England, Ireland, and Scotland.

3.—The institution of a "General Board of Superintendence and Control" in each kingdom—to be composed of an equal number of representatives from each branch of the profession, and to be elected annually by voting papers (as proposed by Mr. Warburton,) whose duty it should be to regulate education—to appoint Censors to attend on the different Boards of Examination—to grant licences for practice to (but not examine) those presenting diplomas from the different Corporations—to recognize teachers and schools—to keep and publish registries of the qualified, and to act in general as a Court of Appeal and Board of Health.

4.—"Diplomas in Medicine" to be granted only by Universities and Colleges of Physicians, "Diplomas in Surgery" by Colleges of Surgeons, and "Diplomas in Pharmacy" by the Apothecaries' Halls.

5.—A licence for "General Practice" to be granted in *future* only to candidates presenting the joint diplomas of the College of Surgeons and Apothecaries' Hall.

6.—A right to recover professional charges to be conferred on those holding the licence of the Board, and not upon others.

7.—Individuals holding the licence of the Board to be alone eligible to fill situations in the Army and Navy, and public institutions.

8.—The establishment of an Apothecaries' Hall in Edinburgh.

9.—The rights and privileges of existing practitioners to be saved in the most full and ample manner.

By order,

GEORGE ATKINSON, *Sec.*

Dublin, Mary Street, March 5, 1841.

ANCIENT USE OF FRENCH AT CAMBRIDGE.

MANY of the public forms of address used by the Bedells, as will be seen in subsequent parts of these extracts, were in the French language; some of them were probably derived, along with nearly all our other forms of proceeding, from the University of Paris, the most ancient and the most celebrated of the universities of Christendom. The use of the French language, which was that of the Court, was permitted, in common with Latin, in the statutes of some of our earlier colleges, such as Clare Hall, founded in 1326, and also in those of King's Hall, founded about the same period, as given by Richard II. Mr. Hallam has referred (*Introduction to the Literature of Europe*, p. 63), to a similar regulation in the statutes of Ariel College, Oxford, founded in 1328. No such permission was given in the more ancient statutes of Merton College, or in those of Peterhouse, which were almost entirely copied from them.—*Observations on the Statutes of the University of Cambridge.*

[In another part of the work Dr. Peacock corrects the statement as far as regards Peterhouse, where in the ancient statutes the scholars are requested to use "*Latino eloquio nisi forsitan aliquotiens et ex justâ causâ et rationabili Gallico, sed perraro Anglico fruuntur*," &c.]

It is hardly necessary to observe that in early times, when Latin was the dialect of the learned, and French the language of the court, English must have been considered a rustic jargon, just good enough for a fair or market, like the Flemish or *Platt Deutsch* of the present day.]

NOTE ON PTOSIS.

To the Editor of the Medical Gazette.

SIR,

MR. HUNT'S observations on my case of ptosis, which appeared in the last number of your journal, scarcely require any reply, the accuracy of my remarks not being called in question ; for it does not appear the operation has ever been *practised* before in any case of paralytic ptosis. My object was to show, by experience, that the plan is equally applicable to the paralytic form of the affection as to that consequent on injury of the levator palpebræ muscle. I had not preserved any note of the journal in which Mr. Hunt's case was recorded, or I might have alluded more particularly to it, and noticed his recommendation of the operation in cases of paralytic ptosis. The objection to the operation, on the score of the paralysis of the third nerve affecting the iris, suggested by Mr. Hunt, did not apply to the case in which I operated, as the pupil acted, though sluggishly, and was not preternaturally dilated.—I am, sir,

Your obedient servant,
T. B. CURLING.

1, Mount Place, London Hospital,
April 13th, 1841.

UNIVERSITY OF LONDON,
SOMERSET HOUSE.

THE following gentlemen have been appointed Examiners for Medical Degrees for the ensuing year, commencing July 1st.

In Anatomy and Physiology.—F. Kiernan, Esq. F.R.S. and Professor Sharpey, M.D. F.R.S.

In Chemistry.—Professor Daniell, F.R.S.

In Botany.—Rev. Professor Henslow.

In Materia Medica and Pharmacy.—Dr. Pereira, F.R.S.

In Medicine. — Dr. Billing and Dr. Tweedie, F.R.S.

In Surgery.—Mr. Bacot and Sir Stephen Hammick.

In Physiology and Comparative Anatomy.—Dr. Roget, F.R.S.

In Midwifery.—Professor Ferguson, M.D.

In Forensic Medicine.—Professor Daniell, Dr. Ferguson, and Dr. Pereira.

SOCIETY FOR RELIEF OF WIDOWS
AND ORPHANS OF MEDICAL MEN
IN LONDON AND ITS VICINITY.

WE beg to direct the attention of our readers to the advertisement announcing that the Annual Dinner of this Society will take place to-morrow (Saturday), when H.R.H. the Duke of Cambridge has graciously consented to take the chair.

RECEIVED FOR REVIEW.

A Complete Practical Treatise on Venereal Diseases, and their immediate and remote consequences ; including certain affections of the uterus attended with discharges. By William Acton, late Externe at the Female Venereal Hospital, Paris.

Dr. William Davidson on the Sources and Mode of Propagation of the Continued Fevers of Great Britain and Ireland.

Mr. Tuson on Curvature of the Spine.

Dr. Wightman on the Sympathetic Relation between the Stomach and Brain.

APOTHECARIES HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

April 1, 1841.

F. Weatherley.—V. W. Blake, Kennington.—A. Clake, Gloucester.—H. Young — J. Berncastle, Lewes, Sussex.—S. Williams, Kennington.—C. S. Bompas, Bristol.

April 15, 1841.

F. Wright, Derby.—J. J. Clapcott, Dorchester.—T. O'Conner, Elphin, Ireland.—T. Ansell, Hampstead.—J. R. Phillips, Birmingham

A TABLE OF MORTALITY FOR THE
METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 3d April, 1841.

Small Pox	17
Measles	8
Scarlatina	12
Hooping Cough	54
Croup	10
Thrush	2
Diarrhoea	5
Dysentery	1
Cholera	0
Influenza	21
Typhus	20
Erysipelas	6
Syphilis	0
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	137
Diseases of the Lungs, and other Organs of Respiration	277
Diseases of the Heart and Blood-vessels	18
Diseases of the Stomach, Liver, and other Organs of Digestion	56
Diseases of the Kidneys, &c.....	4
Childbed	6
Ovarian Dropsy	0
Diseases of Uterus, &c.	3
Rheumatism	1
Diseases of Joints, &c.	6
Ulcer	0
Fistula	1
Diseases of Skin, &c	0
Diseases of Uncertain Seat	118
Old Age or Natural Decay	68
Deaths by Violence, Privation, or Intemperance	28
Causes not specified	5
Deaths from all Causes	884

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, APRIL 23, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXXI.

Spinal hæmorrhage. Paraplegia. Facial palsy and facial anæsthesia; their symptoms, prognosis, and treatment. Other forms of local paralysis, and local anæsthesia.

I HAVE done with apoplexy as it respects the brain; which is the same thing as to say that I have done with apoplexy. You will find the same term applied, indeed, to effusions of blood in other organs of the body: but this use of the word is a perversion of language. Apoplexy, as I have frequently observed before, is the abolition of the functions proper to the *brain*; of sensation, voluntary motion, and thought. In short, it is coma, coming on under certain circumstances.

Spinal hæmorrhage.—I shall not speak therefore of *spinal apoplexy* (though that would be less improper than *pulmonary apoplexy*, or *hepatic apoplexy*), but of *spinal hæmorrhage*. Of this I really have little to say, except that it is well known occasionally to occur; and that the symptoms to which it gives rise are by no means peculiar or distinctive. They consist in *pain* in some part of the spine; *convulsions*; *palsy*: that is, they are the very same symptoms which inflammation, softening, mechanical violence, and other disorders of the same part, may give rise to. Spinal hæmorrhage is much more rare than cerebral hæmorrhage. Dr. Abercrombie had met with only one case of it. He gives the heads of seven

others which have been recorded by different authors. Dr. Bright has never seen it: but he publishes the particulars of one case, which was communicated to him by Dr. Stroud.

I will read you one or two short examples of spinal hæmorrhage, as specimens. A girl, fourteen years old, was attacked with headache, *pain in the back*, and a tendency to sickness when in the erect posture. At the end of a week the pain in the back became suddenly and very greatly aggravated; and this was followed by general convulsions, which proved fatal in five or six hours. The spinal canal was found filled with extravasated blood, in the lumbar region, where she had felt the pain. The brain and all the other viscera were sound. The case is detailed by Mr. Chevalier in the third volume of the *Medico-Chirurgical Transactions*.

Take one more instance from Ollivier, whose work on the spinal marrow you may read hereafter, when you have leisure, with advantage.

A gentleman, aged sixty-one, had just arrived in Paris after a long journey, when he was seized with *pain of the back*, all the way down from the cervical vertebræ to the sacrum. In the course of a few hours he became paraplegic, and was unable to retain his urine or fæces. He then sent for a physician, and died while talking to him. A very extensive extravasation of blood was found in the spinal canal, beneath the membranes of the cord. The lower part of the canal was filled with a bloody mass, in which the substance of the cord could not be distinguished. Above the third dorsal vertebra the cord was entire, but of a deep-red colour, and very soft.

The *suddenness* of the symptoms may lead you to suspect the true nature of these cases; but I cannot pretend to point out any other feature by which they may be distinguished from other morbid conditions of the spine, already spoken of. I show you one

preparation; of which, however, I do not know the history.

I have nothing to add, to what I have already said, respecting that species of palsy which is called *hemiplegia*: and I have only a very few farther observations to make in regard to *paraplegia*.

Paraplegia.—The cause of this kind of palsy is sometimes obvious; sometimes most obscure. If we find, in the spinal canal, blood effused, softening of the substance of the cord, traces of inflammation of its investing membranes, tumors pressing upon it, pressure from disease or displacement of the bones, we have a sufficient explanation of the paralysis of those parts of the body, the nerves of which come from the spinal marrow at or below the place of the disease. There are three preparations on the table of tumors that pressed upon the cord; scrofulous tumors I believe they are: each of the three persons from whose bodies they were respectively taken was more or less completely paraplegic.

But in very many cases we detect no alteration that seems adequate to explain the paraplegia. The palsy creeps on slowly and insidiously, without any particular pain, or violent symptoms: there is no tenderness or bending of the vertebræ. The weakness commences mostly in the legs, which appear to the patient heavier than usual, and of which the healthy sensations are often perverted. The toes tingle, or are numb: he experiences a feeling in them as if a number of ants were crawling on the skin. This is so common a circumstance as to have given a name to the symptom, *formication*. The patient straddles as he walks. His legs are lifted awkwardly, the toes being often the last part to quit the ground: they are then flung obliquely forwards and outwards, and the feet flap down heavily and uncertainly at every step. By degrees the weakness of the lower limbs increases: the palsy creeps upwards, affects the bladder and rectum, at length invades the arms, and ultimately the patient dies: yet very faint traces of disease, or no traces at all, may be visible upon inspecting the brain and spinal cord.

Very recently I had a patient with these symptoms in the Middlesex Hospital. He went out contrary to my wishes; he was discharged, in fact, by mistake: he took refuge in St. Bartholomew's, where he died after a few days. I have been informed that serum was found beneath the arachnoid, and that the whole brain had a wasted appearance; but nothing satisfactory was made out. The palsy had by slow degrees ascended from his legs to his arms; he had had no head symptoms beyond some occasional confusion and vague uneasy feelings there.

At this present time a patient in a similar condition comes to the hospital every Thursday, dragged by a friend in a hand-chair. He looks very well in the face, but is palsied below.

It is in these cases that you may expect to witness the very remarkable phenomena which I mentioned before as evincing the separate existence of a "true spinal marrow," distinct from the brain and its prolongations into the spinal canal, endowed with special and peculiar properties, and performing functions that are independant of sensation, consciousness, or the will. If you pinch or tickle the surface of the paralytic limbs, particularly about the feet, they will, in many cases, start up and move strongly, not only without any voluntary effort on the part of the patient, but in spite of him, and even (in those instances in which there is anæsthesia as well as palsy) without his knowing it. The legs often spring up *of their own accord*, as it seems; but, no doubt, the apparently spontaneous movement is frequently an excited movement, and takes place in obedience to the law that governs the automatic motions of the body. Some impression, made first upon the peripheral extremities of afferent nerves, runs through the nervous arc of communication, and exhibits its ultimate effect at the extremities of the corresponding efferent motor nerves. We can imagine many such accidental and unsuspected sources of excitement; a casual touch, the varying contact of the bed-clothes, the bite of a flea for aught I know to the contrary, may suffice. Even the passage of fæces or flatus along the lower bowel, or of the urine through the urinary passages, may be enough (as we are taught by unquestionable facts) to produce these movements. They are more readily excited, *cæteris paribus*, in proportion as the interfering influence of the will is more completely cut off.

I knew a gentleman, who had retired from the medical profession, and who, though not paraplegic, laboured, I believe, under some morbid condition of the spine. He had been, in early life, a hard drinker, and had suffered delirium tremens. Every night, sometimes more than once or twice, the trunk of his body, and all his limbs, became for a while fixed and stiff, from rigidity of the muscles. He told me this curious fact. Whenever he scraped his shoes on the scraper at the door, his leg flew up, with a spasmodic suddenness, from the iron, notwithstanding his endeavour to prevent it. I believe he was found dead in his bed.

In some cases of paraplegia involuntary retractions of the palsied limbs can be excited; in some cases they cannot. When the influence of the cerebrum is quite ex-

cluded by the operation of disease affecting the spinal cord itself, then is the susceptibility of excited movements the most lively. But the increased susceptibility, which has this inverse relation to the voluntary power, is limited to that portion of the body, the nervous arcs belonging to which lie beyond the seat of the disease; more distant, I mean, from the brain. Hence it follows that we may determine, approximately, the place of the disease, by the test of these reflex actions. The mischief may be situated, or may extend, so low down, that there are no uninterrupted nervous arcs below it. Supposing it to lie as low as, or to reach, the commencement of the cauda equina, we should have no involuntary movements. Conversely, when no involuntary movements can be excited, the spinal disease is, at least, as low as the upper lumbar vertebræ. Thus, I say, we have another mode, in addition to those pointed out in a former lecture, of determining, in a given case of spinal palsy, whereabouts, or to what extent, the cord is implicated in the disease.

We do not so often observe these reflex movements in cases of hemiplegia; apparently for this reason, that in hemiplegia the sensorial influence is not, usually, so completely shut out as it is apt to be in paraplegia. Yet I have seen some of these phenomena in hemiplegic patients. One of them, whose right hand and arm were quite passive under the strongest efforts of his will to stir them, took notice himself, as did his nurse, that whenever he yawned and stretched himself the fingers of the palsied hand participated in the action, and were thoroughly extended: and I could, by tickling the sole of his foot, excite some starting of the leg, long before any power of voluntary movement returned.

Do not forget the important fact that, in these cases of paraplegia, the urine generally becomes ropy, alkaline, and stinking; and that the bladder, after death, presents appearances such as chronic inflammation might produce; roughness and redness of its inner surface, and thickening of its coats. What is the order and relation of these changes, I confess I do not know: whether the quality of the urine is first altered, and then the bladder suffers from the perpetual contact of this unnatural secretion; or whether the bladder becomes diseased in consequence of the palsy, and pours forth unhealthy mucus, whereby the quality of the urine is affected; has not, I believe, been clearly ascertained. The first of these two suppositions seems to be the most probable. I fancy, indeed, (but I am not sure) that the urine remaining in the pelvis of the kidney in some of these cases has been found to be alkaline. Perhaps the disorganization of the bladder, and the alkaline state of the

urine, may both be common results of the interruption of the nervous influence.

We have reason to believe that the defect in some of these cases of paraplegia is merely functional: independant, I mean, of any such change in the nervous matter as is cognizable by our senses. It may be brought on by various causes: by cold; by intemperance in drinking; by excessive sexual intercourse; or, still more surely, by self-abuse. I have had the latter cause assigned to me voluntarily by patients themselves. In such cases we may presume that the loss of function is confined to the spinal marrow. But there is another way in which paraplegia may be accounted for, although its physical cause is very liable to be overlooked. It may result from serous effusion into the spinal canal; which effusion may have originated *there*; or, what seems sometimes to be more probable, may have been poured out *within the cranium*, and descended by the force of gravity to the lower part of the cavity of the spine. Dr. Baillie read a paper on this subject before the College of Physicians: it is contained in the sixth volume of the *Medical Transactions*. He was not the first person to whom this mode of explaining certain obscure cases of paraplegia suggested itself; but he was the first I believe who published upon it. This effusion may very readily be overlooked. Commonly the brain is examined first; and no great attention is paid to the escape of fluid from the vertebral canal. It would be better to lay open the spinal cavity first, at its lowest part, and to puncture the theca, and then to observe what quantity of fluid runs out when the body is placed upright. There *should* be a *certain* quantity: but if *much* serum so escaped, we might conclude that it had existed in hurtful abundance during life, and had caused the paraplegia. In most of these obscure cases you may trace *some* head symptoms; giddiness, transient confusion of thought, loss of memory; and it really will be worth your while to make the examination in the way I have pointed out, whenever you have occasion to inspect the body of a patient who has died paraplegic.

I regret very much that I had not the opportunity of doing this in the case of the man whom I had been for some time watching in the hospital.

Cases, such as I have now been describing, are by no means uncommon. They are usually slow and tedious; and you will be called upon to administer to their relief. I need not repeat the caution which I have several times given, in respect to the condition of the bladder; you must take care that it does not become over distended with urine; and you must enjoin strict attention on the part of the nurse to keeping the patient clean and dry. Friction along the course of

the spine; blisters to the loins or sacrum, frequently repeated; issues; and electricity: all these means you will generally have opportunity enough for trying, and for regretting their inutility. In such cases it is warrantable and right to give strychnia; a poison which mainly affects the spinal cord; causing, when given in an over dose, tetanic spasms of the limbs, with very little affection of the sensorium. I have heard of some striking instances of recovery from paraplegia under the use of this drug. I wish I could tell you that I had even *seen* such. I should not advise you to begin with a stronger dose of strychnia, or of the sulphate of strychnia, than the twelfth part of a grain every six hours: this may be gradually and cautiously increased, until it gives rise to twitchings of the limbs, or to some other obvious effect. The twitching is usually confined to the palsied limbs. This shews that it results from the agency of the remedy upon the excito-motory system, or true spinal marrow; of which the reflex function is always more readily exercised when the sensorium has lost its customary controlling power. When this symptom occurs, you had better go on with the same dose; it would be unsafe to increase it: and the progress of the case will soon inform you whether any benefit is likely to accrue from the continuance of the medicine. The out-patient now attending has taken the strychnia. It made the palsied limbs start and extend themselves; but no permanent power has been gained.

There is one other drug which I should recommend you to try in such cases; viz. the tincture of cantharides. It certainly has sometimes a very beneficial effect. Generally, when it does good, it acts as a diuretic; and Dr. Seymour has thrown out a suggestion that it is most likely to be useful in those cases of serous effusion into the spinal canal, of *spinal dropsy*, which I just now described. He recommends the tincture as a good diuretic in several forms of dropsy; and supposes that it benefits paraplegia by tending to produce absorption of the serum effused within the vertebral canal.

Moreover, there is another principle upon which this medicine may be sometimes advisable. Cantharides are well known to have a peculiar effect upon the bladder; which effect is doubtless produced by the corresponding part of the spinal cord. If, by means of the Spanish fly, we can excite, though but from time to time, the function of that part, we may obviate, in a great degree, the distressing consequences of incontinence of urine, arising from paralysis of the *sphincter vesicæ*. Dr. Marshall Hall relates a very interesting fact, bearing directly upon this point. A young lady had a tumor within the tenth and eleventh dorsal vertebræ. It gradually, but completely,

severed the spinal marrow, and induced perfect paraplegia. The bladder lost its power of retention. But on giving a dose of the tincture of cantharides the power of retaining the urine was always restored *for the time*. That power would cease, and again be restored, on suspending or repeating the medicine.

Dr. Hall remarks that the cantharides obviously acted through the segment of the excito-motory system left below the division of the spinal marrow.

The tincture may be given in half-drachm doses.

Facial palsy and facial anæsthesia.—The forms of paralysis that have hitherto been noticed are forms of *partial* paralysis. When the palsy is still more limited, although the epithet partial would be equally applicable, the term *local* palsy is more commonly used. There is one of these local palsies which is exceedingly interesting, and of much importance: I mean palsy as it affects exclusively one side of the visage; *facial* palsy. It is sometimes called, not very correctly, paralysis of the portio dura of the seventh nerve. The most common kind of facial palsy is indeed paralysis of the muscles supplied by that nerve. But the word paralysis is misused when it is intended to express any other loss of function than that of the faculty of motion in muscular parts. It is incorrect to speak, as some authors do, of palsy of the kidney; it is equally inexact to speak of palsy of a nerve.

I say that facial *palsy*, and facial *anæsthesia*, (for the two should be considered together) are very *interesting* affections, because they elucidate, in the human subject, some of the most curious speculations of modern physiology: and they are *important* affections for you to study and understand, inasmuch as, though always distressful and alarming to the patient and his friends, they sometimes are merely inconvenient and disfiguring, and sometimes bespeak a condition of extreme danger.

Let us first consider that affection in which the majority of the muscles on one side of the face alone are palsied. I have already briefly touched upon this form of palsy when it constitutes a part of hemiplegia. But it is of more consequence to attend to it when it occurs without any similar affection of the limbs. When the arm, or leg, or both, are paralyzed at the same time with one side of the face, we know that the whole results from cerebral disease; but it is not necessarily so when the face alone is palsied; and it is often of great moment to the comfort and the safety of the patient, that we should be able to tell whether the palsy does imply disease within the skull, or not.

The appearance presented by patients

affected with facial palsy is peculiar, and very striking. From one half of the countenance all power of expression is gone; the features are blank, still, and unmeaning. The other half retains its natural cast, except that, in some cases, the angle of the mouth on that side seems drawn a little awry. This is apt to be mistaken for proof of a spasmodic condition of that part; but it is owing simply, as I stated before, to the want of the usual balance or counterpoise from the corresponding muscular fibres of the palsied side. The patient cannot laugh, or weep, or frown, or express any feeling or emotion with one side of his face, while the features of the other may be in full play. One half of the aspect is that of a sleeping, or of a dead person; the other half is alive and merry. The incongruity would be ludicrously droll, were it not so frightful also, and distressing. To the vulgar, who do not comprehend the possible extent of the misfortune, the odd appearance of such a patient is always a matter of laughter and merriment. On the other hand, his friends and relations imagine that he has had a fit, and are in great alarm for his life. In the majority of these cases there is not, however, any real danger of that kind to be apprehended; a circumstance which, of itself, would render the exact diagnosis of the complaint peculiarly interesting. In general there is no deficiency of sensation. And we sometimes have loss of sensibility in the same parts, without any diminution of the power of motion. The best way, I believe, to place the phenomena of these curious affections plainly before you, will be by examples.

A house-maid, Jane Smith by name, twenty-eight years old, became one of my out-patients at the Middlesex Hospital, with the following symptoms. She had lost all power of moving the right side of her face. When she endeavoured to raise her eyebrows, the right side of the forehead remained smooth, and the left was wrinkled. When she attempted to close her eyes, the right eye was but partially covered, the eyeball rolling upwards, and carrying the cornea within the curtain of the upper lid, which descended a little to meet it. When she tried to snuff in air through the nose, not being able to keep the right nostril stiff and open, its sides came together, and no air passed up on that side. When she smiled, the right side of the face remained perfectly still, like a mask; and it wore at all times a vacant and inanimate character. When she was told to perform the action of blowing, her right cheek was puffed out like a loose bag, and the breath issued, whether she would or no, at the right angle of her mouth. The same thing happened with her food and drink; she could not prevent their escaping at the

right corner of her mouth: nor could she convey morsels of food from the right to the left jaw, without the aid of her hand applied externally in support of the paralyzed cheek. The masseter and temporal muscles, however, acted as strongly on the one side as on the other; she could chew perfectly well on the palsied side, and the sensation of the palsied parts remained perfect; and there was no paralysis of any other part of the body.

All these phenomena are invariably met with in all well-marked cases of this kind. I will contrast them with the phenomena presented by another of my patients, who was in the hospital, and whose name was Ann Church. I give their names, that I may the more readily distinguish the one from the other. When this woman, Church, applied for admission, she complained of intense pain, with some swelling, of the right temple, and extending thence generally over the right side of the face and head. It was soon discovered, however, that although she complained of most severe pain in these parts, they had entirely lost their ordinary sensibility to external impressions. She felt nothing when her forehead, or cheek, or nose, or chin, was touched on that side. In short, there was complete *anæsthesia* of the right half of the face; just as in Smith's case there was complete *palsy*. The insensibility was very exactly limited to the right half, and terminated abruptly at the mesial line. It was remarkably evident in a part in respect to which the bystanders could scarcely be deceived, even if there had been any reason (which there was not) for distrusting the patient's own statement. The surface of the eyeball is proverbially sensitive, even to slight impressions. But you might place your finger upon this woman's right eye, or you might brush it with a feather, without giving her the smallest pain, or producing any sensation at all: whereas, on the left side, the lightest touch caused involuntary shrinking, and closure of the eyelids, and a gush of tears. She declared also that she had no feeling in the right half of her mouth; she neither tasted sapid substances, nor was at all conscious, from any sensation produced by them, that they were placed there. Her lips, on the same side, were equally destitute of sensibility; so that when she drank, having no perception of the contact of the cup with her lips beyond their middle point, she felt as if she were drinking from a vessel with a broken rim. This is a circumstance which all persons who are thus affected are much struck with; and it almost always forms a part of their voluntary account of themselves.

Besides this defect of sensibility, the power of contracting the masseter and temporal

muscles on the right side was entirely abolished in this patient. You may deceive yourselves on this point, if you do not investigate it carefully, and with certain precautions. At least I have known persons doubt, because, having directed the patient to open and shut his mouth, they have confounded the movement of the whole jaw with the action of the masseter muscle. But if you tell the patient first to close his mouth, and then to perform the action of grinding with his teeth, placing your fingers at the same time on the corresponding muscles on each side, the difference, when it exists, will be very striking. In the woman of whom I speak, no swelling of the masseter or temporal muscle on the affected side took place when she forcibly closed her jaws. There was no other paralysis.

Now we cannot separate the physiology from the pathology of such affections as these. Nor ought we. The morbid conditions of which the two cases just described furnish samples, illustrate in a very beautiful manner the modern doctrine respecting the special uses of particular nerves. In the first of the two cases the palsy resulted from suppression of the function of the hard portion of the seventh pair of cerebral nerves; and the anæsthesia, in the last of the cases, depended upon suspension of the function of the fifth pair. You know that experiments performed upon living animals have proved that the division, by the scalpel, of the *portio dura*, before it spreads out into that remarkable nervous network on the side of the face, paralyzes all the muscles the combined play of which gives variety and significant expression to the countenance; and that, on the other hand, the division of the fifth nerve deprives the same parts of their sensibility. In these two cases, and in such as these, for they are by no means infrequent, a similar set of experiments upon the same nerves, in the *human* living body, is performed before our eyes by the agency of disease, or accident: and the result justifies most completely those conclusions which had been deduced in the first instance, from contrived observations made upon the lower animals.

There is one point in the history of these cases upon which I must dwell a moment longer; for it is a most interesting point. That the condition of the temporal and masseter muscles should be reversed in two patients so oppositely situated, was no more than might have been expected. But in each these muscles were affected in a manner the very contrary of that which the general circumstances of the case would, *à priori*, have prepared us to anticipate. Where the superficial muscles were paralyzed, and the principal movements of the face suspended, there the masseter and temporal muscles were in

full power and action; and where the loss of sensation was the predominant phenomenon, and the ordinary motion and expression of the countenance remained, there these muscles were in a state of complete palsy.

A few years only ago, this difference and apparent inconsistency would have been quite inexplicable. The progress of modern science has removed the difficulty, by establishing a general agreement between the *functions* of different nerves, and certain observed peculiarities in their *anatomical relations* and *arrangements*.

Suffer me to *remind* you (for I know that these interesting points of physiology must have already been taught you) that the nerves which proceed from the spinal column on each side are connected with it by two fasciculi of nervous fibrils—two *roots*, as they are metaphorically called—of unequal size; that when the larger of these, which is situated posteriorly, and is furnished with a ganglion, is divided in a living animal, the parts to which the nerve is distributed lose the faculty of sensation, while the power of voluntary motion remains unimpaired; and that when the smaller and anterior, which has no ganglion, is alone cut, the same parts are instantly palsied, but retain their sensibility. In other words, the posterior fasciculi minister to the faculty of sensation, the anterior to that of motion.

Now the fifth pair of nerves was observed to have a similar origin; to be composed, that is to say, of two fasciculi or roots, one larger than the other, and invested with a ganglion; the other smaller, and having no ganglion. It was natural to infer that the functions of these roots would be analogous to those of the corresponding portions of the spinal nerves; that the ganglionic fasciculus would relate to sensation, and the other to motion. And such is found to be the case; and the arrangement here is really very curious. The smaller portion of the fifth nerve is exclusively expended upon a very few muscles; viz. the masseter, the temporal, the two pterygoid muscles, the circumflexus palati, and the tensor tympani. The action of the two first of these, of the masseter and temporal muscles, is obvious to common observation; and therefore their condition is noticed in such cases as I have related. Again, these very same muscles have been shown, by careful dissection, to receive no nervous branches from the seventh nerve, which is a motor nerve, and which ramifies so abundantly upon the superficial muscles of the face.

It was to be expected, therefore, that any diseased state confined to the *portio dura* of the seventh nerve, would leave the temporal and masseter muscles fully effective: and that disease involving the fifth nerve, but leaving the seventh untouched, would destroy,

not only the general sensibility of the face on that side, but also the power of contracting these particular muscles. And this was thoroughly exemplified in the two cases that I have detailed. The girl Smith had total palsy of the superficial muscles; but sensation, and the action of the deeper-seated muscles, continued perfect: while in the woman Church there was default of sensibility, and paralysis of the temporal and masseter muscles; but the movements of the superficial muscles were unimpeded.

Every interruption of the function of the portio dura will paralyse these superficial muscles of the face: and such interruption may be occasioned either by *sudden injury* done to the trunk of the nerve; or by *disease* affecting its proper structure; or by *pressure*, the consequence of disease in the parts contiguous to it. And it is of great importance to observe that the morbid condition which causes the interruption may be situated in any part of the course of the trunk of the nerve: while it is yet within the cranium; or during its passage through the petrous portion of the temporal bone; or after it emerges upon the face, through the stylo-mastoid foramen, to be ultimately spread in meshes over the cheek and temple. The nerve is often compressed or hurt while still within the skull; but in *most* cases of this kind other portions also of the nervous matter are involved in the mischief, and other sets of voluntary muscles testify this by their immobility or their irregular action. This is sometimes the case when facial palsy occurs as a part of hemiplegia. In many instances, however, of hemiplegia, there is but slight distortion of the countenance, a mere hanging of the cheek, with no paralysis of the orbicularis muscle of the eye. In these cases, it has been suggested to me by my colleague, Dr. Todd, that the seventh nerve is probably not affected at all, but the motor branch of the fifth nerve only. When the *facial muscles alone* are paralysed, it happens in a great majority of instances that the nervous function is interrupted in that part of the portio dura which lies incased in the bone, or in the more exposed part which issues in front of the ear: and hence it arises that this particular form of palsy is, in general, unattended with any danger to life.

The physical cause of this remarkable disfigurement, and the true explanation of its prevailing immunity from danger, were first pointed out by Sir Charles Bell: but both the existence of the malady as a distinct form of disease, and its comparative harmlessness of character, had been observed and described some years previously: although the reason of neither the one nor of the other was at that time understood. Dr. Powell had narrated, in the fifth volume of the *Transactions of the*

College of Physicians, three marked instances of this form of local palsy; and had noticed at the same time its apparent independance of any apoplectic tendency, or cerebral disease.

The exciting causes of the complaint are various. Sometimes it is the consequence of mechanical violence, by which it is plain that the nerve has been lacerated, or otherwise injured. Sir Charles Bell, to whom we are indebted for much information on this subject, mentions several examples of this kind. In one a man was shot by a pistol ball, which entered the ear and tore the portio dura across at its root. In another, the patient was gored by an ox; the horn of the animal entered beneath the angle of the jaw, and came out in front of the ear, tearing the nerve across. In a third, the nerve was divided by the surgeon's scalpel, in an operation for the removal of a tumor which lay above and around its course. In all these cases the injury was external and obvious. In a fourth the palsy followed a blow on the ear which caused hæmorrhage from that part: here probably the nerve was hurt in its passage through the bone. Some time ago, a man was brought into the Middlesex Hospital who had fallen from a height, upon his head. The muscles of the left side of the face were paralysed. He died in a few days; and examination of the head shewed a fracture in the base of the skull, passing through the petrous portion of the temporal bone, and rending the seventh nerve at its entrance into the meatus auditorius internus. In the year 1832 I had a patient (Richard Hills) in the hospital with the same kind of paralysis, which seemed, in him, to have been occasioned by a mere shock, or jar. He was a coachman, and one day, when he was off his box, his horses started away, and ran to their heads to stop them, but he was thrown down in the attempt, striking his hip and elbow. He received no blow on the head at all. Three hours afterwards he found that he could not *spit* properly. The affection is not unfrequently discovered by that circumstance. He could not avoid spitting on his clothes on one side; and he could not whistle. Another circumstance worthy of notice took place in this man, which often, though not always, happens in these cases, and which I did not mention before. He remained for about two months in the hospital; and regained during that time in some degree the power of exercising the affected muscles; but he still was unable to close the right eyelid. The eye itself was unharmed. After he was made an out-patient he resumed his functions on the coach-box; and his eye, permanently half open and unprotected, was more exposed to cold and to currents of wind than it had been when he was an in-patient. Moreover

he got drunk ; and he soon presented himself again with universal redness and inflammation of the conjunctiva. Sometimes the inflammation in such cases produces opacity of the cornea and a total loss of vision. This is one of the worst consequences of facial palsy. Fortunately it is only an *occasional* consequence : and it will occur or not, according to the quantity of motion which remains to the eyelids, and the degree of exposure to the ordinary causes of inflammation.

Sometimes the palsy depends upon manifest *external disease* ; sometimes upon disease which is hidden, and probably *internal*, in the bony canal. Sir C. Bell describes an instance in which it accompanied the disorder called the mumps. Dr. Malden, of Worcester, witnessed another in which a fixed, hard, indolent tumor, had formed between the ramus of the lower jaw and the mastoid process of the temporal bone. As this tumor gradually subsided, the palsy disappeared. In each of Dr. Powell's three cases the affection was apparently caused by exposure of the side of the head for some time to a stream of cold air. A medical acquaintance of mine, residing in London, had a patient at Greenwich whom he visited daily. It was cold weather ; and on one occasion, as he was returning in the cabin of a steam-boat, he was sensibly incommoded by a keen east wind, which blew through an open window directly upon his ear. The next day he presented himself to me with that side of his face fixed in the manner I have been describing. Exactly the same mishap befel a Scottish physician while travelling to London by a coach ; and sent him in great alarm to Sir C. Bell. Six years ago a marked example of facial palsy occurred in one of my hospital patients ; it appeared to be owing to his having been constantly in the streets for some days without shoes or stockings, during a cold thaw. It may be presumed that in these instances some swelling was produced in the soft parts around the nerve, compressing it where it lies within the unyielding bone. Exposure to cold in this way is the commonest of all the exciting causes of the complaint, and cases thus arising are more obedient to treatment than most others. Probably some of you saw a female patient who came under my care in the hospital in May last, (1838), in whom facial palsy had existed on one side for eighteen years. When about three years old she had the measles ; and a scrofulous tumor formed behind the ear, and broke ; and after some time, a portion of carious bone came away. Then the wound healed (of which deep traces are still visible) : and the peculiarity of her features was observed. There are still other cases in which we fail to discover any direct explanation of the paralysis, either in the his-

tory of the patient, or in his physical condition. In the girl Smith, whose symptoms I stated in detail as an example of the appearances uniformly present, the malady came on without any obvious cause, and it resisted all the means employed for its removal.

That the greater number of cases of this kind are free from serious peril, is a fact of great practical importance. It enables us to quiet the alarm of the patient and his friends ; and it regulates in many instances the *treatment* ; rendering it less severe than it might and ought to be, if the palsy were really the harbinger of apoplexy. At the same time you should not be ignorant that a similar limitation of paralysis to the particular muscles supplied by the portio dura is sometimes (though rarely) observed, when the disease has a more inward origin ; when it affects and involves the brain itself. The following case caused me much anxiety, for the subject of it was a personal friend of mine :—I was summoned to his house in the autumn of 1829, and found him with complete palsy of the left side of the face, which had existed a day or two. I shall not describe the appearances and symptoms that resulted from the paralysis ; for they were precisely the same as were presented by the girl Smith ; and they are always, and necessarily, very much alike. But though the *palsy* was strictly limited to this set of muscles, there were other symptoms present which indicated that the interruption of the functions of the portio dura was connected with some morbid condition within the cranium : nausea and vomiting, twitching of the muscles of the *other* side of the face, great drowsiness, and a slow pulse, 48 only in the minute. He lurched also, and staggered as he walked ; but he distinguished this from the reeling of vertigo, and denied the latter sensation altogether. He was deaf, too, on the affected side.

His previous history did not tend to diminish the fears which his actual state excited.

In the preceding February he had been attacked, rather suddenly, with intense pain just above the right eyebrow, and became extremely drowsy. Being desirous, on account of these feelings, to excuse himself from a dinner engagement, he found that he was unable to write a proper note : he could not remember how he ought to express himself.

All these symptoms soon passed off ; after the operation, I believe, of an emetic. But he had another attack of the same kind in the subsequent May : the same severe pain over the right brow, with great drowsiness and confusion of mind. He could not recollect the first line of the *Æneid*. He wished a friend to look at the *signatures* of some letters that

had arrived: and though he knew the root, he could not tell how the word he wished to use was formed; whether it was signition, or signation, or signature. The digestive organs on this occasion were made the object of treatment; and he soon got well.

There was another instructive part of his history; and therefore I mention it. Before these attacks he was in the habit of eating and drinking freely; and his power of digestion was supposed to be enormous. After the attack in May he commenced a strict course of temperance. He drank no wine *till three or four days before* the occurrence of the facial palsy: he had then taken it again, and had about four glasses daily; and on one of the days he drank two glasses of Champagne.

It was of some moment to this gentleman, not only that he should recover, but that he should recover quickly. He had been appointed by Government to a mission to Ceylon, and all his equipment was already on board a vessel, which would sail in a fortnight.

Cupping behind the ears, blistering, purgatives, and small doses of calomel continued till the gums were slightly sore, removed the paralysis, and all the other symptoms, in about ten days. He went to Ceylon, and performed his mission so ably that after his return the Government appointed him to one of far greater importance in India, where he now is. He has remained perfectly well; and possesses one of the clearest and strongest intellects that I am acquainted with.

I must trouble you with one more case, to complete the history of this disease: a case in which the cause of the facial palsy was situated within the cranium, and proved fatal, and became visible after death.

Samuel Dovey, a tailor, 57 years old, was admitted under my care into the hospital, in February 1833, with complete palsy of the muscles supplied by the portio dura on the right side; and of no others. There were symptoms enough, however, to shew that some serious mischief was going on within the skull. He suffered intense headache, more on the left than the right side; was dizzy and staggering; and could not get to the ward without being led.

The palsy had come on about ten days before, in the night. He found when he came down stairs the next morning that he could not spit as usual; and his friends observed the unnatural state of his features. He had had no fit, nor loss of consciousness; but he thought his memory was failing. At the time when the paralysis was first noticed, he had some numbness and tingling of the right arm, extending to the last two fingers. He was quite deaf in the right ear. This is a point deserving attention in such cases. The deafness, when it occurs, marks an

affection of *both the portions* of the seventh nerve; and therefore indicates the probability of an *internal* cause.

The whole progress of this case was very interesting; but I must confine myself to those circumstances which bear upon our present topic. He lived about a month after his admission, and during that interval he suffered great pain in the head, was delirious at times, and at other times in a state of coma: at one period he suddenly presented the ordinary symptoms of apoplexy, from which he partly recovered.

I found a cancerous tumor occupying the right hemisphere of the brain; at its under part was an apoplectic clot, as big as a hazel nut. I found also a very satisfactory explanation of the deafness and the facial palsy which had been noticed during his life-time. The portio dura and the portio mollis, where they emerge as distinct cords from the medulla oblongata on the right side, were adherent to each other. The portio dura was both harder and larger than the corresponding nerve on the opposite side, while the portio mollis was wasted and diffuent. The same change was traced up to their entrance into the petrous portion of the temporal bone. Immediately over the medulla oblongata, and in a vertical line above the point of emergence of the seventh pair of nerves, a nipple-like portion of brain projected downwards, and had apparently communicated pressure to these nerves; and this projection from the lower surface of the brain seemed to have been produced by the general pressure resulting from the growth of the tumor.

The remarks which I have been applying to *palsy* of these parts hold true also in respect to their *loss of sensibility*. The anæsthesia may or may not portend danger to life, according as the interruption of nervous function on which it depends is situated more or less near to the origin of the fifth pair of nerves in the brain. The patient, Church, whose case I have several times referred to in this lecture, left the hospital with the sensibility of her face nearly as perfect as ever. The treatment consisted in local blood-letting and counter-irritation. She had erysipelas of the head while in the hospital, and was in some danger from that complaint, which was attended with a good deal of fever and delirium. With the exception of the delirium, which belonged no doubt to the erysipelas, there was no reason to suspect any affection of her brain.

Treatment of facial palsy.—I have incidentally adverted to the plan of treatment to be pursued in these cases of facial palsy. When the complaint is recent, and has an obvious cause, the appropriate remedies will readily suggest themselves. When, for example, it has come on after exposure to a

current of cold air, or after a blow, or any circumstance likely to give rise to inflammation, you must treat the case as you would treat inflammation; bearing always in mind that a small amount of disorganization, a little thickening or induration of the parts around the nerve, may render the deformity and the inconvenience *permanent*. If there be inflammatory fever, bleed from the arm: if there be not, take blood from the neighbourhood of the affected nerve by cupping: apply fomentations; or, what is better in these cases, conduct the steam of hot water against and into the ear: and administer mercury so as just to touch the gums. I should always take this latter precaution, lest any effusion of lymph should cause abiding pressure on the nerve. If the palsy gives way before the gums become tender, the mercury need not be pressed farther.

Where there is any ground to suspect that the brain is implicated, the treatment just described must be pursued with greater diligence, and with such modifications as the nature of the case may require. If there be evidence of chronic disease in the petrous portion of the temporal bone, such as tenderness of the mastoid process, deafness, a protracted discharge from the ear, and an imperfect state of the membrana tympani, we can scarcely expect much good from very active treatment. We must then have recourse to counter-irritation, and such other measures as I spoke of when the subject of otitis was briefly considered.

Other forms of local palsy and anæsthesia.—The examples which are met with of local palsy, and local anæsthesia, are numberless; but those which I have mentioned are the most common and the most important. They are always deserving of attention; but more so when any suspicion arises that they may be connected with cerebral disease. Sometimes they evidently have no such connexion. In the month of November, 1834, a coachman became my patient in the hospital with imperfect paralysis affecting some of the muscles of the right foot alone, with numbness of the foot. He could both stand and walk; but on advancing that leg, his foot flapped suddenly down, and he could not deliberately direct and plant it, like the other. His general health was quite good; he had no headache, or giddiness, or palsy of any other part. But a month before he had been sitting with the right leg thrown over the opposite knee; and he continued in that position until the foot felt numb, tingling, and was (what is called) asleep; and it had remained in the same condition from that time. After some general treatment (cupping and purgatives) before he came to the hospital—treatment which was quite proper in the way of pre-

caution, but which was probably, in truth, unnecessary—I had his leg electrified; and in about ten days the sensation and the power of the limb were almost restored. Mr. Swan mentions a somewhat similar case, in which anæsthesia of the hand was produced by strong pressure made upon the wrist.

There are some very curious facts, connected with anæsthesia, shewing that the voluntary exercise of the muscles is regulated in some measure by the sensations of the limb that is employed. The sense of resistance prompts to such contraction of the muscles as is required to balance that resistance; reminding the will (so to speak) of the necessity that exists for its perpetual and vigilant operation. Continued volition is essential to the continuance of the muscular tension. Thus Dr. Yelloly describes a woman who had no power of feeling in her hand and fingers, although the power of moving them, and of grasping any objects, was entire. This woman found that she could carry glasses or plates in that hand very well and safely, if she continued to look at and attend to them; but if her eyes were turned another way, as she did not feel what she held, she was very apt to drop it. Dr. Ley met with just such another case. A woman had defective sensibility on one side of the body: she could hold her child in the arm of that side so long as her attention was directed to it; but if surrounding objects diverted her from taking notice of the state of her arm, the flexor muscles soon began to slacken, and the child was in danger of falling. All this is exceedingly curious.

Andral has recorded a most singular example of local anæsthesia, which preceded an attack of apoplexy. The patient lost, from time to time, all sensation in certain isolated parts of the skin upon the thorax; there were five or six of these insensible spots, each about the size of a five-franc piece. You might pinch the skin in these places without producing the slightest feeling in the patient. In all other parts the sensibility was perfect and lively.

There are other cases also on record, more remarkable still; in which the patients have lost both the power of motion and the faculty of sensation in almost every part of the body, and yet have survived for a considerable time. Thus one person (whose case is related in the *Bulletin des Sciences Médicales* for Jan. 1828) became first amaurotic, then deaf, and then by degrees lost all power of sensation and motion except in the tongue and in the muscles of deglutition and respiration. His speech and intellects were unimpaired. It was accidentally discovered that a small patch on the right cheek retained its sensibility; and by

tracing letters on this sensible spot, his wife and children were enabled to interchange ideas with him. He died at length, and his body was not examined.

I shall finish what I have to say on this head, by relating a case of the same kind, which occurred under Dr. Abercrombie's notice; and which we are sure therefore would be observed with care, and recorded with fidelity.

A servant girl, about 20 years old, sprained her back in lifting some heavy article of furniture. She felt no great inconvenience at the time; but some little while after, weakness of the legs came on, and gradually increased to perfect paraplegia. After an interval, the affection extended to the arms, and she then had not a vestige of motion of any of the parts below the head, except a very slight movement of one of the fingers: but the internal functions were all perfect, and her speech was distinct, except that in speaking she was sometimes seized with spasmodic twitches of the lips and lower jaw. She lived in that state, without any change of the symptoms, and her general health continuing good, for about twenty years. In the morning she was taken out of bed, and placed in a chair, so contrived as to support her in a sitting posture. Her arms rested on a cross board which passed before her; and if by any accident one of them slipped from this support, she had no resource but to call the assistance of another person to replace it. Having been on one occasion left alone for about two hours after one of her arms had thus slipped down, the hand had become extensively oedematous. In the same manner, if her head fell forward upon the thorax, it remained in that position until raised by an attendant. Her mind was entire. She died after four days illness with symptoms of low typhus fever. You may suppose that Dr. Abercrombie looked with the greatest interest for the cause of these most remarkable symptoms. "I examined the body with the utmost care, (says he) along with Dr. Pitcairn, who had been in the habit of seeing her for several years; and we could not discover any disease, either in the brain or in the spinal cord."

It is much to be regretted that when this case was under observation, the excito-motory functions were not understood, nor attended to.

I shall next proceed to consider those diseases (and there are several of them) which are marked by definite symptoms, which consist essentially in some disturbance or disorder of the nervous system, but which are not accounted for by any physical changes that we can appreciate in any part of that system. After some of these diseases we do, to be sure, sometimes meet

with morbid appearances in the brain and spinal marrow: but none that are constant, or uniform.

CLINICAL LECTURES,

BY DR. CORRIGAN,

*Delivered at the Hardwicke Fever Hospital,
Dublin,*

During the Session, 1840-41.

LECTURE V.—FEVER, No. 3.

Nature of Fever, and principles of its Treatment—Rate of Mortality—Importance of attending to the State of the Bladder, and Necessity of using the Catheter.—Certain Remedies to be avoided, particularly Bleeding, Sudorifics, & Purgatives.

WE have, I hope, by our observations of last day's lecture, in some measure cleared the way for an understanding both of the nature of fever, and of the principles of treatment, to which we shall devote this day's lecture.

Before presuming to recommend any particular lines of treatment, it is only right to ascertain whether the results are such as to recommend it. I believe it will be found that they are. Mr. Edgeworth has been kind enough to draw up a table of the admissions and deaths, and causes of death, from July 1, when the wards came under my care, to December 31, a period of five months. The total number of admissions within that period has been 297; total number of deaths 12. Of these deaths there were from—

Scarlatina, 2, A. Prendergast, and E. Mulalley; pleuro-pneumonia, 1, Charles Doyle; pleuritis and empyema, 2, J. Finlay and T. Kavenagh; old age, 1, L. M'Keow; hydrocephalus, 1, Margaret Fitzgibbon; hæmorrhage, 1, Margaret M'Gregor; cholera, 1, Bridget Foran; meningitis, 1, John Neile. Total, 10. We have had during this period only two deaths from fever—Honor Walsh, æt. 22, who died on the 14th day; and Anne Mayne, admitted in a dying state. Our statistics for five months will then stand thus:—cases of maculated fever admitted, 287; number of deaths, 2: which gives us as our rate of mortality one death in 143½. This is a very favourable result, and the treatment under which it has occurred we shall now go into a little in detail.

There is one organ to the state of which I have repeatedly called your attention, and impressed on you the necessity of closely watching, viz. the bladder. The case of Murphy afforded us a good illustration on this point. He was very ill in maculated fever—so violent that it was necessary to put a strait-waistcoat on him. His delirium was furious; his tongue was dry and brown;

his pulse beating above 130 ; his skin covered with both maculæ of fever and petechiæ of purpura. He had not slept, and his eyes were suffused : he passed his fæces in bed, and we were positively assured by the nurse that he had also passed urine copiously under him. This report seemed to be confirmed at first sight on turning down the bed-clothes, for there was a strong urinous smell ; the clothes were stained by the urine ; and the urine was seen welling from the orifice of the urethra, and dribbling over the thigh. Notwithstanding all this I had the catheter introduced, and there were drawn off certainly not less, and I believe more, than two quarts of urine. You can easily conceive, were the bladder allowed to remain thus distended, what the ultimate effect on the system would be. It is hardly possible to suppose any thing more calculated to destroy life in fever than such a state of bladder ; the organ so distended as to have lost its muscular power, and no urine passing off, save the surplus quantity which can no longer find room in the distended bladder. It is a rule of practice I would most earnestly impress on you in the treatment of fever, never to omit the introduction of the catheter in cases where there is either delirium or coma, unless you have the quantity of urine passed preserved for your own inspection. If, on the introduction of the catheter, there be no urine in the bladder, there is no harm done, and you have satisfied yourselves ; if there be urine present you perhaps save your patient's life, by freeing him from the irritation produced by the distension of the bladder, and from the bad effect of the retention of so much excrementitious fluid, which, if not removed, will at least in part again be absorbed to his serious injury. Never in such cases trust to the report of the nurse as to the quantity of urine passed. Trust in nothing but the inspection of it ; or, what is better, the test of the catheter. You may suppose that in such cases examination of the hypogastric region will be sufficient for your information as to the passing of urine, without the introduction of the catheter. Believe me, it is not to be trusted. I shall never forget a case that impressed this strongly on my mind. A gentleman, of low stature and full habit, was lying comatose from erysipelas of the head and face, and had for several hours passed no urine. Were I to mention the names of the gentlemen who were in attendance with me on the case, it would be a sufficient guarantee that the examination of the region of the bladder was made with care and tact, and the result of that examination was almost a conviction that there was no urine in the bladder, and that the case was a fatal one of suppression ; still the catheter was employed, and a quart of urine was drawn off. The result of attention to the

bladder two or three times a day was the gradual clearing up of the coma, and the recovery of the patient. You might also suppose that percussion of the hypogastric region would be sufficient to detect urine in any amount in the bladder. It is not. Either from the natural inclination of the bladder, its being thrown more backwards in some persons than in others, from a degree of tympanites in the intestines, from the posture of the patient, who always, in the cases where the examination is most called for, has lain for many hours on his back, or from all those circumstances combined, there will be clearness on percussion over three-fourths or more of the hypogastric region ; and yet the bladder will contain a large quantity of urine, which the patient, in the oppressed or debilitated state of his nervous system, will not pass. We have many times percussed the hypogastric region, and elicited a clear sound ; and yet immediately afterwards drawn off a large quantity of urine. Even though the quantity taken away from the bladder be not very great, it is probable that there is a good effect produced by its removal. Its removal will probably encourage a free secretion from the kidneys ; and a copious secretion of urine is one of the most favourable circumstances we can have in fever. The ultimate conclusion at which I think we must now arrive, relative to our consideration of the state of the bladder, is, that we ought to trust neither to the report of the nurse nor to the examination of the hypogastric region by the hand, nor to percussion, but only to the catheter, or to the inspection of the actual amount of urine passed from visit to visit. I do not think I can overrate the importance of attention to the state of the bladder in stages of fever where there is great nervous debility, coma, or delirium.

We shall now proceed with other parts of the treatment, and perhaps it may be not more necessary to say what treatment we have followed, than to point out what practice we have avoided. But let me not be understood as now presuming to recommend the line of practice at present adopted as applicable to fever in general ; it is to be understood as applied only to the present epidemic of maculated fever. There are three active remedies which I have found it necessary to abstain from—general bleeding, sudorifics, and cathartics. General blood-letting has not been employed in a single instance ; nor has there occurred one among so many cases where even a thought of its employment could suggest itself to the mind. This forms a very remarkable feature in our present epidemic, as it belongs, perhaps, not more to the character of the fever than to the state of the human constitution at large, at the present time at least, in this

country. The fact is obvious to every one of observation, that not only in fever but in other diseases, in acute inflammatory attacks, there is neither the same necessity now for active blood-letting that existed some years since, nor would it even be borne. In relation to the capability of bearing blood-letting or depletion under disease, there has occurred in the human constitution, within a period of some years, perhaps most remarkably since the invasion of cholera in 1832, a very marked change. This singular change in the human constitution, and which perhaps follows something like a regular cycle, is always first and most remarkably seen in fever, because fever engages more of the vital functions simultaneously than any other disease; and thus the phenomena of fever are deserving of our closest attention, not only for their own sake, but as affording an insight into modification of treatment in local affections. This singular change in the human constitution, and which as positively occurs as a change of climate or of season, affords us an explanation of those changes, or even apparent contradictions, in treatment, that are sometimes found in writers of different times. Those changes in the treatment of diseases, that have occurred from time to time, which are, for the reason already noticed, always most remarkable in fever, but which are also seen, in corresponding degree, in all local diseases, are occasionally alluded to by persons of shallow mind, or little comprehension, as evidences of changeableness and uncertainty of principles in the practice of medicine. If, while constitution and disease were running their cycle of change, our treatment, like Sangrado's, were always to remain the same—unsuited to constitution, unadapted to symptoms—then, indeed, the criticism might pretend to some show of truth. Those who would esteem a settled and unvarying line of practice to be suitable and best for all times and changes, exhibit as much sense as the savage lawgiver, who proclaimed that in all stages of changes of the social constitution his rules and laws should be immutable. These general observations may seem at first unsuitable to a clinical lecture. I do not think they are. I do not think the fact of the change which occasionally comes over the human constitution, and which always becomes most perceptible in disease, is generally sufficiently kept before the mind of the practitioner. Attention to it at the bedside is not less necessary than attention to the disease itself; and how soon the present type of disease and present constitution of the living system may change their characters, it is quite impossible to say. If we bear in mind that the change has already frequently occurred, and will certainly occur again, we shall be better prepared to detect it and to meet it.

Sudorifics come next as a treatment to be abstained from in our present epidemic. Of all crises or terminations of our maculated fever, that by perspiration is the most to be dreaded. Crises may take place imperfectly by urine, by sleep, even by diarrhoea, by any of these, even only by remission, and the case be even benefitted by the partial crisis; but if after the ninth or tenth day a copious perspiration comes on, and that the strength show the slightest tendency to sink under it, or, what is still worse, that the pulse rise in frequency while the perspiration is coming out, the termination will almost certainly be fatal. In fact, recovery under such circumstances is an exception. I have such a dread of the crisis by perspiration, that I would rather see the fever considerably prolonged than run even the chance of the consequences of perspiration. I am confident one of the causes for fever being more fatal in private life than in hospital is, that from the feather beds, the warm drinks, the heavier covering of private patients, there is in such patients a greater tendency to perspiration than in the cool wards, the light covered beds, and the cool drinks of the hospital patients. Every thing that can encourage perspiration in our maculated fever is to be avoided; and if the patient, as sometimes happens, has been in the habit of wearing a flannel vest, or dress, in bed, let it be removed. It sometimes requires the most peremptory order on the part of the physician to enforce those regulations. Popular knowledge, or rather ignorance, confounds the maculated or typhoid fever with the common inflammatory fever or synocha. A copious perspiration will cure a patient in the latter; it will probably kill in the former.

Purgatives are the third on our list of remedies to be abstained from, or to be cautiously used. We have used them very sparingly indeed. In by far the greater number of the worst cases we have never administered a purgative by the mouth, and there is this satisfactory result, that during a period of five months, in 287 cases of maculated fever we have not had a single case of urgent intestinal tympanitis. Among the complications of fever there is scarcely one more to be dreaded than a tympanitic state of the intestinal canal. Even without fever at all, as a mere local disease, distension and want of tone of the intestines, as in one form of ileus, is sufficient to kill. What, then, must be its danger in a constitution with not one, but every, vital function depressed to its lowest ebb. We are to estimate the value of any particular line of treatment in fever, not alone by the symptoms we can combat, but by the dangers we can prevent; and if by adopting a particular line of treatment the occurrence of a dangerous complication of fever is prevented, that line of

treatment recommends itself even more strongly than a line of practice which would permit the danger to arise, and then turn to combat it. There has been no necessity in our epidemic for the use of purgatives; the bowels have been almost always moderately free, or, if not so, or if a tendency to tympanites has shewn itself, an enema has always been sufficient for the purpose of procuring a discharge, or removing flatulence. We cannot cut the fever short by purging. We can do no good by it; but we may do much harm, both by lowering the strength considerably, and by producing mucous irritation. We had better, therefore, refrain from it; and this recommendation is supported by the best of all tests—the practical results.

Having noticed some practices to be avoided, we shall next turn our attention to treatment to be adopted.

AN ESSAY

ON THE

SOURCES OF TYPHUS OR CONTINUED FEVERS.

By JOSEPH BELL, Surgeon, Barrhead.

[Concluded from page 144.]

HISTORY from the earliest period informs us that fever has been a too constant attendant on war. This connection has become proverbial in Germany, where it is called war-pest (*Kriegspest*).

The continental epidemics have either accompanied or succeeded seasons of poverty and destitution. The fevers of Italy in 1764 and 1817 took place under circumstances of great want. The epidemic which raged at Louviers in Normandy in 1770, broke out under similar suffering. Lepecq de la Cloture informs us “that for a number of years the price of provisions had become extremely high, and that suffering having greatly increased, the commune of Louviers was considerably debilitated, which caused the manufacturers to stop a part of their work; consequently a number of operatives remained without employment, and the wages of others were diminished. What happened then? One did not see, as formerly, the workmen enjoying themselves on fête-days, and destroying a part of their care in wine. No; there were for them no more movements of joy. Sadness was painted in pallid colours on every countenance. Misery had crushed their spirit. In short, they

were reduced for the most part to deprive themselves of some of their miserable furniture; to dispose of them that they might satisfy their hunger, and to provide some morsels of bread to the urgent calls of their children.”

In Scotland the prevalence of fever has been also coexistent and coextensive with privation.

This appears evident from the following facts adduced by Dr. Alison:—

1st. “It appears, from observing the times of these epidemics, the first in Edinburgh beginning in 1817, after two bad harvests, and at the same time as the Irish one; the next in 1826, after the great failures in 1825, and the sudden cessation, particularly of building speculations, in Edinburgh; and the last in 1836, after the great depression of trade both in Glasgow and Dundee, with which towns the lower orders here are much connected, and under the combination of other circumstances already mentioned, which have depressed the condition of the poor in Edinburgh of late years*.”

In consequence of this depression, Dr. Alison argues, fever has been greatly on the increase. “For many years past,” he observes, “contagious fever has never been absent from Edinburgh, and there have been three great epidemics of that disease in the last 22 years, beginning in 1817, 1826, and 1836, each lasting nearly three years, and each of the last two affecting, I believe, nearly ten thousand persons. The number of fever patients admitted into the Infirmary and Auxiliary Fever Hospital, from November 1817 to November 1820, was 3090; from Nov. 1826 to Nov. 1829, it was 4318; and from October 1836 to October 1839, it was 4850.”

The increase of typhus has not been confined to Edinburgh. Some of the other large towns have suffered to a still greater degree, especially Glasgow. This will appear obvious from a glance at the Vital Statistics of Glasgow, by Dr. Cowan. According to this physician, the number of fever patients admitted into the Glasgow Royal Infirmary during seven years ending 1802, was 484. The following seven years 512 cases were admitted; and from this till 1816, a period of seven years, 574 were admitted. From 1816 to 1823, the number increased to 3866, and

* Management of the Poor, &c. p. 13.

the following seven years it rose 6075; and from 1830 to 1836 inclusive, the number of admissions amounted to 11,750.

This calculation, however, can be merely regarded as an approximation to the actual number of fever cases which occurred in Glasgow. The hospitals appropriated to the reception of patients labouring under fever are not adequate to admit the whole of the poor who are seized with the disease. Many cases must have also occurred among the better classes of society. That this increase of fever is owing to the same cause which produced it in Edinburgh, is quite evident from Dr. Cowan, as well as from the testimony of several others who have investigated the subject. It is true that Dr. Cowan states a circumstance which at first sight appears to oppose this view, but on a second consideration it will be found to have no such tendency. At p. 13 of his *Vital Statistics of Glasgow*, he observes, "This increase, especially during the last seven years, (from 1830 to 1836), has taken place, not in years of famine and distress, but during a period of unexampled prosperity—a period when the wages of labour have been ample, the prices of provisions comparatively low, and every individual able and willing to work, secure of steady and remunerating employment." This state of prosperity did not long continue, and has not yet returned. At page 34, Dr. Cowan informs us that "from the close of 1836, one of those periodical depressions in trade . . . deprived a large proportion of the population of the means of subsistence . . . a very large proportion of the inhabitants were suddenly deprived of employment, and consequently of the means of procuring food. The high price of coal was the means of diminishing the hours of labour, and consequently the amount of wages in numerous factories, and placed fuel beyond the reach of the lower classes for domestic purposes."

To these sources of destitution we may add the bad harvest of 1836, and consequently the high price of food during the latter part of that year, and the whole of 1837.

We perceive that the period of prosperity was prior to 1836: during the latter part of which year these privations commenced. We find that when

the people had constant employment, and obtained good wages, fever prevailed to a much less extent than it has done since. In 1835 the number of fever cases which occurred in Glasgow is stated by Dr. Cowan to be 6180; in 1836 it increased to 10,092, and in 1837 to 21,800.

The working classes of Glasgow were in the same destitute condition during the epidemics 1816-17-19. There was great stagnation of trade. The operatives were subjected to great suffering, especially hand-loom weavers; 5256 of whom were out of employment in 1819. And Dr. Cleland informs us that "in 1817, 23,000 persons applied to the charitable funds for relief."

From the foregoing facts we perceive that, between the increase and spread of fever and a state of destitution, there exists a very close connection—a connection which has been maintained since 1817, not only in Glasgow, but also in the other large manufacturing towns of Scotland. We shall only instance Dundee. Dr. Alison, after detailing the causes of destitution to which the working class of this town have been exposed since 1818, observes, "that prior to 1818 little demand was made, comparatively speaking, on the Dundee Infirmary for the reception of fever cases; but during that and the following year the disease raged to such an extent in town and suburbs, that the house became inadequate to the wants of the community. Since that period the progress of the disease has been various, but on the whole *vastly on the increase*. The last epidemic appears to have attained its maximum there in 1836, when the fever patients in the infirmary were 773, and the deaths from fever stated in the mortality bills (which I understand to be kept very accurately) are 297. But in the years 1836-7-8-9, fever continued very prevalent; and the whole number of deaths in these years having been 7160, or 1790 a year, in a population which in 1831 was 45,000, but is since supposed to have extended, chiefly however by accessions from the country, to above 60,000. From these facts we may infer that nearly 10,000 inhabitants of Dundee, or nearly one in six of the population, must have had fever in these four years."—p. 14.

Such, then, is the connection which has usually existed between the pre-

valence of fever and the existence of poverty.

I am convinced that the history of fever in the town of Paisley will afford an illustration of this relation. It has done so in the village of Barrhead for the few years that I have resided there. This is particularly the case at present. The population is chiefly composed of operatives, many of whom have been in very destitute circumstances for the last six months; and fever, which was rare previous to that period, has prevailed extensively since, principally among the poor; many of whom are in the most abject misery, having neither clothing, fuel, nor food. Since the month of August last, in one range of houses inhabited by 272 individuals, sunk into the depth of poverty, 61 cases of fever have occurred. In many of these instances no other source of fever could be traced on the most careful investigation, except poverty.

From the universal co-existence of privation and fever, which the mass of facts adduced in the preceding pages amply prove, we are justified to infer that these two evils frequently stand in the relation to each other of cause and effect. We have seen that wherever disaster and destitution prevailed for any length of time, affecting large communities or congregations of men, fever is a too certain and constant attendant.* We have seen, from ample experience, that in times of scarcity and of stagnation of trade, and disasters in military affairs, fever has raged with dreadful virulence. Many other circumstances corroborate the opinion that fever is frequently originated from privation. Do we not find large communities, placed under exactly similar circumstances as regards their habits of life, their mode of dwelling together in crowded and ill-ventilated apartments, surrounded by noxious effluvia? But those who compose some of these communities are found to suffer the pangs of hunger to an extent wholly unparalleled to the members of the others, where the poor are shielded from this abject misery. Do we not find that among the former fever prevails to an extent out of all proportion to that which occurs among the latter? When we find these very differences occurring in a marked degree between the poor which

inhabit the large cities of England, and those which reside in our Scottish towns, our opinion of privation being a source of fever is strongly confirmed. The English pauper is in many respects placed under circumstances similar to his brethren of the north, differing, however, widely, in the extent of destitution and the prevalence of fever. The poor of both parts of Her Majesty's dominions are exposed to the influence of malaria, originating from the same sources: they are alike crowded together in small, ill-furnished, and badly ventilated apartments; they are of the same filthy and intemperate habits. The only difference in their condition is, that those who have the good fortune to live south of the Tweed are protected from the awful destitution which exists among the poor who live on the north side of that river. In Scotland the vast majority of our poor are wholly unprovided for by parochial relief. Those to whom this charity is extended receive a trifling sum, about two shillings per week—a sum totally inadequate to their maintenance; whereas, in England, three times this amount is given. In England the average expense for maintaining the poor is still five shillings and tenpence per head on the population. Previous to the late shameful reduction it was much larger. It is, however, nearly five times as much as that which is levied in Scotland. It is less than sixteen-pence. Let us examine, then, the extent to which fever attacks the poor of these two countries.

In Manchester the cases of fever admitted to the hospital in 1836 amounted to 780, or one to every 292 of the population. In Leeds the number treated in the fever hospital was 274, or one to 450 of the inhabitants. In Newcastle and Gateshead the number admitted to the fever hospital is 39 annually, or one to 1485 of the population. In Carlisle, where fever was epidemic in 1831, one out of every 121 of the population was admitted to the infirmary; previous to this year 63 cases was the average number admitted. In Sunderland, with a population of fifty thousand, only 17 cases of fever were taken to the hospital in each year, during 1836-37-38. In Oxford, fever occurs in the proportion of one to every 3200 of the inhabitants. Liverpool, in 1836, presented to the hospitals 1700 cases of fever, or one for every 112 of the population. In

* Vid. Brit. and For. Med. Review, No. xxi.

London, during the epidemic of 1838, one out of every 67 of the population was seized with fever. The population of the four districts of this city* where the fever principally raged, amounts to 253,784: of this number only 6097 were seized with fever, or one out of every 42.

Let us compare these numbers with Glasgow. In 1836 the admissions to the fever hospitals amounted to 3125, or one out of every 78 of the inhabitants; one out of every 22 of the inhabitants was seized. In 1837 there occurred 21,800 cases of fever out of a population of 244,000, or one out of every 11 of the inhabitants. In Edinburgh and Dundee we have seen that the inhabitants suffered to a proportionate degree with the citizens of Glasgow.

From these facts we have the painful conclusion forced upon us, that the poor inhabitants of Scotland are subjected to the inroads of fever to an extent totally unparalleled in England, where the destitute obtain more adequate assistance. It may be argued that the prevalence of fever among the poor of Scotland cannot be considered as any proof of poverty being a source of fever, so long as the same disease attacks, though to a less extent, the English poor, who do not suffer the same privation which is experienced in Scotland. It has been said that if fever arise from poverty, it cannot prevail where the evil does not exist. In England, therefore, where the poor have a provision made to protect them from destitution, there should be no fever; but in England fever does prevail to a very great extent—*ergo*, poverty cannot be said to be the source from which it originates. Those who adopt this mode of reasoning forget that the destitution in Scotland and England is only comparatively different: they overlook the important fact that the poor of these two countries differ in the extent of their privations. The English pauper is only half provided for; whilst the poor Scotchman is left almost wholly destitute. Now we find, from the most incontrovertible authority, that the prevalence of fever in England and Scotland also differs in extent in the same ratio in which the state of destitution is different. Both countries will ere

long, however, be reduced to the same level in these respects, if it be in the power of the Poor Law Commissioners to effect it. We must not overlook the powerful influence which contagion exercises in generating and propagating fever. We should also take into consideration that, in England, many causes exist which increase the virulence of contagion, and assist its diffusion, such as bad ventilation, &c. This special pleading can have little effect in opposing the conclusion that poverty is not only an attendant on fever, but a source of the disease, supported as this opinion is by the mass of facts adduced from all quarters.

To conclude, we have seen, then, in the first place, that contagion is, beyond all doubt, a source of typhus fever: the evidence on which this inference rests is of the most direct nature.

2. We have examined the facts from which some medical men attempted to prove the malarious origin of fever. We have found, in the first place, that there is an insufficiency of facts to warrant this doctrine; in the second place, that the facts adduced do not prove the malarious origin of fever, but merely that fever generally prevails among the habitations of the poor, who do not receive adequate nourishment; thirdly, we have found many powerful facts, which completely oppose the existence of any such relation between effluvia and typhus as that of cause and effect.

3. We find an immense collection of facts to support the opinion, that privation is not merely an attendant on typhus, but is frequently (though by no means always) the very source from which the disease is originated. I wish these conclusions, however, to be viewed in their proper light—to be considered as the inferences which the facts that we possess on the subject authorize us to deduce; consequently they are mere approximations to the truth. Future investigations may reveal many circumstances by which they may either be still further confined, or completely overthrown. I would again repeat that I view them as correct deductions from the facts furnished by the present state of our knowledge of the phenomena attending the origin and propagation of typhus fever. I fully admit that bad ventilation and filth are great evils, and ought to be removed; they are doubtless active

* St. George the Martyr, Bethnal Green, White-chapel, and Lambeth.

agents in the propagation of contagion; but let the influence which they exercise be viewed in its proper light. I would strongly urge the removal of those beds of filth and nastiness which disgrace our cities, and mock our pretensions to civilization and humanity. I would warmly advocate the construction of sewers, the widening of streets, and the formation of places for recreation in the centre of these crowded cities. Let the external circumstances of the poor be made as comfortable as possible. Let measures be adopted to allow "the entrance of the light of the sun into their dark and gloomy recesses," and the sweet and invigorating breath of heaven "into their foul and reeking habitations." If such means were employed, I doubt not but that we would have less reason to lament over the ravages of fever, less cause to mourn over those horrid scenes of destitution and disease which we have had lately under consideration. But the advocates for the malarious origin of typhus would stop here. They contend that by proper ventilation, by the formation of sewers, and removal of filth, fever could be totally destroyed. At page 14 of the report "of the Sanatory," &c. to which allusion has been made in a previous part of this essay, they tell us "that by proper sanatory police regulations . . . the typhoid fevers of London and other places might be made to disappear."

This doctrine is fraught with the most dangerous consequences to the interests of society. We find it adopted by the Poor Law Commissioners, who recommend the principle to be put into immediate practice, in order to stop the ravages of fever. These gentlemen and their friends strongly maintained that nothing else is necessary to obtain this much to be desired object. Never were men more miserably deceived; and would that this were the only mischief which will arise from the practical adoption of these views. The eyes of the public will not be opened to the deception until thousands of victims shall have fallen a sacrifice to the very disease with which these men are not only tampering, but actually fostering. They might as well think to destroy the thousand ills to which suffering humanity is liable, as to imagine they can annihilate fever by merely removing the alleged causes of malaria.

Though they ventilate and clean the dwellings of the poor till they attain to the highest state of perfection, yet fever will still continue to rage; nor will it cease its devastations until you feed the poor—until you give them sufficient stamina to withstand its influence. Typhus originates from causes within the human body, not from any exterior source. To attempt the removal of the disease by destroying the external circumstances with which the poor are surrounded, and, at the same time, to allow the internal cause either to remain unnoticed or become aggravated, is as unwise as it is absurd. It is really painful to witness the endeavours of the Poor Law Commissioners to reduce the dietary of the poor to the very lowest ebb consistent with the maintenance of vitality. They seem to act on the principle of the man who managed to reduce the food of his horse to a straw a day; but, perhaps, would not regret with the man the result of this wicked economy. It is true, indeed, that the funeral expenses might give rise to a pang in the hearts of the junto of Somerset House.

These advocates of pork water and gruel, in their ardour to have filth and the other alleged causes of fever removed, as the only means necessary to ensure health and happiness to the poor, completely overlook the fact that filth and nastiness are the effects of poverty; they are its universal concomitants; and so long as privation continues, these conditions will exist. It is great folly, to say the least of it, to endeavour to remove the effects, and to aggravate the cause. These wiseacres, while they are earnest in their recommendations to clean the poor man's dwelling, seem exceedingly anxious that his inside should not be defiled with alimentary matters. They forget that human beings are not chameleons, and require something more substantial to maintain health and prevent disease, than air, however pure.

Would that the eyes of government were opened to the absurdity, or, I should rather say, the wickedness of the principles of economy advocated by the Poor Law Commissioners. The economy which they advocate is lavish expenditure. Let the poor receive better and more adequate nourishment, and we will have not only less fever, but less filth and nastiness to remove.

Be not content with the removal of the latter only. Destroy both it and the privation which exists along with it. Let not only the dwellings of the poor be well ventilated, and cleansed from the filth by which they are surrounded, but let the inmates receive adequate nutriment. Both measures are absolutely necessary to obtain the object in view, viz. the removal or mitigation of fever. If the poor were well fed, we might have more prospect of "proper sanatory police regulations" effecting some good in the extinction of fever. Then we might expect that this dreadful scourge which infests our large manufacturing towns would be stayed in its dreadful career of devastation. By thus enabling the poor to withstand its deadly influence, we might anticipate a stop being put to the progress of this enemy of human life, which is careering with giant strides over the length and breadth of our land, filling with wailings and misery the habitations, not only of the poor, but of every class of society. "Could the public money," observes an eloquent writer, "be appropriated to a more suitable purpose than the prevention of disease? The preservation of the public health is economy in every sense. It not only saves money, but it makes it. It substitutes men for children—productive for unproductive labours. It supplies a country with its best riches, and its cheapest defence—the arm strong to labour and to fight."

ON THE
SYPHILITIC DISEASES OF THE
LUNG.

BY WILLIAM MUNK, M.D.

Physician to the Tower Hamlets Dispensary.

[*For the Medical Gazette.*]

FROM the period at which syphilis is generally thought to have originated, or at any rate from that when it first forcibly attracted the attention of the public and of the profession, up to the latter end of the eighteenth century, a space of more than three hundred years, the study of its natural history, more particularly of its progress and of its tendencies under peculiar modifying influences, was but little regarded; writers on the subject giving themselves more to speculation than to

observation: the few, indeed, who pursued the latter and only legitimate course, being overlooked and forgotten amidst the extensive and overwhelming literature which had year after year been accumulating on this subject.

Towards the end of the period above specified—in the year, that is to say, of 1784—Hunter gave to the world his great and important treatise on the Venereal Disease; a work which, to use the words of a distinguished living writer, "poured a flood of light not only on the natural history of the disease but also on its pathology and treatment."

The merits of this work were so evident that it became at once, and as it were by acclamation, the guide for students and practitioners—the text alike of lecturers and writers. The latter did little more than follow out and yet further illustrate the various subjects discussed by Hunter; but few presuming to enlarge the boundary on which he trod. It seemed, indeed, as though the master-mind had grappled with the whole subject; had embraced within his comprehensive grasp all possible ramifications of the disease; had solved the most difficult problems in the inquiry, and had left only to his successors the working out of some secondary points, which time and more important topics had allowed him but briefly to shadow forth.

Any formal discussion on the peculiar merits of this important work would now, indeed, be superfluous, public opinion having long since, and most justly, awarded to it the highest place among the modern contributions to our art. Hunter studied the subject practically; he carefully observed the phenomena that came before him, reasoned cautiously upon them, and then committed to writing and to the world his matured conclusions concerning the disease. Further than this he did not go. The work was the pure result of his own observations and reflection, unaided by the facts or doctrines of predecessors or cotemporaries.

Hunter was a surgeon, and in his day the demarcation between surgery and physic was better comprehended, and less infringed, than at the present time. His opportunities consequently of observing the disease must have had reference chiefly to it when occurring

in those situations the morbid affections of which have by common consent, and long-established usage, been consigned to the surgeon's care. The diseases of the general surface, of the orifices of the body, as far as these are within the reach of the senses of sight and touch, of the bones, &c., are those with which the surgeon is chiefly busied; and syphilis, as affecting these parts, has been well described by Hunter. But the disease may fix itself upon other parts—upon parts not mentioned by Hunter or his followers; upon parts the diseases of which fall properly to the physician's province.

These circumstances will in some respects account for the late very general neglect of many internal syphilitic affections, the existence of which was fully recognized by the older physicians, and has either specially or incidentally been treated of in their writings. Perhaps the most evident, certainly not the least important, of these, are the syphilitic affections of the lung, a subject to which my attention was first directed, when a student in Holland, by the writings of my esteemed friend Professor Schroeder van der Kolk, of Utrecht*. It is to Dr. Graves†, of Dublin, however, that the profession in this country stand indebted for once more directing attention to this really important subject. In one of his clinical lectures published in 1835, he makes some highly interesting and practical observations on the disease, acknowledges that from the late Mr. Hewson he derived his first information concerning it, and expresses his surprise that the subject should for so long a period have been overlooked or neglected.

The records of our profession abound, however, with illustrations of pulmonary disease, the existence of which was attributed, and as it seems justly, to the contamination of the system by syphilis. Morton devotes the seventh chapter of his well-known *Phthisiologia* to the special consideration of *phthisis à lue venerea orta*, and concludes his observations with the detail of a well-marked instance of the disease cured by mercury and sarsaparilla. *Etiam verum est* (says he) *et in praxi*

non raro observandum venit, vere pulmonalem phthisin a mera sanguinis et humorum alteratione lue venerea facta originem suam ducere. Neque quidem mirum est cum tota humorum massa tam evidenter ab hujus morbi fermento alteretur et in statum colliquativum reducatur (quod a tumoribus malignis gonorrhœis et ulceribus hunc morbum usitato comitantibus apparet satis) si etiam hujusmodi tumores atque ulcera venerea, aliquando in spongioso et molli pulmonum parenchymate accidunt unde phthisin vere pulmonalem insequi necesse est. Ego equidem potius mirari soleo lue veneream saltem chronicam seu malè curatam unquam sine pulmonum ulcere et phthisi inde nata reperiri.

Sauvage*, partly on the authority of Morton, and in part from his own observation, admits, as the eighth species of secondary phthisis, *phthisis syphilitica*; a form of the disease which has been received likewise into the nosological systems of Cullen and Macbride.

Portal†, in the tenth section of his work, treats at length of this disease, and the late learned Dr. James Sims, in his observations on epidemic disorders, has the following remarks. The kind of this disorder (*phthisis*) most commonly met with here, as well during this as other years, was such as took its origin from a bad crisis of the blood, rather than from any inflammation of the lungs. In many cases this seemed to be derived from a previous gonorrhœa, of which the person thought himself completely cured a year or two before. I would therefore advise every physician, when called to a tabid patient whose manner of life may have thrown him in the way of such disorder, carefully to inquire whether they ever had any venereal complaint, as it is never to be disregarded in the prescription. This *phthisis*, though it did not differ materially in its symptoms from those to be described hereafter, yet required a very different treatment, mercury being the sole refuge, which never failed performing a cure when taken before the lungs had inflamed and suppurated‡.

In addition to the authorities above

* *Observationes Anatomico Pathologici et Practici Argumenti*, fasc. 1. Amst. 1826, p. 130.

† Renshaw's *London Medical and Surgical Journal*, vol. vii. p. 198.

* *Nosolog. Methodica*.

† *Observations sur la nature et sur le traitement de la phthisie pulmonaire*. 8vo. Paris, 1792.

‡ Page 117.

referred to, many of whom have, in corroboration of their general remarks, adduced instances of the disease, individual cases will be found recorded in the *Acta Medicorum Berolinensium**, by Hoffman†, Morgagni‡, Tode§, Schwartz||, Meza¶, Saucerotte**, and others. These names will, it is hoped, suffice to prove that, as far as reputable authorities and a tolerable amount of antiquity are concerned, the idea of syphilis attacking the pulmonary tissues, far from being a newly-broached and novel doctrine, is in truth to be regarded as perfectly orthodox medical belief.

By every author above referred to, Dr. Graves alone excepted, the pulmonary affection induced by syphilis has in its nature been regarded as akin to, if not identical with, phthisis; whence the name *phthisis venerea vel syphilitica*, by which it has invariably been designated. Since the use, however, of the word phthisis has been confined within narrower limits, and has been regarded as identical with pulmonary tubercle, and these as terms mutually convertible, the propriety of such a term as syphilitic phthisis has indeed become somewhat doubtful. In the present day there exists a pretty general disbelief of any peculiar power possessed by syphilis of giving rise to tubercle, and as modern researches have satisfactorily shown that scrofulous or tubercular cachexia is an invariable, and indeed necessary precursor of tubercular deposits, it is evident that tubercle, if at all, can under two circumstances only be regarded as venereal. These are, either when syphilis develops this peculiar cachectic state, and thus by its agency, and therefore indirectly, generates tubercle, or when it occurs in an individual already cachectic or tuberculated, pursues its course with, and materially modifies these conditions.

In reference to the former of these it may be objected that it is very doubtful whether syphilis has a greater tendency than any other severe disease materially disturbing the constitution, to induce a scrofulous state, and supposing that it does so, whether the resulting condition differs in any material respect, as regards nature or treatment, from the more commonly observed cases. Regarding the latter it may be urged that the accidental co-existence of syphilis and tubercle, or of its prelude, scrofulous cachexia, particularly when the last-named conditions have had a priority of being, will not justify our formally distinguishing and separating such cases from those of every-day occurrence, unless it be first shown that the cachectic state, and ensuing tuberculization, be materially altered in their course, symptoms, or essential treatment, by the co-existent contamination.

These, then, are but a scattering of the difficulties which surround us when we go to admit the existence of such a disease as syphilitic phthisis.

For the satisfactory determination of the question, we at present possess no sufficient evidence. The different subjects bearing upon it have either been wholly neglected or but theoretically discussed, and our information consequently is both vague and limited. First it would be necessary to define the meaning to be attached to the term syphilitic phthisis; and then to work out the investigation by an extended series of well-observed and carefully-collected facts. As the object of this paper is the illustration of those pulmonary diseases which result directly and immediately from syphilis, syphilitic phthisis, the latter being used as above explained, cannot fall within the scope of our remarks. The only reason, therefore, in view, in this discussion, is that all the recorded examples of syphilitic pulmonary disease—examples from which indeed I shall have in part to draw my own conclusions—are incorrectly, according to my view of the case, chronicled as phthisis.

What then, it will be asked, is the intimate nature of the morbid action set up in the lungs by a syphilitic taint of system, if it be not tuberculization? My own observations, and the careful review of recorded cases, justify

* Tom. ii. p. 92. 1718.

† Opera Omnia, tom. iii. p. 424. Obs. v. *Phthisis Venerea*.

‡ De Sedibus et Causis Morborum, epist. xxii. art. 11.

§ Societatis Medicæ Havniensis Collectanea, vol. i. 1774. Art. xxi. *Venereæ phthisicæ felix sanatio ope mercurii sublimati corrosivi*.

|| Dissertatio Observationes quasdam medicas continens. Gött. 1787. p. 13.

¶ Acta Regia Societatis Medica Hafniensi, tom. ii. p. 211.

** Journal de Médecine, par Corvisart. 1812. p. 350.

me, I think, in affirming, that inflammation is the action, and either the bronchial mucous membrane, or the pulmonary parenchyma, the immediate seat of that action: in other words, that syphilis displays itself in the lung under the varied forms of bronchitis, pneumonia, or bronchopneumonia. I am not prepared to say, observes Dr. Graves, which of the pulmonary tissues is most usually attacked by the venereal poison, but I believe that it chiefly tends to the bronchial mucous membrane, although, like other animal poisons, *e. g.* those of measles and scarlatina, it may also occasionally produce pneumonia.

The remarks of Dr. Graves having reference to the chronic form of syphilitic bronchitis are amply borne out by Dr. Stokes, who by his account of the acute form of the same disease has made a most important addition to our previous information. His remarks being extremely valuable I shall quote them at length. With respect to the bronchial system, says he, we may observe the disease as an acute or more chronic affection. In the first instance it is analogous to the bronchial irritations of the exanthemata, of which I have seen a few interesting examples, while in the second there is a chronic irritation, which, when combined with the syphilitic hectic, and with peripneumonia, closely resembles true pulmonary phthisis. In the first of these cases I have observed, that after a period from the first contamination, the duration of which has not been determined, the patient falls into a feverish state, and presents the symptoms and signs of an irritation of the bronchial membrane. These having continued for a few days, a copious eruption of a brownish red colour makes its appearance on the skin, and the internal affection either altogether subsides or becomes singularly lessened. Here we see the bronchial membrane taking on an action which is peculiar, and very different from its ordinary irritations. There is an inflammation only analogous to that of the exanthemata, and no doubt can exist that it is connected with the syphilitic poison. My friend Dr. Byrne, whose situation as medical officer of the Lock Hospital gives him the greatest opportunities of observation,

informs me that he has in many instances seen patients who had been formerly diseased, and who had come into hospital either for new sores or for gonorrhœa, attacked with intense bronchitis and fever. This attack would come on suddenly, and the distress was so great that bleeding had to be performed; the effect of which was, that soon after, a copious eruption, often combining the lichenous and squamous forms, made its appearance, with complete relief to the chest. In some of these patients on the day before the eruption the stethoscopic signs had been those of the most intense mucous irritation, and yet when the skin disease appeared the respiration became either perfectly pure, or only mixed with an occasional rhonchus in the large tubes. The same gentleman has observed the reverse of this; as, when a syphilitic eruption has been repressed, the bronchial membrane has become much engaged, and the patient affected with general febrile symptoms. These phenomena subsided after bleeding and mild diaphoretics, which had the effect of restoring the cutaneous eruption. Here we have an additional evidence in favour of the analogy between syphilitic bronchitis and that of the exanthemata.

The deservedly high position of Dr. Graves and Dr. Stokes, as authorities on all matters connected with thoracic pathology, renders my feeble testimony upon this point altogether superfluous; and their recorded statements are, it seems to me, amply sufficient indisputably to establish the existence of such a disease as syphilitic bronchitis, acute as well as chronic.

The chronic form of the complaint is in all respects the most interesting and the most important. It is the most usual form under which syphilitic bronchitis presents itself; and, when combined with some other morbid conditions often consequent upon the existence of the syphilitic poison in the system, is exceedingly likely to be mistaken for true phthisis, and thus to lead to an unnecessarily bad prognosis, and to be both incorrectly and inefficiently treated.

[To be continued.]

SOME PHYSIOLOGICAL REFLECTIONS
ON
THE NATURE AND TREATMENT OF
ANGINA PECTORIS,
AND OF ANALOGOUS STATES.

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[For the Medical Gazette.]

Introduction. Physiology of violent circulation—lymphatic system. Influence of repletions. Physical changes in the heart which attend advancing years—the like in the valves, surfaces of contact, and perforations. Slight disorders approaching to angina

DISEASES, like the natural functions, may be physical, nervous, or humoral, or they may involve all these circumstances. It is our object here to illustrate the physical part of the general doctrine of cardiac affections still more than to show how these doctrines are to be applied in a particular set of cases. Without, therefore, aiming at too great precision, and without supposing that, here and there, we may not be altogether wrong, we desire to offer these opinions as in the main available, and even essential, to the explanation of most thoracic diseases. We venture to conclude that there are no satisfactory explanations of the anginal affections amongst authors. Perhaps the most complete history of angina is to be found in the paper of Dr. Forbes, in the Cyclopædia of Practical Medicine. Dr. Forbes admits the basis of our opinions, and he quotes some writers who seem still more to favour them; but those who may refer to the article in question will hardly feel that the subject does not require a more distinct exposition. It is hoped the following views may ultimately lead to this.

We shall regard the anginal attacks as of varied character, and endeavour to show on what the different features depend; premising only that there are probably many more facts than those we mention which are to be explained in a similar manner; and, again, others which need a different train of illustration.

Our first object will be to consider the physiological state of the circulation at the ordinary onset of the first attack.

Walking, or any continued exercise, steadily accelerates the circulation. The muscles compress, distort, and impede the arteries, and empty the veins, generally facilitating the capillary circulation. The right side of the heart contracts on a larger amount of blood. The expansions of the chest in respiration, and especially the inferior lobes of the lungs, are extended to a much greater degree than is commonly thought, under slowly but steadily increasing efforts; while it also becomes more frequent. By the same gradual changes the left side of the heart is adapted to a fuller circulation: it is dilated more freely, and contracts more powerfully. Doubtless, too, the aorta attains an acmé of expansion under the increased impulses. All men are familiar enough with the main fact here expressed, that the fullest freedom of the chest is only well attained by slowly increasing exertion. It is what trainers call second wind; and cannot be suddenly attained, nor can it very rapidly cease. Its perfection is scarcely to be looked for beyond the freshness of manhood; and, without doubt, we ought to expect that this exaggerated physical state of the circulation is in danger of surpassing the bounds of health. We suppose, moreover, that the manifestations should be both sudden and severe, in the event of any part failing in its office.

Other physical circumstances are more or less directly or frequently concerned with the above. The influence of pressure on the lymphatics is to empty them also rapidly towards the heart; but this consideration applies most particularly to the lacteals, supposing them to be engaged, as after a meal; although it is doubtless true, that the more active distribution of blood over the whole body, when in exercise, is a means of retarding the assimilative functions of the bowels.

In addition, a full abdomen adds to the accumulation of blood in the chest, and also obstructs the lungs, and in a measure impedes the filling of the left heart. We shall again find occasion to introduce the occasional sources of mechanical difficulties in the circulation.

There seems to be another necessary consideration in relation to the healthy condition of the heart in full play. We refer to the orifices and their valves. In the fourth volume of the

Guy's Hospital Reports, the valves were shewn to possess more or less of surplus curtain—some membrane to spare in the ordinary easy action of the organ—constituting pretty defined and extensive surfaces of contact: and with respect to these parts in the healthy state, it is necessary to remark that certain exertions are required to bring them into operation as parts of their proper valves; and that their occasional use is essential to their due nourishment and strength. It is by the healthy yielding of their several orifices that the valves are enabled to exercise the full breadth of their curtains, and without this exercise they fall into diseased and inefficient states, of which we shall speak in their proper place. The reader will not be at a loss to conclude that we propose to date the commencement of common angina from the mere physical state belonging to what is called second wind.

In the next place, however, we have necessarily to reflect upon the peculiar changes which the heart undergoes as the period of life approaches at which anginal attacks may arise. M. Bizot has proved numerically that in health the capacity and thickness of the left ventricle increases steadily from manhood upwards; and also that the general breadth of the heart bears a pretty definite relation to that of the shoulders or chest; and we suppose we may conclude that, as age advances, the elevation of the shoulders and increased expanse of chest, by facilitating the access of blood to the left ventricle, are not without their influence on the dilatation of that cavity, while the growing difficulties in the arterial circulation call for the augmenting force and nutrition of the ventricular parieties. The feebler and slower circulation may still require other considerations to account for them completely. We may remark, too, of the rise of the shoulders and corresponding changes, that this is not all the result of senile emphysema, and it is by no means necessarily an impediment to the transmission of blood from the right to the left heart. The dilatation of the heart, blood-vessels, and capillaries, as well as of air-cells, may bear a pretty exact proportion to the expansion of the chest (the inspiratory muscles having but few and weak antagonists), and to many other correlative changes.

It is not at all apparent that hypertrophy, with an increased cavity, is indicative of augmented power in the ventricle as a whole: the two circumstances, simply, are mainly indications of delay and accumulation; yet in one hour such a ventricle may act too freely on a moderate quantity, or be burthened with an excessive fulness. Now the great orifices of the heart evince a similar tendency to expand with age; and under the most favourable circumstances the valve-curtains seem to extend themselves almost commensurately: but this is too uncommon: for being so delicate and so constantly and unequally exercised, they are, of all the parts of the heart, most prone to fall into disease, and the commonest changes are those of thickening and contraction. Two more changes we shall also speak of in the valves, which are truly morbid, but which, like the last, often seem of small importance in the easy states of the circulation. The surfaces of contact, when perhaps a too uniform state of the circulation may have been long preserved, are liable to two kinds of disorder, which are to be found separately, or commingled in many degrees. Portions of the curtains, through disuse, become attenuated and perforated, or altogether lost; and, again, the parts employed are liable, where most drawn upon, or when most intimately pressed or rubbed together, to become thickened, and even contracted.

At present we have no occasion to dwell more at length on these often trifling alterations; but, with respect to their occasional importance, we would remark, that their influence is most considerable where the truest and strongest propulsion is demanded; namely, in the left side of the heart; and here, too, their features are most strongly marked and fully developed; and, most of all, in the aorta.

Perforations near the free edges of the aortic valves are very common after thirty years of age, and they are very easily overlooked. Before they can be brought into mischievous influence (having been induced by a more or less easy life and circulation), the valves must be unusually extended by a combination of repletion and exertion, and probably, in the common case, by hypertrophy of the left ventricle also. Gradual contraction of a curtain must

operate in a similar manner, leaving it liable to become inefficient.

It is not necessary for us to describe all the states of the circulation which concur with different anginal affections. These can only be treated of in connection with their specific symptoms. The strong heart, the atrophied organ, and that which is materially altered in its relative proportions, offer very distinct cases. We know how variously the heart may fail with advancing life, in its substance, its cavities, its orifices, or its valves; and even in its dependence on other organs.

Many changes may thus affect the heart along with the muscular system generally; and it is ever to be remembered, that, in order to approach a diagnosis of the state of the heart, as to its mass, texture, &c. it will be very available to consider the age, sex, and constitution of the patient, in reference to the muscular system in particular, as well as generally.

The sudden affections of wasted or enfeebled hearts constitute a distinct set of anginal disorders; but in every kind of case we must contrast together or study the relations between the distension and resistance, the size of the cavity, and its power of contracting, and also between the extent of the orifices and the states of their valves, before we can estimate the share which any part takes in the production of symptoms or of death.

It is not to be forgotten, as one useful part of the introduction to the study of angina, that there are some pretty sudden and severe affections which may be induced in younger and more healthy subjects by causes like those of angina. These may be regarded as states (shall we say?) short of angina. Palpitation, or an excessively rapid pulse, or even an irregular pulse, are common effects of exertion. In undue or severe degrees these must be referred to weakness and dilatation of the heart, or a part of it, as we shall see hereafter. The small frequent pulse may well be accounted for by an over-full left ventricle, weak, overburthened, and having lost all the easy relation which existed between its size and form, and the aperture of egress.

The small and less rapid pulse belongs to the defective supply to the left ventricle from any cause. In either of the foregoing instances the pulse

may also be irregular. We attribute the intermitting pulse of the more healthy to a certain disproportionate hypertrophy of the left ventricle.

The side-ache of exertion, repletion, or agitation, may illustrate the states under consideration. We conceive they belong to congestions, as in the spleen or liver. When brought on by running, the pain is singularly under control by rest or pressure, or even perhaps full breathing.

Some dyspnœa from exercise, under many circumstances, may be considered natural, but its unhealthy degrees are frequent enough. An additional quantity of blood passing on to the lungs produces the increased *besoin de respirer*, whilst undue fulness of these (and still more at times of the bronchial linings) renders the inspiratory effort laborious. Delay in the left heart, or excessive propulsion from the right, may aggravate the mischief. Bronchitis and asthma tend to diminish the supply of blood to the left heart, and render its actions almost too easy; and so also may a very simple disposition to dyspnœa in exercise, which also, by inclining to rest, leaves the left heart too little engaged and nourished. On the other hand, in some thoracic diseases, gentle exercise, in the intervals of repletions, accelerates the general circulation and secretions, and so relieves the lungs and stimulates the left heart, which, but for this, would become an additional cause of delay.

We have not adverted to the instances of casual syncope from exertion, yet it must appear that these will serve to illustrate the less mixed cases of fatal fainting, and the first are not to be omitted in a review of the slight and transitory states, by the contemplation of which the gravest conditions may be the more easily explained. The faintings of delicate persons from slight causes, or of those which are stronger from somewhat greater disturbances, seem all to depend on this—that the right heart receives too little blood. The veins are not full enough, and the general relaxation of muscles (whether from defective blood or nervous supply) leaves the veins to yield to the effects of gravitation.

In this place it should also be remarked, that perspiration and the general freedom of secretions, as well as of capillary circulation, and perhaps even the ful-

ness of superficial* and other veins when greatly excited, indicate the nature as well as the need of relief, and in a manner are near approximations to disease or distress.

[To be continued.]

ON THE OPERATION FOR TALIPES.

To the Editor of the Medical Gazette.

SIR,

IN the communication you did me the favour to insert in your number of the 11th of December last, I promised you a report of some cases, illustrative of a new operation for the cure of a certain variety of talipes, for which the Stromeyerian operation was inapplicable. I shall now endeavour to fulfil my promise, although my avocations compel me to do so very briefly. Besides its reference to those cases of talipes, it is moreover interesting as calculated to meliorate many other infirmities arising from total paralysis, or diminished power, of certain classes of muscles.

In the month of June 1840, I had my attention directed to this subject by observing the peculiar gait of a gentleman who was passing along the street. Although I neither knew nor spoke to the individual, I was struck with the manner in which he walked, and took more notice of it from the circumstance of being at the time much engaged in the practice of the Stromeyerian operation for the cure of club-foot. That operation consists essentially in the division of the tendons of muscles, which act too energetically, and extending them, before the lymph which is thrown out to cement the divided extremities together has become consolidated: so that, in the end, there is a new portion actually implanted between the divided extremities of the originally contracted tendon. That this is not a speculative idea I can prove by specimens in my possession. I observed that in the variety of the malformation referred to—evidently caused by paralysis—there was no morbid tension; but, on the contrary, a morbid relaxation. The idea occurred to me, that if a portion of the tendon were cut out, and the divided extremities made to reunite, by being kept in contact during

the healing process, it would at least improve the flapping condition of the weak member, and by retaining it in a state of greater tension, might produce a tendency to contraction in the paralysed muscles. On my return home I explained my views to my intelligent friend and patient, Mr. Rhind, Surgeon, Edinburgh, who was then under my care for the cure of congenital talipes varus of the worst degree in both feet. I told him that the propriety of the operation was so strongly impressed on my mind that I was determined to adopt it the first time a case of the kind referred to came under my care. He expressed himself much pleased, and considered it a fortunate idea. It was not long ere I had an opportunity of testing the value of my proposed new mode of practice in the case of Miss —, which I shall now detail.

CASE I.—Miss —, æt. six years, has been deprived of the use of the left leg by a paralytic stroke. She has undergone a variety of treatment under some of the first medical gentlemen of this town; but, for the last two years, has been given up as a hopeless case. Her present condition is this: the left leg is perfectly powerless, dangling by the side of her crutch, without reaching the ground, and is much colder than is natural; the foot assuming the appearance of a slight degree of varus, so that it would, if brought to the ground, rest on its outer edge, the toes inclined a little inwards, and the heel slightly elevated. On the 30th of June, 1840, assisted by Mr. Rhind, my son, and one of my apprentices, Mr. Pacey, I made a longitudinal incision along the course of the peronæus tertius, which I elevated, and excised a portion of it, to the extent of three-sixteenths of an inch. I then closed the wound with plaster, and applied splints and bandages, so as to approximate the divided ends, and maintain them in contact.

2nd day.—Has been free from complaint.

4th.—Removed the plaster, and, after bathing the foot, again applied plaster and bandaging as before. Care has been taken, during the dressing, not to extend the foot so as to separate the recently approximated extremities of the divided tendon.

6th.—All going on well; and now feels the foot and leg quite warm.

* The blood-horse illustrates this fact.

7th.—Has entire command of the leg, and so much strength in the foot, that I requested her to take hold of my hand, and try if she could use her limb, when, to the surprise of all present, she was able to walk across the floor of my surgery.

10th.—In the presence of my talented friend, Dr. J. L. Bardsley, and others, she actually walked across the room and back again without any assistance whatever,—a fact which equally astonished and delighted him and all present. In twenty days she was enabled to walk about in a laced-up boot; and in a week more to throw her crutches aside entirely. She has continued well and strong ever since. Besides the operation nothing else was done in this case, excepting the use of a stimulating liniment, which she had employed before without the least benefit.

CASE II.—Miss —, æt. 10, has been suffering from an attack of valgus of the right foot for the last six years; the malleolus internus nearly resting upon the ground when supporting the weight of the body on this leg; not from contraction of any muscles, but from a paralytic state of the tibialis posticus and flexor longus pollicis pedis of the right foot. With the same assistance as in the former case, I excised nearly a quarter of an inch of the tendons of the tibialis posticus and of the flexor longus pollicis pedis, and dressed and bandaged as before, so as to keep the divided extremities in contact. All went on perfectly well, and from the day the operation was performed, the patient said the foot felt warm, although previously the foot and leg were constantly cold. In three weeks she could walk quite straight; the foot and ankle being maintained in their proper relative position.

CASE III.—The next case was Mrs. —, æt. fifty, who had suffered from varus of the right foot from infancy, arising chiefly from a paralytic state of the peronæus longus and brevis. On the 6th of July, 1840, I excised three-eighths of an inch of these tendons, above the malleol. extern., closed the wound with plaster, and bandaged it so as to keep the ends of the divided tendon as much approximated as possible.

2nd day.—Quite free from constitutional irritation, and all complaints.

3d.—Makes no complaints; says she feels the foot much warmer, and can

now move the toes for the first time in her life. Every thing went on in the most satisfactory manner in this case; and in twenty-one days she was enabled to put on a laced-up boot, provided with a steel support, which enabled her to walk about with much more comfort than formerly. From want of attention the improvement did not continue so manifest as at first. The experience I have now had leads me to believe that this may be partly attributed to the circumstance of the tendo-chillis being slightly contracted. Had I such a case to operate upon now, I would divide and lengthen the tendo-achillis, whilst I would take a portion from, and thus shorten, the other tendons referred to.

CASE IV.—Master Gresty, æt. 6½ years, has had talipes valgus since one year old, arising from absolute paralysis of gastrocnemii and other muscles on the back of the leg; so that there is not only total inability of extending the foot, but such relation of all the extensor muscles, as to allow the foot to be pressed upwards until it touches the front of the leg. On the 4th of Sept. I excised three-eighths of an inch of the tendo-achillis.

3d day.—All going on well.

5th.—Makes no complaint; wound nearly closed, and very little matter discharged.

7th.—All going on well, and still great warmth of foot and leg, which has increased since the operation, although there is no pain or inflammation to account for it.

10th.—Wound nearly closed; tendon so firmly united as to resist any attempt at pressing the foot upwards, excepting a little beyond a right angle with the leg; and has now power of flexing and extending the foot freely; showing that the paralytic state of the extensor muscles is completely overcome. At three weeks after the operation this patient fell and injured the leg, so as to cause inflammation and suppuration around the newly formed cicatrix, which retarded his recovery. After this had subsided, with the use of a stimulating embrocation, this patient regained the use of his extremity, so that he could in three months after the operation walk with ease; flexing and extending the foot freely, and capable of supporting the foot and leg in its proper relative position. Before the operation any attempt at walking was attended with

great awkwardness; the leg being tossed forward, the toes turned very much outwards, and the inner ankle approximating the ground.

CASE V.—Master B——, æt. 12½ years, from a paralytic seizure or accident, they cannot tell which, has been much in the same state as the last patient, for about a year. In October 1840, I excised five-eighths of an inch of the tendo-achillis, dressed and bandaged as usual.

3d day.—All going on satisfactorily.

4th.—Makes no complaint; wound nearly closed, and very little matter effused; dressed and bandaged as at first.

6th.—Wound almost entirely closed, and has now the power of flexing and extending the foot freely, indicating the reunion of the tendo-achillis as well as restoration of retractile power. Foot and leg still kept bandaged in the same manner as at first.

8th.—All going on most satisfactorily.

In two months put on a laced-up boot, with which he walked freely; has continued without complaint, and can now walk nearly as well as if he had never been so afflicted.

CASE VI.—Master Moss, æt. 10 years, has right leg quite useless from a paralytic seizure in infancy. The leg not only powerless, but also cold and withered, being about half the size of the other one. The knee bent to an angle of 45°; the foot turned outwards, and can be easily pressed up against the tibia, showing the extreme want of tone of the gastrocnemius and tibialis posticus. I excised three-eighths of an inch of tendo-achillis, and about as much of tibialis posticus; dressed and bandaged as usual. All has gone on well; and he is provided with a high-soled boot, to compensate for the shortness of his leg. He can now walk without a crutch, merely requiring the assistance of a stick. This leg is much grown, and greatly warmer than before the operation; indeed, he is altogether stronger.

CASE VII.—Miss ——, æt. 10 years, a case of calcaneo-valgus of left leg of twelve years standing, arising from total paralysis of gastrocnemius, with contraction of tibialis anticus, extensor longus pollicis pedis, extensor longus digitorum pedis.

On the 25th of Sept., 1840, I operated, by excising a full one-half of an inch

of the tendons of the tendo-achillis, and dividing the tibialis anticus, extensor longus pollicis, extensor communis digitorum pedis. Applied plaster to the wounds, and splints and bandages, so as to maintain the extremities of tendo-achillis in apposition, and those of the other parts.

2d day.—All going on well; makes no particular complaint, and free from every symptom of constitutional irritation.

3d.—Still going on well.

4th.—Removed the dressing; wounds in the anterior part of the leg closed; that in the posterior looks well, is nearly closed, and very little pus discharged from it. Dressed, and applied splints and bandages as before.

6th.—All going on well, with the power of flexing and extending the foot; showing the reunion and restored muscular contractility of the tendo-achillis and gastrocnemius muscles, as well as the antagonist tendons on the anterior part of the leg. From this time all went on so well as to require no particular remark; and in six weeks she wore a laced-up boot, walked with comparative ease, and is now comparatively well and strong. As in the other cases, the leg, which was cold and powerless, acquired an increased degree of heat as well as strength. To aid this, and strengthen the limb more rapidly, I recommended friction with stimulating liniment, as in the other cases.

CASE VIII.—Wm. Bowker, Sheffield, æt. 7 years, has had talipes valgo-calca-neus from infancy, supposed to have arisen from a paralytic stroke when six months old. Has no power of gastrocnemii, so that the foot can be pressed upwards until it almost reaches the tibia. When walking, the foot is turned very much outwards, and the malleolus internus nearly touches the ground. On the 26th of February, 1841, I operated by excising 3-16ths of an inch of tendo-achillis, and dressed and bandaged in the usual manner for such cases.

2d day.—All going on well.

4th.—Makes no complaint; removed the bandages, &c. and reapplied them as before. There was the smallest possible quantity of pus under the plaster.

6th.—Can now flex and extend the ankle-joint and toes. From this time

continued to progress in the most satisfactory manner, and in eighteen days could walk easily without any apparatus applied to the foot or leg, having completely regained the power of the gastrocnemius and tibialis posticus and flexor longus digitorum pedis.

I could easily multiply cases of a similar nature, but shall only add one more, where I excised about half an inch of the tendons of the biceps flexor cruris, the semimembranosus, the semitendinosus, and the gracilis, in a case of such extreme weakness of these muscles, that the tibia and fibula could be so much flexed upwards on the femur as to form a very considerable angle at the knee. Although both legs were similarly affected, I only operated on one, the left, at first, being much the worst. After the operation, having the wounds closed with plaster, and the legs flexed at right angles with the thigh, splints and bandages were then applied to retain it in that position, so as to allow the divided extremities to reunite with the least possible intervention of new tendinous substance between them. Every thing went on to my entire satisfaction; and in ten days such firm reunion of tendons had taken place as to offer considerable resistance to extending the leg completely. In two days more I did not hesitate to let the patient stand on his legs; when it was most interesting to remark the improvement which had resulted from the operation, this leg being now perfectly straight, the other still falling back in the old form. The day fixed for the operation on the second leg the boy was attacked with scarlatina, which, of course, caused it to be postponed. However, I intend shortly to perform the operation, and have no doubt but the results will be equally satisfactory as those which attended operation on the first leg. I have also operated by incising a portion of tendon of the tibialis anticus, extensor longus pollicis pedis, and of extensor longus digitorum pedis, in cases of paralysis of the muscles on the anterior part of the leg, with similar results.

I believe this operation is entirely new; and I trust the cases now detailed are sufficient to prove that it is most efficient, and a highly important addition to the resources of our art in an interesting and hitherto incurable class of diseases. It of course requires

great care and consideration in the execution of it, to excise a sufficient portion, and no more; but I hesitate not to say that, when properly performed, it will succeed in every case of the sort I have referred to. I do not profess to have been quite so correct as I could have wished in every case of the kind I operated on from the first. Experience is required in every operation, and particularly so in such as this; because, if too little be excised, it will not excite that activity of contractile power in the muscular fibre necessary for the correct and energetic discharge of its functions; whilst, on the other hand, if this is excited to too great a degree, from too much being excised, the result will be the innervation of the other muscles of the limb operated upon. In a future communication I shall explain my views on this subject, illustrated by the detail of cases in which I have been enabled to restore increased heat, sensibility, and power, to paralysed parts, so that patients who have been powerless of limbs for many years have had the use restored to them, as can be certified by professional friends who have seen the patients and operations. This is accomplished by the converse mode to that now detailed, viz. by the division and ultimate elongation of muscles which had been held in a state of permanent contraction, and thus robbed the limb of that diffusion of nervous energy to the other muscles which is required to ensure and maintain their normal functions.—I am, sir,

Your obedient servant,

JAMES BRAID,
M.R.C.S. Edinburgh.

10, Piccadilly, Manchester,
April 8th, 1841.

ON THE FORMATION OF ARTIFICIAL ANUS IN ADULTS,

FOR THE RELIEF OF RETENTION OF THE
FÆCES.

To the Editor of the Medical Gazette.

SIR,

THE subject of the formation of artificial anus in adults, for the relief of retention of the fæces, having received but little attention in this country, I

beg to forward you some remarks upon it, which I shall feel obliged by your inserting in your very valuable journal, if you think them of sufficient practical importance to merit the attention of your readers. They are for the most part taken from a memoir read by M. Amussat at the Académie de Médecine of Paris, in 1839, with a copy of which he favoured me.—I am, sir,

Your obedient servant,

JOHN ERICHSEN.

24, Welbeck Street, March 24, 1841.

Various operations have been projected, by opening the large intestine in some part of its course, for the relief of retention of the fæces, consequent upon obstructions of the rectum, or an imperforate state of the anus. Littre*, as early as the year 1710, proposed in these cases to open the sigmoid flexure of the colon from the left iliac region; but it was not until the year 1776 that any operation of the kind was performed, when Pillore†, a surgeon of Rouen, was the first to make an artificial anus on the adult, for the relief of retention of the fæces; this he did, not according to Littre's method, but by opening the cæcum from the right iliac region. Fine‡, of Geneva, in 1797, opened the transverse colon from the umbilical region, in a case of retention of the fæces produced by scirrhus of the upper part of the rectum.

Although these operations serve to fulfil the indication of relieving the retention of the fæces, they are all defective in one most important respect; for, as the peritoneum must, in all of them, of necessity be wounded, an intense and frequently fatal peritonitis is the inevitable consequence. It was to avoid this serious complication that Callisen§, in 1796, proposed opening the colon from behind, in the left lumbar region, where it is not covered by peritoneum. He once attempted this operation on the dead body of a child, but failing in his endeavour to reach the intestine without wounding the peritoneum, he seems to have relinquished all further idea of it; and it has subsequently been rejected by all those writers on surgery who have

treated of this subject. Sabatier* gives the preference to Littre's operation: Duret†, Martin le Jeune‡, Dupuytren§, and Velpeau||, all reject it on the ground of impracticability; and, like Sabatier, prefer that of Littre. But the operation was not understood: the surgical anatomy of the parts implicated was not sufficiently attended to; and it was not known that the distended state of the intestine lessened the difficulties, and facilitated the performance of the operation.

Amussat, at the time that he was attending the celebrated Broussais for that scirrhus affection of the rectum of which he ultimately died, was led to reflect on the resources that surgery offers in similar cases; and after making some experiments on the dead body, with the view of contrasting the merit of the different operations that have been proposed for the formation of artificial anus in cases of obstruction of the large intestines, he became convinced that the operation proposed by Callisen, if somewhat modified, was not only practicable, but safe. He soon had an opportunity of putting this opinion to the test of experiment, and has since performed this operation twice with complete success.

Before describing the operation itself, it may be as well to give a very brief account of the surgical anatomy of the parts concerned, namely, the left lumbar region and the corresponding portion of the colon, more especially as they have hitherto been little studied in relation to surgery. The lumbar region may be said to consist of two distinct parts, namely, the abdominal wall itself, and the colon, with a portion of the peritoneum which fixes it to that wall. As the different structures that enter into the formation of the abdominal wall in this situation are fully described in all works on anatomy, they need no further attention on our part; but it is with the anatomy of the colon, and with its relations to the peritoneum, which are of the greatest importance in reference to the operation proposed by Amussat, that we have to do.

* L'Histoire de l'Académie des Sciences, 1710.

† Mémoires de la Société de Médecine de Lyons, 1798.

‡ Annales de la Société de Médecine de Montpellier, tom. 6.

§ Systema Chirurgiæ hodiernæ. Hafniæ, 1800.

* Médecine Opératoire, 2d edit. tom. 3. p. 337.

† Recueil périodique de la Société de Médecine, tom. 4.

‡ Recueil des Actes de la Société de Médecine de Lyons.

§ Dictionnaire de Médecine, tom. 3, p. 117, Anus Artificiel.

|| Médecine Opératoire, tom. 4, p. 751.

After having detached, layer by layer, all the tissues that cover the colon in this region, it will be seen that the peritoneal investment of this intestine is wanting in its posterior third, which, in this respect, resembles the anterior surface of the bladder. This disposition of the serous membrane gives a peculiar character to the colon, and causes it to differ completely from the other intestines; enabling the surgeon to open it without penetrating into the peritoneal sac. Where the serous membrane is wanting, the colon is covered by several layers of dense and strong cellular tissue, which alone separate it from the quadratus lumborum.

Amussat states that out of the great number of bodies that he has examined with this view, he has never seen a mesocolon in the *left lumbar region*; but, on the contrary, the intestine is always closely attached and adherent to the abdominal parietes by dense cellular tissue. In order to judge whether the colon in the left lumbar region be provided with a mesocolon or not, this intestine must be examined from behind, without opening the cavity of the abdomen; for, if it be examined from before, in the usual way, a mesocolon may inadvertently be formed by drawing the intestine forwards, and thus separating the peritoneum from its cellular connections. If the colon be now distended with air or water, care being taken that the fluid does not pass through the ilio-cæcal valve, it will be seen that the extent of this cellular interval will increase in proportion to the distension of the intestine: the peritoneum being pushed back more rapidly and more distinctly than it is even from the anterior surface of the bladder, when that viscus is much distended. In order to judge better of the extent of the cellular interval which exists at the posterior part of the lumbar colon, this intestine should be detached from the body, and then that part of its surface which is not covered by peritoneum may be compared with that which is invested by this membrane. At the same time it may be useful to note the colour of the colon, which differs from that of the small intestines by being of a more greenish tint.

It has already been stated that Cal-lisen was the first who proposed opening the large intestine without wounding the peritoneum. His operation con-

sisted in making a vertical incision from the last false rib to the crista of the ilium, taking as his guide the external border of the quadratus lumborum muscle. He only attempted this operation once, and then failed in his object of not wounding the peritoneum. In consequence of this failure he appears to have been discouraged, and to have given up all further idea of the operation.

The following is a description of the modified operation that Amussat proposes, and which he has already performed twice with success:—A transverse incision is to be made two fingers' breadth above and parallel to the crista ilii of the left side; or rather in the middle of that space which is bounded by the false ribs above, and by the crista ilii below: the incision should commence at the external margin of the erector spinæ, and extend outwards for about four inches. The spinous processes of the lumbar vertebræ, the crista of the ilium, and the last false rib, are the principal guides. The superior margin of the crista ilii is, however, the safest of these, and the transverse incision may be said to correspond to the middle third of this part of the ilium. After having divided the skin and all the more superficial tissues, the deeper layers are next to be incised as they present themselves; if necessary the external border of the quadratus lumborum may also be cut across. The dissection is then very carefully to be carried through the layers of cellular tissue which lie immediately upon the intestine, and the colon sought for: this will in general readily present itself, and may at once be recognized by its colour and distended appearance. The operation may then be completed by passing a tenaculum, or needle armed with a strong waxed thread, into the most projecting part of the intestine, and by this means drawing it to the surface of the wound, in order to prevent it shrinking or sinking back when opened: it is now to be punctured with a large trochar or bistoury; and its contents having been evacuated, the sides of the opening in the intestine are to be fixed to those of the incision in the skin by four or five points of suture, so as to prevent the contents of the bowel being effused into the cellular tissue of the wound. It is of importance to draw the colon well forwards before opening it, in order to prevent its

contents from being effused into the loose cellular tissue of the wound, where they may set up considerable irritation, and retard the union of the parts. If the patient be very fat, the operation will be much facilitated by dividing the deeper-seated tissues in a crucial manner, so as to give the operator more space. When the operation is practised on the dead body, it will be found on dissection that the following are the parts cut through:—After the skin and cellular tissue, the latissimus dorsi will be seen divided towards the posterior third of the incision, and the obliquus externus in the anterior two-thirds of it; the obliquus internus and the transversalis, sometimes the quadratus lumborum, the cellulo-adipose tissue which immediately covers the intestine, and finally the colon itself. Very few vessels or nerves are wounded, as they, for the most part, run parallel to the line of incision; whereas, if the vertical incision of Callisen were adopted, they would necessarily be cut across.

When we compare the different operations that have been proposed for the formation of an artificial anus, it will be found that Callisen's, as modified by Amussat, is the one to which the preference must be given, for *by it alone* the peritoneum is not wounded, and thus the inflammation of that membrane, which is a necessary and so often a fatal consequence of the other operations, is avoided. Besides this advantage, which is of the very greatest importance, Amussat's operation presents several other claims to our notice: as only one side of the colon can be drawn forwards, and not a knuckle of it, as would be the case if the small intestines were operated upon, it is evident that the spur-like process, which has been so accurately described by Dupuytren, must exist to a very small extent; and consequently, if the artificial anus should ever become useless, the natural passage for the fæces having been re-established, it could readily be closed up. If, however, the peritoneum should be accidentally wounded in the attempt to reach the colon from behind, even then this operation would be preferable to that of Littré; for the cavity of the abdomen having been opened at its most depending part (in the recumbent position), the fæcal matters would have much less tendency to be effused into it, than if it were opened in front.

In respect of not wounding the peritoneum, this operation closely resembles that of puncturing the bladder above the pubes, and below the reflection of the peritoneum. There is, however, one important difference between the two operations, which may influence the result; for in the one case, as the patient lies upon his back, the urine has a tendency to stagnate in the wound, being obliged, in order to escape, to mount against its own gravity; whilst, in the other case, the fæcal matters find a ready exit from a wound situated in a depending part.

The question as to the formation of an artificial anus in those cases in which the large intestine is completely obstructed at its inferior part has never been properly discussed, although it is a subject well deserving the attention of surgeons. Most systematic treatises on surgery are silent on this point, and in the journals there are only six cases published in which any operation having for its object the relief of obstructed intestines has been performed on adults. Amussat refers to two preparations in the Musée Dupuytren, which deserve examination as illustrative of this subject. One represents tumors in the form of vegetations arising from the inner surface of the rectum; the other, taken from the celebrated tragic actor, Talma, exhibits a simple and complete stricture of the rectum. These two preparations prove that it would have been possible to have established an artificial anus in these cases, and to have hoped for complete success; more especially in the case of Talma, which was one of simple stricture without any scirrhus disease. Amussat remarks that these cases of simple stricture of the upper part of the rectum, and of the sigmoid flexure of the colon, occasioning death by producing a complete obstruction to the passage of the fæces through the intestines, are by no means rare.

With regard to imperforate anus, or rather the absence of more or less of the rectum in infants, Amussat does not think that, in these cases, an artificial anus should be established in the abdominal region, except when the intestine cannot be reached from the anal or coccygeal regions; and then he advises that the colon should be opened from behind in the left lumbar region.

[To be continued.]

ON HIP DISEASE AND LUMBAR ABSCESS.

BY WM. OLIVER CHALK, Esq.

Resident Surgeon to the Royal Sea-Bathing Infirmary, Margate.

[Concluded from p. 151.]

WHEN the abscess is allowed to form an outlet of itself, I have invariably found that the openings are smaller, fewer in number, more readily healed, and less liable to slough, even should the disease terminate fatally. When the accumulation is large, and the sac has formed in the thigh, some surgeons have recommended the emptying of it, and the compressing of its walls by means of a circular bandage applied tightly around the limb, in order to produce a union, and, with a view of more effectually promoting this process, have used various stimulating injections. This practice, however, is highly objectionable, and generally mischievous to the patient. When the matter finds an exit at the lower part of the thigh, gentle compression by a bandage is of service; but if it be carried too far great irritation ensues; the discharge increases, and fever is excited. The reason of this will be obvious, if we take into consideration the low degree of vitality which characterizes parts affected with strumous disease: in the treatment of scrofulous ulcers the greatest nicety is often requisite in selecting an application that is sufficiently stimulating, without being too much so, and in adjusting the dressings in such a way that over-pressure may not be exerted, and yet adequate support may be afforded to the edges of the wound; for if these precautions be disregarded, the granulations are soon destroyed. Thus, by bringing the parietes of the abscess firmly in contact, we prevent the reparative process, especially if an early opening be effected. Before I became fully aware of this circumstance I had often the mortification of finding my efforts in healing it, under such a plan of treatment, unavailing. There is another objection which applies forcibly to this method—it is, the irritation and suffering produced by the accumulation of pus which necessarily takes place above the bandage all the while the psoas muscle remains affected. At the first opening of the abscess patientt

should be confined to their beds for some days, and every care taken to arrest any febrile movement that may arise. If no untoward symptoms ensue after this time has elapsed, they may be allowed to move about on crutches, as in the earlier stage. The erect position (and consequent dependent state of the limb) is highly favourable to the obliteration of the sac, particularly in the psoas muscle. If patients have been long confined to their bed during the formation and after the opening of the abscess, with the thigh flexed on the pelvis, whenever they quit it the altered position and weight of the limb in the erect posture cause parts once united again to separate, and give rise to considerable suffering and a lingering recovery. Of late years I have had frequent opportunities of comparing the state of individuals who have been confined to their beds during the disease, and those who have been treated on the opposite plan; and I have no hesitation in asserting that the advantages derived from the latter are infinitely greater, both as regards the shorter duration of the complaint and the degree of deformity produced. When we consider the number of years that frequently elapse under the ordinary treatment before the abscess heals, any method that may accelerate the desired effect is invaluable. A period of four or five years very commonly passes over before the healing process is completed. There was an instance last year at the Infirmary, in the case of Wm. Norris, where the discharge from an orifice in the groin had been going on for fourteen years, and continues up to this time.

The sloughing of the orifices of the sinuses in the latter stages of this disorder is of very common occurrence, and it is sometimes followed by hæmorrhage, especially if the case proceeds badly. The walls of the sac become irritable, and discharge profusely; if there has been no pain in the hip and knee since the first stage, it returns with redoubled violence; should counter-irritants have been employed, their evil influence now becomes apparent (by the additional suffering produced in the way already described), hectic fever ensues, and the patient sinks. The treatment of sloughing sores differs in no respect from that already spoken of

in morbus coxæ; the nitric acid lotions, carrot poultices, &c. forming the best local applications. When hæmorrhage occurs in these and other strumous affections, and under very unfavourable circumstances, I have found the Sp. Terebinthinæ the best styptic. In the case of J. S. Beech, æt. 21, admitted May 23d, 1836, and who was discharged Oct. 10, where the abscess after breaking in the groin formed a large tumor in the lumbar region, which was followed by an opening immediately over the sacro-iliac synchondrosis, causing a large sloughing wound, and from which a profuse hæmorrhage proceeded, I succeeded by means of saturating compresses of lint in this spirit, and by applying firm pressure at the same time, in arresting its progress, so that he was enabled to return home, although he died some weeks afterwards. The ulceration that ensues in the latter stage, particularly in cases of long standing, where the vital energies are frequently much impaired, is often very troublesome, and is frequently accompanied by great loss of substance. The local treatment must be conducted with care and attention (in the manner already described when speaking of superficial ulceration in morbus coxæ), and the general symptoms properly attended to. The patient's diet should be carefully regulated at this time. Errors in this respect are exceedingly injurious; and, indeed, throughout the whole course of the disease, I have frequently seen the most promising cases become the most disastrous, in consequence of improper food*.

The case of Edward Brooks, æt. 13, admitted May 16, 1839, and who died Sept. 2d following, is an instance of the kind, and otherwise interesting. The boy was in a state of extreme debility and ill health on his admission; the pulse was small and intermitting, and he suffered from febrile accessions in the evening, followed by perspirations in the morning; the countenance was extremely anxious; the whole surface of the body was of a dark sallow colour, closely resembling jaundice. The ab-

domen generally enlarged; the liver could be distinctly felt extending below the ribs, and was extremely sensitive under examination. His respiration was sometimes impeded, and he had occasional slight cough. The tongue was furred; alvine excretions unhealthy; appetite defective. There was a large accumulation of pus at the upper and inner side of the thigh; he complained of pain in the hip, knee, and thigh, when lying; he walked with difficulty. Immediately over the femoral vessels was the cicatrix of an issue, about the size of half a crown. The hepatic treatment was adopted, and appeared to answer exceedingly well for some time, and his general health underwent great improvement. Meanwhile the sac of the abscess continued to increase in size.

July 11th.—The cicatrix of the issue gave way, and formed a small opening, through which several pints of pus were discharged. (It happened at this time that a sloughing sore had become epidemic at the Infirmary, and almost all persons suffering from ill-conditioned ulcers were attacked with it.) A few days after irritative fever set in, and the cicatrix sloughed. The condition of the patient now became lamentable: the discharge was profuse, accompanied by severe pain, and irritation of the interior of the sac: large sloughs came away through the opening, which by this time exceeded the limits of the cicatrix: the femoral vessels could be seen enclosed in their sheath, and perfectly isolated from the surrounding structures. It seemed that hæmorrhage was unavoidable. Fortunately, however, the sloughing was arrested, the febrile symptoms subdued, and he continued to go on well for several weeks. I was suddenly called to see him one morning: he was suffering from great thirst, together with a distended and painful state of the abdomen. He had vomited during the night. The tongue was furred. The discharge from the abscess, which had previously diminished, was now profuse: the wound, which only a few hours before had been in a healthy condition, was the very reverse: the thigh was exceedingly hot, and painful to the touch. This sudden change, together with the nature of the symptoms, led me to suspect that he must have taken improper food. On inquiry I

* It is a source of extreme regret, particularly to the medical officers of this establishment, that the funds of the charity have been, and continue to be, insufficient to admit of surrounding the building with a wall, for want of which improper food is often admitted, and various other abuses too easily practised.

found that he had eaten voraciously the day before of some that had been procured for him. He died five days after this occurrence.

The appearances after death were as follows:—Chest: The substance of both lungs occupied throughout with miliary tubercles, proceeding to suppuration at their summits; the walls of the left ventricle of the heart hypertrophied. Abdomen: Liver much enlarged and adipous; pancreas unusually firm and granular. The mucous surface of the intestines, from the duodenum to the rectum, presented throughout numerous patches of ulceration, which, in some places, had nearly destroyed the peritoneal covering. The colon was much contracted, and contained some scybalous matter. Local disease: Abscess in the substance of the left psoas muscle, commencing opposite the second lumbar vertebra, which had descended over the brim of the pelvis (producing caries), and formed a large sac in the thigh, extending nearly to the knee: the adjacent muscles were entirely divested of cellular tissue. The sheath of the femoral vessels was entirely laid bare for a considerable distance, but was in a healthy condition.

In my observations on the local and general treatment of these disorders, my aim has been simply to lay down a few leading rules, such as have appeared to me most worthy of attention; for it is scarcely possible to do more than this, when we consider the extent of functional disturbance, and the variety of structure involved in these affections, requiring all our skill and judgment to direct the treatment, which must constantly vary according to the exigencies of the case. For my own part I am very far from believing in the favourite maxim of some members of the profession, “that time does wonders in alleviating scrofulous diseases.”

It cannot be denied that there are instances in which patients do recover, under change of air, wholesome diet, &c. with little or no medical treatment; but these form the exception, in my opinion, and not the rule. We have seen that affections of the joint may ensue as a consequence of psoas abscess, but it is by no means impossible that both diseases may arise at one and the same time: yet, as far as my own observation has extended, such cases

are very rare; and indeed I have never met with more than one instance of the kind. This was in the case of John Garrod, æt. 9, admitted May 18, 1837. The local symptoms were as follows. Circumscribed hardness and fulness of the nates; great pain on percussion over the articulation; the limb shortened, the toe scarcely touching the ground, although there was no contraction of the knee; great pain in the hip and knee at night, and in the recumbent posture. He complained of much uneasiness on pressure in the lumbar region of the same side, and there was considerable fulness, together with tenderness, and a sensation of fluctuation at the lower part of the abdomen.

He stated that about seventeen months prior to his admission, he fell, in consequence of treading on a piece of orange-peel, and struck the hip violently; and that ever since the complaint had been on the increase. Some time previous to this he said that he experienced slight injury to the spine from a sprain; but there was no reason for supposing that this had any effect, as the symptoms commenced simultaneously.

From what has been said of the difficulty of the diagnosis, I will here take an opportunity of giving a brief summary of the symptoms which characterize them.

In *hip disease*, flattening of the nates, uncertain gait, apparent lengthening of the limb from canting of the pelvis; pain confined chiefly to the articulation, ascertainable on percussing the trochanter; rotation, outwards and inwards, flexion and extension, produce pain. In stooping to pick up any thing from the ground the patient bends the knee, but does not flex the thigh on the pelvis; less suffering in the recumbent posture. As the disease advances, lengthening of the limb from the destruction of the ligamentum teres; circumscribed hardness and fulness of the nates; altered situation of the great trochanter; shortening of the limb from dislocation of the head of the femur on the dorsum ilii, &c.; suppuration not generally profuse, and not always present.

In *lumbar abscess*, flattening of the nates, uncertain gait, frequent stumbling, canting of the pelvis, and apparent lengthening of the limb; par-

tial flexion of the thigh and knee; rotation outwards, and extension, productive of pain; none caused by flexion and rotation inwards. Pains in the hip and knee, at first erratic, afterwards constant, more especially at night, and in the recumbent posture; thigh flexed on the pelvis when lying. Absence of pain on percussing the trochanter; its prominence from position of the limb greater as the disease advances. Cessation of pain (constituting the second stage) as the matter gravitates and quits the psoas muscle, followed by a soft fluctuating tumor of the nates (the great trochanter retaining its natural position), or on the inner side of the thigh. Frequent exit of matter in the vicinity of the articulation; profuse discharge; shortening of the limb from contraction of the hip and knee-joints.

The following is an account of several post-mortem examinations of cases of lumbar abscess, which had been treated as disease of the hip-joint:—

E. F. Reynolds, æt. 12, admitted May 20th; died August 13, 1837.—Caries of the bodies of the second and third lumbar vertebræ, and brim of the pelvis; abscess in the left psoas muscle; total destruction of the cotyloid cavity, and of the head and neck of the femur; entire destruction of the soft parts; the integuments of the hip and thigh alone constituting the boundary of a vast abscess: there were three openings, two of which had arisen from the sloughing of the cicatrices of two large issues in the vicinity of the joint, and which had caused severe sufferings during life. The liver was enlarged as well as the spleen; the former viscus had contracted strong adhesions to the diaphragm; the pancreas was granulated, and considerably altered from its natural structure and appearance.

E. Hewlett, æt. 9, admitted May 11, 1839, died July following.—Abscess situated in the right psoas muscle; the sac, which was small, commencing opposite the third lumbar vertebræ; the spine free from caries; carious state of the brim of the pelvis; the matter had gravitated along the course of the psoas muscle, producing caries of the cotyloid cavity, and of the head and neck of the femur; the soft parts were entirely destroyed, and the integuments of the hip and thigh formed

the sole boundary of an enormous abscess, the sac of which was filled with pus and dark coagulated blood: there were two orifices, one situated on the upper and outer side of the thigh; another, which had formed over the sacro-iliac synchondrosis a few days prior to death, from which a profuse hæmorrhage took place, and terminated fatally in about thirty-six hours: the cicatrix of a large issue, placed behind the trochanter, was in a state of incipient sloughing on its inner surface.

Chest.—Several small scattered tubercles at the summit of the right lung; a vomica, about the size of a small nut, near its anterior surface; slight but long-standing pleuritic adhesions on the left side; traces of recent inflammation at the lower part of the lung.

Abdomen.—Liver slightly indurated, and somewhat altered in structure, inclining to nutmeg; scybalous accumulations in the colon.

Daniel Macarty, æt. 7, admitted May 18th, died July 20th, 1839.—Abscess commencing in the psoas muscle, opposite the last lumbar vertebra, descending beneath the fascia iliaca, passing over the brim of the pelvis: diseased state of the periosteum and subjacent bone at this point: the cotyloid cavity, together with the head and neck of the femur, were in a state of caries; a small perforation was found at the bottom of the former; an enormous ulcer, situated over the great trochanter, formed by the sloughing of the integuments in the previous situation of a large blister, was the outlet of the abscess.

Chest.—About four ounces of fluid in the pleura of the right side; the pericardial sac contained rather more than an ounce of serum.

Abdomen.—Contained nearly two pints of fluid; liver much enlarged and indurated, presenting a palish mottled appearance on incision, approaching to adipous degeneration; the gall-bladder was distended to nearly three times its natural dimensions, and contained a viscid straw-coloured fluid; the spleen rather small, indurated, and of a brightish red colour; the structure of the pancreas was unusually dense; tuberculated state of the mesenteric glands, with disease of the glandulæ agminatæ; scybalous accumulations in the colon.

Nancy Lockington, æt. 9, admitted

May 9th, and discharged Oct. 23, 1839.

—The patient did not die until the following winter, when I had an opportunity of a post-mortem examination. The abscess was situated in the left psoas muscle, and had descended beneath the iliac fascia, producing caries of the brim of the pelvis, and incipient consecutive disease of the hip-joint. The child died of phthisis. The left lung was completely solidified, from summit to base, by tubercular deposit, and there was an enormous cavity in the centre: there were other appearances of visceral disease, but as I did not take notes of the case at the time, I am unable to enumerate them.

Frederick Rose, æt. 18, admitted May 13th; died the 22nd of the same month.—Small abscess situated in the right psoas muscle, the matter from which had descended beneath the iliac fascia, over the brim of the pelvis (which was slightly carious) to the hip-joint; the cotyloid cavity was entirely destroyed by caries, as well as the head of the femur; there was total destruction of the soft parts, so that the integuments of the hip and thigh were the boundaries of a vast abscess; the head of the femur protruded through an opening which formed the outlet to the abscess during life.

Thoracic viscera healthy.

Abdomen.—Liver enlarged, adipous, and of an uniform dull yellow colour throughout its whole substance; strong adhesions to the diaphragm. Spleen enlarged and solidified; pancreas small, atrophous, flabby, and of a dull reddish hue.

Eleanor Stephens, æt. 12, admitted May 16; died July 19, 1840. Abscess situated in the right psoas muscle, arising from caries of the last lumbar vertebra; the body of the latter being nearly destroyed by it: the matter had descended beneath the fascia iliaca, following the course of the psoas muscle, to the hip, communicating with two orifices, one on the upper and inner side of the thigh; the other near the great trochanter. The capsular ligament of the hip-joint was entire; but on examining the cavity the ligamentum teres presented a purple tinge, and was somewhat thickened. On examining the head of the femur I observed a blackened appearance, which was perceptible through the investing

cartilage. On cutting through it I found the parts in a highly softened state; the cancellous structure of the bone could scarcely be recognised. Chest: slight pleuritic adhesions on both sides. The thorax contained about twelve ounces of serum, and the pericardium three. Miliary tubercles scattered throughout the substance of both lungs. Abdomen: liver enlarged, firmer than natural, highly congested, and here and there of a mottled yellow colour. The gall-bladder distended with bile of a very bright yellow. Spleen unusually hard, containing a small tubercular deposit in its substance.

Remarks on Sea Air, Bathing, &c.

The curative influence exercised by sea air and bathing, on scrofulous diseases, seems, as far as I have been able to observe, generally admitted by the majority of the medical profession: such being the case, it may appear presumptuous on my part to combat an opinion so commonly received: nevertheless I will venture to offer a few remarks on this subject, which are somewhat opposed to it.

It seems to me that a residence at the sea-side acts as variously on the constitutions of scrofulous patients as it does upon those who labour under other chronic diseases. The opportunities for observation are ample; for almost every variety of chronic affection may be witnessed at one time or other at this Infirmary, especially among the out-door patients. Some individuals, on their arrival, experience an exacerbation of their symptoms, loss of appetite, and other derangement of the general health, and an increase of the local disorder; upon others, little or no apparent effect is produced; whilst a certain number derive so much benefit in a short time that it would lead one to imagine that a speedy cure was about to be effected; and in a few instances it cannot be denied that it really happens. If a sea-side residence were so favourable to the cure of scrofulous maladies generally, it would follow, as a matter of course, that those who lived on the sea-coast should be particularly free from its influence; but this is contrary to the fact. The causes of the tuberculous, or phthisical diathesis are as common to it as to other parts of this country; some localities,

indeed, being more favourable to its production than others. Some of the severest cases of strumous disease received at the Infirmary have occurred amongst those who have been bred and born at the sea-side. Phthisis, the most destructive form of scrofulous disease, is common on the sea-coast.*

It may appear, perhaps, from the preceding observations, that I consider sea air, &c. prejudicial to strumous affections generally: this, however, is far from being the case; they are only offered as a caution to those who entertain an idea of their specific influence in curing them. Sir Arthur Clarke, of Dublin, (who had extensive opportunities for observation) in his *Treatise on Bathing* states, that he never saw any good arising from it in these cases. In my opinion, however, sea-bathing judiciously conducted, and combined with proper care and attention to the general health, is often of the greatest service. If much visceral disturbance exists, I have invariably found cold bathing uncertain, and often injurious in its effects: the warm bath forms a much safer means in such cases; but even this should be carefully used. The object of all bathing is the same, viz. to excite an action on the skin in a greater or less degree; and whenever it fails to produce this effect, some mischief commonly follows. The mode of cold bathing adopted at the Infirmary consists in immersing the patients once or twice only, and not allowing them to remain in any longer than is necessary to accomplish this. The warm bath is usually applied at a heat of 96 or 98 degrees, and persevered in for ten or fifteen minutes. My own plan of late years has been to use it of the latter heat, only allowing the patient to remain in five or six minutes. I have been induced to adopt this method, as I have found it far more beneficial than a longer time. In using the vapour baths it is best not to immerse the head. Few patients can sustain a course of bathing with the whole person exposed to the influence of the vapour, without injury to the general health. The douche baths, warm and cold, form an admirable relief in affec-

tions of the joints, spine, rheumatic affections, &c., and are often productive of great relief.

On the use of the Preparations of Iodine.

In the year 1832, I was requested by Dr. Canham (to whom the Infirmary must ever remain indebted for the introduction of the iodine treatment) to read M. Lugol's treatise on scrofula, and to place some patients under the treatment. From the very little knowledge I then possessed of those preparations I felt very sceptical as to the result, and entered on the plan with some reluctance. It was begun in one ward only; but it very soon became obvious that iodine was a remedy of much efficacy and power. The excellent effect produced on scrofulous ulcers generally was remarkable; sores hitherto unmanageable soon assumed a healthy aspect; glandular tumors were readily absorbed under its influence, or got rid of by suppuration. The peculiar power of remedying ill formed cicatrices, noticed by M. Lugol, could not fail to strike our observation. Chronic abscesses, which had hitherto resisted all the ordinary means, soon healed under its influence.

It would be foreign to my present purpose to offer any more than a few cursory observations on its mode of application at the Infirmary. A mild ointment of iodine, and iodine of potass, is commonly used as a simple dressing to the sores; and the use of sea-water (which formed a very irritating application to the wounds, generally speaking) is for the most part abandoned for the iodine lotions. Ioduretted fomentations are much used, and are found highly useful in some forms of the disease, especially in strumous ophthalmia. The ointments of iod. of mercury, sulphur, and lead, have been found very useful, especially in certain forms of skin disease. The internal exhibition of the remedy is now chiefly confined to the iodides of potass, iron, and mercury; the latter is prepared from Lugol's formula, and not from the Pharmacopœia.

In careful hands the various preparations of iodine cannot fail to prove highly beneficial and useful: and their introduction must be considered one of the greatest improvements in modern therapeutics.

* Whenever patients are received at the Infirmary in the latter stage, the symptoms invariably increase.

MEDICAL GAZETTE.

Friday, April 23, 1841.

“Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri; potestas modo veniendi in
 publicum sit, dicendi periculum non recuso.”

CICERO.

THE NEW REGULATIONS OF THE
UNIVERSITY OF CAMBRIDGE.

OUR readers will have observed with some interest that the University of Cambridge has taken part in the present general movement in medical affairs, and has published a new scheme of medical education for those who are desirous of its license to practise. The main object of the scheme, of which we gave a copy a fortnight since, appears to be to increase the length of study, and to add several new subjects to those with which an acquaintance has hitherto been required. Its publication has excited much surprise among those acquainted with the usual proceedings of the University, in consequence of its tacitly admitting some important principles to which the authorities had hitherto been supposed adverse, and of its tending to give the system of Cambridge medical education more of that popular and general character which had hitherto been peculiar to the younger universities.

It is admitted now in words, which has long been matter of fact, that the University does not afford all the education which its examinations require that the candidates for its degrees should have received. Hitherto the standard of the examinations (at least as far as the questions asked gave evidence of it) has been much above that of the instruction afforded in the University itself. For although its Professors of Anatomy, Medicine, Chemistry, and Botany, are second to none in the extent and depth of their several scientific acquirements, and in their

fitness for teaching, yet it has been but too evident that the limited means of practically studying the most important departments of the science—anatomy and clinical medicine—have put it out of their power to give their pupils that knowledge which they felt to be necessary to practitioners. Certificates of having attended hospital practice in other places than Cambridge have therefore been for many years required; and in general it has been supposed (though not expressly required) that anatomy has been studied by dissection in London or elsewhere. In the present scheme, however, foreign study is demanded to a much greater extent. Lectures must now be attended on all the old subjects, and, also, on medical jurisprudence, surgery, midwifery, and practical anatomy,—all of them lectures which neither ever have been, nor are ever at all likely to be, delivered within the precincts of the University.

Now we cannot but greatly doubt the wisdom of a measure such as this. If the University has hitherto had any reputation for the medical education which it has afforded, and which, as far as it went, has always been of the highest order, this scheme must tend to neutralize that reputation, by admitting, even in this limited extent of study, a suspicion of inferiority or insufficiency. By allowing and even encouraging its candidates for the license to obtain all their medical knowledge in the metropolitan schools, the University goes far towards assuming the character of a mere diploma-giving body, and towards entering into a class of institutions by whose association it cannot but be deeply disgraced. It is evident that the tendency of these regulations will be to make all Cambridge medical students attend their lectures in London; for some of those required are not given at all in Cambridge, and for these therefore they must study in

London, or in some large medical school; and of course, as a matter of convenience, the majority will attend their lectures at the same time and place, and will therefore separate themselves as much as possible from the University, with which it would have been for many reasons desirable that their connection should be maintained. It seems to have been forgotten that the defects of the system hitherto have been those of a want not of oral but of clinical and practical instruction, and that against these the present arrangement does not provide much more than the former did; while it adds nothing of importance or value to the prælectional part of education, to compensate for the discredit which it casts on that which has hitherto been given. We repeat, therefore, that by almost ceasing to offer to educate physicians, and to render its graduates fit for the reception of its diplomas, the University of Cambridge is in danger of falling into the same class with those of Erlangen, Tubingen, and others, that confer degrees on those with whom they have before had no connection whatever. Of course we do not mean that it can ever fall to their level; we cannot imagine circumstances that would so degrade it; but the danger is lest it should fall to the same class with them, and others, in which diplomas, but no adequate instruction, are given.

That such an error as this appears to us should be committed, is the more strange, when it is considered how little it is necessary that Cambridge graduates should be subjected to the course of education now made compulsory upon them. The present curriculum is actually, as far as lectures are concerned, almost identical with that established by the Society of Apothecaries, against which the most reasonable complaint has always been, that it requires the study of a number of

subjects more or less irrelevant to the actual practice of medicine. And against this objection the only rational answer ever made has been, that the study of these subjects has a tendency to elevate the mind, and to increase the intellectual capacity, of the student; to prevent his becoming a mere empirical practitioner, and to give him, as far as possible, the exalted tastes, and the powers, of the man of science. Whether they do so or not may be much questioned; we greatly doubt it; and are, at any rate, sure that the general discipline of the intellect ought to be completed before the study of medicine is commenced; for surely it is absurd that a man should be studying a science, and learning to study, at the same time. However, as we have said, this is the best argument that can be advanced for the introduction of the collateral sciences, into the course of medical studies.

But, for this purpose, these sciences are, or ought to be, altogether unnecessary to Cambridge graduates. The previous studies of the University have for their especial object to render them fit for the study of all other sciences; and in reference to the science of medicine, to teach them, if not to be physicians, at least how they may learn to become so. The intellectual discipline of the graduate is supposed to be perfected when he takes his degree in arts, or has kept the terms necessary for that degree; he might therefore, and ought to, proceed at once to the practical study of the science of his profession. To him chemistry and botany should be only adjuncts to physiology, which, if any one can do it, he ought to be able to contemplate in all its wide extent, as a science involving the applications of the principles of nearly all sciences, whether of dead or living matter. To practical medicine, chemistry and botany, as at present taught, they have no direct re-

lation whatever; unless they are studied as sciences merely, and for the sake of intellectual exercise, for which purpose mathematics and comparative anatomy would answer as well or better, they are absolutely unnecessary even to the ordinary medical student. To the Cambridge graduate we are bound to regard them as superfluous.

Again, with regard to medical jurisprudence. It is plain that this can consist only in the application of the facts and principles of medicine in general to the peculiar circumstances in which they become mixed with questions of law; and in all these a generally well-educated man must be supposed able to make the applications for himself, in each case in which he is called upon to do so. It would be a scandal on the general system of the Cambridge education to suppose that those who have received all its advantages are incapable of doing thus much without special instruction; yet this one is almost compelled to suppose, on the evidence afforded by the recently published curriculum.

On the whole, the scheme seems to us very objectionable. It places the graduate of a distinguished University on almost exactly the same level as the student who has just left a country grammar-school. It seems to suppose the one as little capable of guiding himself in his studies, or of appreciating, by his knowledge of the principles of universal science, the facts of a new branch of it, as the other. If the University authorities deem a knowledge of all the prescribed subjects essential to the character of an accomplished practitioner—and on this we would not dispute—they needed only to have given out that these subjects would be included in the examinations; but they might surely have left the mode of attaining such

knowledge to the judgment which all their previous system of education was intended to cultivate and to perfect. Why should they tie down their candidates to all the annoyances of attending this and that course of lectures, when it might be both easier and better for them to study the subject by actual practice? We should have been less surprised if regulations had been published absolving graduates from all necessity of attending lectures, than we are at that before us: such a scheme, though fraught with danger in the case of the great mass of medical students (whose deficiency is generally an inability to study aright, rather than an inability to learn what is put before them), would, we believe, have been perfectly safe to Cambridge graduates. At any rate, all the instruction by lectures which they can be supposed to need might well have continued to be supplied by their own professors; and charged with this, and with their intellectual powers strengthened by their exercise in the ordinary studies of the University, they might safely be trusted to work their own way in the great field of medical science. To suppose the contrary would be to set oneself against the evidence of all past experience, which has manifestly shown that under their old system the doctors of the English universities have been able at all times to place themselves in the very first ranks of their profession, and against the existing fact of their being at the present day in no wise inferior to any of those whose system of education is in future to be imitated.

Another objection urged against the new scheme is, that it is made retrospective, and will, therefore, fall very inconveniently on many who have been leisurely pursuing their medical education, in the belief that the old system would be continued, at least as far as

their interests were concerned. We are not aware of any good reason why, in this instance, the principle hitherto almost universally acted on in the University, of making all changes entirely prospective, should have been departed from. We can hardly suppose that it was thought necessary to do this in order to avoid an accusation of indifference or bigotted attachment to old things: the steady progress of improvement which has long been going on in many departments of the University, would have secured it against conviction on such an indictment; and surely, if there were no very urgent reason for the present precipitancy, it would have been better both for the convenience of those with whose plans the new scheme will materially interfere, and for the maintenance of the good and long established principle of avoiding retrospective changes, to have shielded all but future students in medicine from the demands of the new and burdensome curriculum.

BIOGRAPHICAL

MEMOIR OF MR. WINKFIELD.

To the Editor of the Medical Gazette.

SIR,

I HAVE ventured to indulge the belief, that a brief memoir of the life and character of a real country surgeon—"a man to all the country dear"—who, for upwards of half a century, ruled without a rival in all medical matters over an extensive district of Hertfordshire, will not be without interest to a large body of your readers. Those among them to whom the name of old Winkfield, of Market Street, is familiar, will, I am sure, be gratified by a few brief notices of his birth, parentage, education, and career. Those who now hear the name for the first time, will grieve to think that so much benevolence is gone out of the world, and that they

can never hope to enjoy the privilege of his warm and hearty friendship.

Your obedient servant,
GEORGE GREGORY, M.D.

31, Weymouth Street,
April 15, 1841.

John Winkfield was born at King's Langley, Hertfordshire, on the 12th March, 1752—a memorable year, when the old style was changed for the new. His parents were respectable people, of small independent property. They had nine children, eight girls and an only son, the subject of this memoir. At the early age of twelve years, John Winkfield was apprenticed to Dr. Kirby, of Luton, with whom he resided for seven years. On the termination of his apprenticeship he proceeded to London, and became a pupil of St. Bartholomew's Hospital. He studied surgery under Mr. Pott, but could not have devoted much time to the instructions of this great master; for, in 1772, we find him, after passing a creditable examination, commencing practice at Market Street, being then only twenty years of age. In that village Winkfield continued to practise for the unprecedented period of sixty-two years, during which time he had successively seven partners. He finally retired from business in 1834, when he had attained the age of 82. Mr. Charles Sweeney, now of Upper Seymour Street, succeeded him.

Winkfield, in the course of his medical career, enjoyed the friendship of many of our first surgeons. Sir Astley Cooper, Mr. Cline, and Mr. Abernethy, knew him intimately, and always spoke of him with affectionate regard. Abernethy makes honourable mention of him in one of his works. For those luminaries of surgery Winkfield entertained feelings of regard approaching to devotion. Often have I heard him repeat with pride the casual remark of Sir Astley Cooper, when Winkfield had expressed his admiration of some masterly effort of Sir Astley's skill, "You are a great operator yourself, Mr Winkfield."

On his skill as an operator Winkfield prided himself not a little; and from all I can learn, he really had earned for himself a good name as a bold, skilful, and successful operator. The most remarkable case in which he

was ever concerned was that of a man through whose abdomen a ramrod passed, which penetrated several inches through the back. The man subsequently recovered, without any serious symptoms arising. He was sent for on one occasion to visit a man whose chest the shaft of a gig had entered, and who survived the injury. He was frequently in the habit of mentioning a case of amputation of the arm, which healed without any vessel requiring to be tied. The probable cause of this peculiarity was the previous long-continued pressure of the tourniquet, while the assistance of the surgeon was sought.

His professional reputation stood so high throughout Bedfordshire and Hertfordshire that he was generally consulted in all difficult surgical cases, and the duty of operating generally fell upon him. As an accoucheur, too, he was in high esteem, and his advice was largely resorted to in difficult labours.

With his professional neighbours Winkfield was always a great favourite. He was a constant attendant at the annual dinner of the Medical Benevolent Society held at Dunmow, of which he was long the oldest member. On these festive occasions he always presided, and ruled supreme over the mysteries of the *cuisine*. His epicurean whims were kindly indulged by the country doctors, in consideration of the infinite amusement which they derived from the simplicity and rusticity of Winkfield's character, and his singularly original mode of expression. By the lower orders of people Winkfield was, through life, greatly beloved. He had a ready joke for those who were merry, and a kind word for those who were in affliction. He early secured the esteem and affection of the most opulent man in his neighbourhood, the Honourable Mr. Cavendish, who for many years expended a noble fortune in Market Street. Winkfield was the caterer and field-marshal of the dinner-table; but he was more:—he was the kind and generous distributor of his patron's bounty.

Winkfield passed the latter years of his life at Barnet, with his daughter Mrs. Morison, enjoying, until within a very recent period, surprisingly good health. In the autumn of 1840, he had risen in high health and spirits, looking fresh and rosy; and, ere the family were

stirring, had set off in his pony-chaise to visit Hendon, the scene

“Where once his careless childhood stray’d:
A stranger yet to pain.”

Having a strong, almost irresistible, propensity towards bricks and mortar, he stopped, *en passant*, to see the almshouses then in course of erection by the Leather-cutters' Company. Regardless of the risk, and heedless of the warnings of the workmen, he persevered in exploring the building, till unhappily he slipped through a trap-door, and broke his leg just below the knee. His health had shewn symptoms of decay previously, but this serious accident put the finishing stroke to his career. The fracture, indeed, healed, under the skilful care and unceasing attentions of his kind friend and neighbour, Mr. Green, but the constitution gradually gave way. His appetite, at length, totally failed, and he sank, exhausted, on the 28th December, 1840, and was buried in the family vault at Flamstead church. His remains were attended to the grave by a large concourse of people collected from all parts, anxious to evince their sorrow at the loss of so estimable a man. An impressive funeral sermon, preached on the occasion by his old friend, Mr. Wheeldon, spoke his warm but well-merited eulogium, as a kind neighbour—a faithful friend—a skilful surgeon—and an affectionate parent.

Mr. Winkfield was twice married. His first wife was Miss Eliz. Fleming, by whom he had three children, all of whom died before him. He married, secondly, Mrs. Benger, a widow, by whom he had one daughter, married to Mr. Morison, surgeon, of Barnet, who survives him.

My own acquaintance with Mr. Winkfield was comparatively recent. About ten years ago he first called on me, and soon, by his kindness of heart, and singular simplicity of manner, won my esteem and regard. His last visit to me was on the 20th May, 1840, when his appetite and strength were beginning to fail him. On that occasion he described to me a very curious suffusion of vision under which he had formerly laboured. For many successive nights he perceived his white handkerchief covered, as he supposed, with blood, and was with difficulty

persuaded that his nose had not bled. He told me that he had inquired, in vain, from his professional friends, for some explanation of the phenomenon, and was highly gratified when I directed his attention to an analogous case recorded by Dr. Heberden, in the fourth volume of the Transactions of the Royal College of Physicians. On the same occasion Mr. Winkfield gave me the following details of the ages of some of his own family, proving, I think, very incontestibly, the principle of hereditary longevity. Mr. Winkfield's age, at that time, was 88. His father died aged 75. His mother died aged 97. His eldest sister died aged 88. His next sister died aged 93. Another sister was then living (and perhaps still survives) near Windsor, aged 82.

So long a period of active professional life, in a populous and wealthy neighbourhood, ought to have ended in the acquisition of a competent fortune; but his farming speculations, and that unhappy fondness for building, to which I have before adverted, marred this golden harvest. *Diruit ædificat* might have been the motto to Winkfield's crest.

Mr. Winkfield, in early life, was tall, and of so slim and slight a make as to have acquired the soubriquet of *Threadpaper*. His complexion was blooming, with bright blue eyes, and light auburn curls. As years advanced, he became stout and corpulent; and as such I first knew him. The animated countenance, the pleasant playful smile, and cheerful aspect, however, never left him. They spoke at once of the warm heart and truly benevolent disposition within, which, for seventy years, had rendered Winkfield, of Market Street, an honour to his family, to his profession, and to his country.

ROYAL MEDICAL & CHIRURGICAL SOCIETY.

March 23, 1841.

DR. WILLIAMS in the Chair.

Cases of Paralysis and Spasm of Voluntary Muscles from Sympathetic Affections and Actions of the Brain and Spinal Cord. By PETER NUGENT KINGSTON, M.D. Physician to the St. George's and St. James's Dispensary.

AFTER some general remarks on the influence of the brain and spinal cord on the

motor nerves, partly through the medium of volition and emotion, partly through centripetal nerves, conveying impressions to the nervous centres, and producing involuntary movements; the author observes that, although the existence of certain secondary affections and morbid actions of the brain and spinal cord is admitted, their general history has, as yet, been little developed. Our ignorance, he observes, till lately, that in those transient affections of the voluntary muscles which seem to be excited by impressions on remote organs, and which have been termed sympathetic, the nervous centres with their centripetal and centrifugal nerves always interwoven as immediate agents, preclude our referring such affections of the motor nerves to their real source. And, in instances of more palpable affections of the brain and spinal cord, the relation between it and any coexisting affection of a remote organ is most frequently so obscure, that it is still difficult to meet with cases in which the dependence of the former upon nervous impressions communicated by the latter can be unequivocally proved.

In order to illustrate this subject the author adduces a series of cases witnessed and conducted by himself. He conceives that they may exhibit the share which the impressions arising from disease, in other organs, may have in the production of cerebral and spinal affections; and the effect which these again may have in suspending the control of volition over voluntary muscles, and even exciting in them voluntary movements. In this view he presumes that the remote morbid impressions have not been alone adequate to the production of the involuntary movements.

The cases are grouped by the author under two heads, according as there was or was not actual disease of the nervous centres.

The first case, under the first of these heads, is one of general paralysis, under mucous irritation from the presence of tænia. The second case is one of general paralysis under the irritation of psoriasis.

In regard to these, and many other very carefully detailed cases, the author expresses his objection to the opinion of those who should contend that the observed phenomena arises from a direct sympathy, without the intervention of the cerebro-spinal centre. And he notices the opinion of this effect expressed by M. Cruveilhier, in 1829, respecting the tonic contractions of a painful abdomen.

After proceeding under his second head through a careful series of cases, in which the predisposition to sympathetic affection may be attributed to an actual lesion of

the brain or spinal cord, the author concludes with expressing his hope that the knowledge obtained from such sources may direct us to a discriminating use of *known* remedies which had previously been employed empirically.

The President said, that the paper having involved that very interesting subject, the doctrine of sympathies, he should be glad to hear from the author what were his general opinions on it. He seemed to have adopted nearly the opinion of Dr. Whytt.

Dr. Kingston said that he was inclined to accord with the opinion now generally prevalent, and adopted, he believed, by all the best living authorities, that no sympathy of remote motor organs is ever exhibited, except in consequence of an impression conveyed to the nervous centres, the brain or spinal cord, and from them transmitted to the organ said to be sympathetically affected. He did not, however, regard the spinal cord as some did, as merely forming a part of a nervous circle, through which an impression is conveyed from a sensitive to a motor nerve, but rather as a central organ, to which impressions of one kind are conveyed by one class of nerves, and from which impressions of a different kind, regulated by some natural laws, are conveyed through another class of nerves.

Mr. Cæsar Hawkins said he should be very glad to receive information on a subject nearly related to that of the paper—he meant the treatment of what is called infantile paralysis, the disease in which children lose more or less completely the voluntary power over one or more limbs, without any loss of sensation, or any evident cerebral or spinal disease. He had found severe cases of this kind singularly incurable; slight ones, he knew, admitted of relief from a variety of means; but in others, where the paralysis was complete, he had found almost every sort of treatment wholly unavailing.

The President said many members present could speak on the subject from more extensive experience than himself, but, as far as his own observation went, he had found small doses of calomel the most efficacious remedy. In four or more cases he had treated, they had proved evidently beneficial.

Dr. Merriman said his own experience was nearly to the same effect as Mr. Hawkins's. Severe cases of the diseases were indeed almost incurable; but of all remedies for them he thought the most beneficial was electricity. In two cases, especially, both severe ones, he had seen excellent results follow its employment.

Mr. Hawkins said he had frequently employed electricity, but without avail. He had, however, observed, that in some forms of paraplegia it had remarkable effect in increasing, at least for a time, the involuntary

motions of the limbs, which so often occurred in association with the loss of voluntary power. He thought this was particularly the case in paraplegia from disease of lumbar vertebræ; and he had recently had a case under his care, in which a lad thus affected was treated with galvanism, as well as many other equally inefficacious means. Every time that he was electrified, the lower limbs were throughout the rest of the day, or at least for several hours afterwards, incessantly affected with involuntary motions, like those of chorea, which only gradually ceased and returned again the next time he was similarly treated. Mr. Hawkins then related a singular case of paraplegia lately under his care, in which electricity and all other means had been unsuccessful. The child who was the subject of it was in the habit of walking on his hands, with his legs (which were almost completely paralytic) turned up over his shoulders; and his arms, thus continually exerted, had gained extraordinary power, so that he preferred using them as legs to adopting any measure for strengthening the lower limbs.

Dr. Addison was inclined to ascribe much more value to the use of electricity in cases of paralysis than Mr. Hawkins. In cases of infantile paralysis he thought that he had been able to make out that when one limb only is affected, the case is likely to do well; but if the similar limb on both sides of the body had lost its power, then the case was seldom remedied. In the former class of cases he thought time did more than any thing else: he had seen a considerable number in which recovery had gradually taken place, without its being possible to ascribe it to any remedy, although the child had perhaps been continually under medical treatment. The cases of paralysis, especially benefited by electricity, were those that affect parts of the body in hysterical and nervous persons, and in those affected with chorea, between which and local paralysis he need not say there was a very close connection. In all these electricity properly and assiduously administered was a most valuable measure. The mode followed at Guy's Hospital was the drawing of sparks along the whole or a part of the vertebral column till a distinct efflorescence or redness was produced. In one case the effects had been most remarkable: it was that of a lad in the hospital, whom there was no reason whatever to suspect of deception, and who he thought he could be certain was not guilty of it. He had had for some time almost complete paralysis of one lower extremity; but after the very first time of being electrified he slipped off the stool, and walked away perfectly recovered, and had had no sign of paralysis since. Dr. Addison also related briefly a case at present in the clinical wards of Guy's

Hospital, in which chorea had supervened on paralysis of the facial nerve in a young girl, with considerable alleviation of the latter.

Dr. Mayo related a case of paraplegia which he believed resulted from disease of the arachnoid membrane of the spinal cord, and which had commenced, as he suspected these cases commonly did, with what appeared to be rheumatic pains of the limbs.

CORONER'S INQUEST.

UNFOUNDED IMPUTATION ON A MEDICAL GENTLEMAN.

To the Editor of the Medical Gazette.

SIR,

I BEG to send you the enclosed account of a coroner's inquest which has been held in this city, considering that its more extensive publication in the GAZETTE may be useful to the profession, in showing the injury which may arise to a respectable practitioner from the condemnation of his treatment of a case by a medical man who may happen to be called in after him.

Your obedient servant,

* * * *

Coventry, April 17, 1841.

On Saturday evening, the 3d inst. a Coroner's Jury was impannelled at the White Hart Public-house, Much Park Street, to inquire into the death of Frances Brown, wife of David Brown, a watchmaker, residing in that neighbourhood.

Deceased having died on Thursday, the 1st of April, her husband shortly afterwards went to Mr. C. Holt, the Registrar for St. Michael's District, to obtain a certificate of the registry of her death. One of the necessary inquiries of the Registrar is, as to the cause of death of the person to be registered, and on putting this question to Mr. Brown, he replied, as stated by the Registrar, and witnessed by one or two individuals in his office, "Murdered by Doctor West." Naturally struck with astonishment at such a declaration, the Registrar repeated his question, and received the same reply again, or one conveying the same imputation. Upon this, therefore, he felt bound to acquaint Mr. West of what was said, and accordingly did so; and the latter gentleman, not less astonished than indignant at such a charge, immediately took the necessary steps to have the fullest inquiry and investigation into his conduct in the treatment of deceased, whom he had attended as a patient. It will be seen from what follows that Mr. West attended deceased till within a few days of her death, and that Dr. Arrowsmith being called in, led to the discontinuance of Mr. West's visits.

The first witness sworn and examined was Sarah Webb, a practising midwife, who stated that she was called on by Mr. Brown to attend his wife on the 4th of March. That she did so; and after remaining with her for about an hour, or an hour and a half, the symptoms were such that she advised Mr. West to be sent for. Mr. Brown immediately went to fetch him, and shortly after returned in company with Mr. West; but in the meantime the child was born.—The witness then proceeded to give an account of the means used by Mr. West, stating that, to the best of her judgment, he displayed skill in what he did, and acted as she had observed other experienced surgeons do.

Dr. Arrowsmith being next examined, said, I was called in to attend Mrs. Brown on Tuesday, the 30th of March, about half-past three in the afternoon. This was the only time I visited her, and I was then with her about ten minutes. She appeared in a state of considerable insensibility; but I think was conscious of my presence. She was paralysed on the right side, and could not speak; and I understood from some persons in attendance that she had been in that state some days. Her eyes were closed, but she moved her left hand, which, as I interpreted it, she intended to be expressive of the hopelessness of her state. I inquired if she had had leeches to her head, or if her hair had been taken off; and when I was told that these means had not been used, I remarked it was strange treatment to have omitted these and similar remedies. Some medicines were shown to me, one in a small bottle, which I found to contain ether. Of that small bottle the husband told me she was then taking; and I remarked it was ether. I inquired what further means were being used, and was told that Mr. West had ordered brandy and wine, as he considered her complaint to arise from weakness. I said I thought not, and that they had better not give her those things. Nothing further transpired in the room; but when I left the room, the husband came out with me, and I said to him it was not in my power to render her all that assistance she required; but I recommended him to enter her immediately in the Self-Supporting Dispensary. I then left, and knew nothing of what further transpired. I was told by some persons whom I saw in the room that deceased had been delivered about a month before.

Mr. Nankivell, surgeon, examined.—I saw deceased the first time on Tuesday, the 30th of March, about eight o'clock at night. She was then in a state of complete paralysis; she had lost the sensibility of both sides, as was shewn by her not appearing to feel in the least on her skin being pinched; there was also loss of motion on both sides, the

limbs remaining in exactly the same position in which they were placed by a second person. She was in a state of stupor, inclined to dose and sleep, but still capable of being roused to a slight degree of consciousness, as appeared by her fixing her eyes on persons in the room, and by her attempting to put out her tongue when I requested her to do so; but she seemed unable to speak. The urine escaped involuntarily; she was in a profuse perspiration, and her pulse was extremely quick and weak. I heard she had been confined about a month previously; and that a few days before I saw her she had been taken speechless, and lost the use of her right side.

On Sunday afternoon last, about three o'clock, a *post-mortem* examination took place, at which I was present. On opening the head, there was a slight congestion of the vessels of two of the membranes of the brain, the *dura mater* and the *pia mater*. The brain was much firmer than usual at such a period after death. The convolutions of the brain were slightly flattened; the substance of the brain did not contain more than the usual quantity of blood; the choroid plexus was very pale: the principal diseased appearance in the brain was an effusion of serum or watery fluid in the ventricles, and at the base of the brain. The rest of the body was remarkably deficient of blood. There was much less than the usual quantity in the posterior portion of the lungs; the heart was soft and flabby, and contained little blood; the liver, stomach, intestines, and kidneys, were remarkably pale, from containing less than the usual quantity of blood; the spleen was less gorged with blood than usual.—After describing farther the state of the body consequent upon the particular situation of deceased prior to death, Mr. N. continued—My opinion is, that the cause of death was the pressure on the brain from the quantity of fluid it contained. This may have arisen from various causes; but from my not having seen the whole progress of the case, it is difficult, perhaps impossible, for me to say what was the exact morbid condition upon which the affection of the head depended; but from the examination of the body, especially from its exsanguine condition or deficiency of the blood throughout the various organs of the body, it is most probable that the state of the brain was the effect of loss of blood, or of reaction from loss of blood. The serum, or watery fluid in the brain, was likely to be, and no doubt was, the cause of paralysis. I consider the state of bloodlessness of the different viscera one which would be attended with debility and exhaustion, and render it improper to use active depleting measures; and that, in all probability, it would require, in some degree, an opposite treatment. I

therefore infer that wine and brandy would not be injudicious.

Mr. John Bury, surgeon, examined.—I attended a *post-mortem* examination of deceased on Sunday last, and have been present during the whole of Mr. Nankivell's examination to-day. I concur with him generally in the statement he has made, but there is one little point I think he omitted; that is, the opacity of the arachnoid membrane of the brain, and which gave to my mind the idea that the brain had been subject to disease for some time; and this idea is confirmed by the history given by those who say she had been subject to head-ache for many years. On the choroid plexus a quantity of lymph was effused. As to the cause of death, I consider she was a feeble person previous to her confinement, and probably with a diseased membrane of the brain before also, most likely from a severe labour and much hæmorrhage. I am not surprised at the fatal termination of her illness in apoplexy or paralysis. I have been present during the examination of Mr. West, and I am not aware that his conduct or treatment of the woman was at all improper, or injudicious.

Mr. William Laxon, surgeon, examined.—I was present at the *post-mortem* examination of deceased, and have been present during the examination of Mr. Nankivell and Mr. Bury, and I concur in what they have stated as to the appearance of the body, and in the points added by the latter surgeon. I consider that deceased came by her death by the visitation of God in a natural way, and not from any injudicious or improper treatment inflicted by any one. I consider that she died of serous apoplexy, to which she had a predisposition in the affection of the brain.

Mr. Nankivell said he entirely concurred as to the point alluded to by Mr. Bury and Mr. Laxon, respecting the predisposition to disease of the brain.

The evidence being completed, the Coroner observed to the Jury, that from what they had heard in the testimony which had been delivered touching the death of Mrs. Brown, they must be aware that her death was perfectly natural under the circumstances of her case, and that there was in reality no more occasion for holding an inquest upon her than for any other individual dying in the ordinary way. The only thing which made an inquest desirable was, the unfavourable report which had got abroad respecting Mr. West, whose conduct was now proved to have been perfectly humane; who had shown the greatest possible promptitude in visiting the patient when called upon, and evinced the utmost anxiety to relieve and restore her by the skilful mode of treatment he had adopted. After further eulogising the conduct of Mr. West by argu-

ments drawn from the evidence on the inquest, the Coroner expressed his readiness to read the whole or any part of it which the Jury might wish again to hear; but all seemed perfectly satisfied. The Coroner then adverted to the strange representation made by the husband of deceased to Mr. Holt, the Registrar, in answer to the inquiry made by him in the discharge of his duty as to the cause of her death.

The Jury immediately returned a verdict that deceased “Died by the visitation of God, of serous apoplexy.”

The inquiry lasted nearly six hours.

NORTH OF ENGLAND MEDICAL ASSOCIATION.

A NUMEROUS and highly respectable meeting of this Association took place at Newcastle last week; Dr. Headham in the chair. Numerous speeches were made in favour of medical reform, which we regret that our confined space prevents us from reporting. It was moved by Mr. T. M. Greenhow, seconded by Mr. Gregory, and unanimously carried—

“That, in the opinion of your petitioners, the Bill for the better government of the medical profession, now before Parliament, is based on principles which are calculated to confer important benefits on the community.

“Your petitioners, therefore, humbly beg that those principles may receive the sanction of your Honourable House.”

SOCIETY FOR RELIEF OF WIDOWS AND ORPHANS OF MEDICAL MEN.

THE annual dinner of this society took place last Saturday, and was numerously attended, H.R.H. the Duke of Cambridge in the chair. We regret that we cannot make room for any report of the proceedings, but this we regret the less, as it is our intention, on an early occasion, to bring the objects of this institution under the more particular notice of our readers.

METEOROLOGICAL JOURNAL.

April.		THERMOMETER.		BAROMETER.	
Wednesday	7	from 32 to 54		29.73 to 29.74	
Thursday	8	36 53		29.74 29.76	
Friday	9	39 54		29.80 29.92	
Saturday	10	34 50		29.99 29.97	
Sunday	11	27 49		29.93 29.89	
Monday	12	30 47		29.86 29.87	
Tuesday	13	38 50		29.89 29.91	
Wednesday	14	from 24 43		29.99 30.05	
Thursday	15	31 45		29.82 29.78	
Friday	16	29 47		29.75 29.79	
Saturday	17	34 53		29.89 29.96	
Sunday	18	28 61		29.83 29.86	
Monday	19	40 55		29.84 29.85	
Tuesday	20	33 52		29.74 29.65	

Prevailing Wind, S.W.

On the 7th overcast, rain in the night. The 8th, morning clear, otherwise cloudy, rain in the evening. The 9th, generally clear, a shower of rain in the afternoon. The 10th, evening clear, otherwise overcast. The 11th, morning cloudy, with rain, otherwise clear. The 12th and following day cloudy, rain on the evening of the 13th. The 14th and morning of the 15th clear, afternoon and evening of the 15th cloudy with rain. The 16th and 17th generally clear. The 18th, afternoon clear, otherwise cloudy. The 19th, generally clear, a shower of rain during the morning. The 20th, overcast.

Rain fallen, .26 of an inch.

CHARLES HENRY ADAMS.

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Friday, April 16, 1841.

H. B. Norman.—W. Toogood.—G. F. Keys.—R. D. Pritchard.—R. Axford.—C. Mott.—J. W. Young.—A. Thorn.—H. M. Rawden.

APOTHECARIES HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, April 15, 1841.

Charles Snow Walker, Liverpool — Joseph Agar Locking.—Frederick Wood.—William S. Partridge.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 10th April, 1841.

Small Pox	23
Measles	5
Scarlatina	11
Hooping Cough	53
Croup	11
Thrush	5
Diarrhoea	5
Dysentery	1
Cholera	0
Influenza	13
Typhus	17
Erysipelas	6
Syphilis	0
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses	124
Diseases of the Lungs, and other Organs of Respiration	251
Diseases of the Heart and Blood-vessels	20
Diseases of the Stomach, Liver, and other Organs of Digestion	45
Diseases of the Kidneys, &c.	4
Childbed	5
Ovarian Dropsy	0
Diseases of Uterus, &c.	5
Rheumatism	1
Diseases of Joints, &c.	4
Ulcer	1
Fistula	0
Diseases of Skin, &c.	2
Diseases of Uncertain Seat	96
Old Age or Natural Decay	45
Deaths by Violence, Privation, or Intemperance	24
Causes not specified	3

Deaths from all Causes 780

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, APRIL 30, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXXII.

Tetanus. Its symptoms and varieties. Causes. Diagnosis. Pathology. Treatment: opium; blood-letting; the warm bath; the cold bath.

IN those diseases of the nervous system which have hitherto engaged our attention, the function of voluntary motion, when it has been affected at all, has mostly suffered in the way of diminution, or suspension; the power of moving has been impaired, or lost; there has been complete or incomplete *palsy*. Sometimes, indeed, convulsion, or an irregular and involuntary action of the muscles, has also occurred. But, distinct from the paralytic affections, there is a class of *spasmodic* diseases, of which it is the main and leading feature, that the function of voluntary motion is (not morbidly heightened, as in the preternatural strength of a madman; nor lowered, as in *palsy*; but) *perverted*: performed in an irregular and unnatural manner.

There are two sorts of spasm. One of these is marked by a long-continued contraction of the affected muscles, not rapidly alternating with relaxation: the relaxation taking place slowly, after some time; and then, perhaps, the contraction, after another interval, coming on again. This is called *tonic* spasm; and, by Cullen, *spastic rigidity*. A very familiar example of it is the common *cramp* of the leg. In the other form of spasm, the contraction of the affected muscles takes place repeatedly, forcibly, and in quick succession; and their relaxation is as sudden and frequent. This has been named

clonic spasm. We find illustrations of it in convulsions.

Sometimes the two are mixed together in the same disease; certain muscles undergoing convulsions or *clonic* spasm, and certain other muscles being affected with rigidity or *tonic* spasm. But it is convenient to keep the distinction in mind.

We recognise these disorders by the unnatural conditions of the *muscles*; but you will please to remember that the fault lies in the *nervous* system.

With regard to spasmodic diseases generally, I may say that some of them constitute the most appalling and fatal maladies to which the human body is liable; and some of them, though frightful to look upon, and productive of extreme distress to patients and their friends, are trivial in their consequences, and scarcely ever attended with any peril to life.

I propose first of all to consider one of the most formidable and worst of these spasmodic diseases, viz. *tetanus*; of which *tonic* spasm is essentially characteristic. Its name is derived from *τείνω*, to stretch.

In respect to all those diseases concerning the exact pathology of which we are ignorant, and which we identify by the group of symptoms they present, rather than by any organic changes of structure in any part of the body, the most convenient mode of proceeding will be, first to describe the distinctive symptoms.

Tetanus, then, is characterized by an involuntary, long-continued, violent, and painful contraction—in one word, by *cramp*—of the voluntary muscles of various parts, or of nearly the whole body.

There is no difficulty in recognizing the disease when it is fully formed. But it is of much importance to be aware of the marks of its approach, and of its earliest symptoms; in respect of the treatment to be then adopted.

In general, the muscles that seem to be the earliest affected are those of the neck,

jaws, and throat. The patient feels a difficulty and uneasiness in bending or turning his head; and supposes that he has got what is called a stiff neck. He finds also that he is unable to open his mouth with the customary facility. At length the jaws close: sometimes gradually, but with great firmness; sometimes (it is said) suddenly, and with a snap. In four cases, perhaps, out of five, the disease begins in this way, with *trismus*, or *locked jaw*; so that this last is the vulgar name for the complaint. Along with this symptom, or very soon after it, the muscles concerned in swallowing become affected; and in a short time there comes on, what is often the most distressing part of the disorder, an acute pain at the lower part of the sternum, piercing through to the back. This depends, it can scarcely be doubted, upon cramp of the diaphragm. The pain is subject to aggravation in paroxysms; and each paroxysm of pain is attended with increased contraction of the other parts also that are implicated. The spasm extends to the muscles of the *trunk*; to the *large* muscles of the *extremities*; the muscles of the *face*; and last of all, in general, to the muscles of the tongue, and of the hands and fingers, which often remain moveable at the will of the patient, after all the other voluntary muscles of the body have become fixed; and frequently the muscles of the wrists and hands escape altogether.

With respect to *all* the muscles involved, from the time when they are first affected to the time when the disease is relieved, or the patient dies, they *continue* in a state of contraction, and are swelled and hard in their centres. The jaw, for instance, can never be completely opened; and the muscles of the abdomen are so rigid as to make it as hard as a board. But, besides this, they are all subject to aggravations or exacerbations of the spasm, which occur perhaps every ten minutes, or quarter of an hour, and last for two or three minutes at a time; and then the muscles fall back into the minor degree of contraction in which they were prior to the exacerbation. In a very few instances only has a perfect remission of the spasm been observed. The exacerbations usually begin by an increase of the pain felt at the sternum. Sometimes there is no obvious exciting cause of their occurrence; but frequently it is evident that they are brought on by exertions of the body; even by slight movements, such as belong to a change of posture, to the attempt at swallowing, or speaking. As the disease advances, these paroxysms of aggravation become more frequent, and a rapid increase in the frequency of their recurrence is one of the most unequivocal signs that the case is severe and dangerous. The more speedily the intervals between the paroxysms shorten, the worse.

It is a curious thing, that the spasm is observed to give way, sometimes at least, and the muscles to be relaxed, during sleep. To be sure, in the severer cases, the patient is seldom able to sleep; and it may be that in the less violent instances, the spasm abates or ceases, and the exhausted sufferer sinks into repose, in consequence of this abatement. However, a similar phenomenon occurs in at least another of these spasmodic diseases, as we shall see hereafter. Mr. Mayo had a boy afflicted with tetanus, in the Middlesex Hospital. On visiting him one day, he found him asleep, and remarked that he lay perfectly relaxed. The abdominal muscles were soft and yielding, and had not the least tension. The boy was awakened, and at the instant the full tension of the muscles returned: not being farther disturbed, he fell asleep again in a few minutes, when the muscles again slackened; and again, upon his being a second time roused, resumed the state of spasm.

In most cases the strong muscles of the back are the *most* affected, and they overcome those on the anterior part of the body; so that sometimes the patient during the paroxysm rests only upon his head and his heels, while his body is raised into the shape of an arch. This form of the complaint is called *opisthotonos*, a bending backwards. The sterno-cleido-mastoid muscles of the neck have been so stretched and misplaced as to become powerful *extensors* of the head. In a few instances the body is bent forwards, so that the head and knees are in contact, and the patient is rolled together like a ball. This is called *emprosthotonos*. In the only example of *emprosthotonos* which I ever saw, these two conditions alternated with one another. The patient was a girl, in Edinburgh, under the care of a friend of mine, who took me to see her. It was rather a case of hysteria than of tetanus; but all at once she would be drawn into a position such, that the top of her head, and her feet, were alone supported on the bed, while her body was bent like a bow; then, after a time, with equal suddenness, the opposite posture was assumed, her forehead and knees being brought together. Still more rarely the body is bent to one side. This is *pleurosthotonos*, or *tetanus lateralis*; and this I never saw. Sometimes again, in the height of the spasm, the antagonist muscles counteract each other exactly; and the head and trunk are rigidly extended: and the term *tetanus* is by some writers confined to this form of the disease. It is called *trismus* when the jaw only is affected.

It is well to know that these varieties occur, and may be looked for; but in all of them—*trismus*, *opisthotonos*, *emprosthotonos*, or *pleurosthotonos*—it is the same disease: and the prognosis is not altered,

any more than the diagnosis, by the variety that happens.

During the fit of exacerbation, the aspect of the sufferer is often frightful. The forehead is corrugated and the brow knit, the orbicularis muscle of the eye rigid, the eyeball motionless and staring, the nostril spread, the corners of the mouth are drawn back, the set teeth exposed, and all the features fixed in a ghastly grin—the true *risus sardonius*. The tongue is apt to get between the teeth, and to be severely bitten.

All the contractions are attended with intense pain. You may form some notion of the severity of this pain, if you have ever been troubled by spasm of the gastrocnemius, or cramp of the leg, and if you can bring your mind to conceive that the same sensation which you then felt in the calf, involves nearly all the voluntary muscles of the body. The pain is worst during the exacerbations, and that which is experienced at the sternum is commonly the most complained of. Even to this, however, there are occasional, though very rare, exceptions. Sir Gilbert Blane has described a case of tetanus, which ran the usual course, and terminated fatally, yet the patient suffered no pain: the sensation excited by the violent muscular contractions was a sort of tingling, of rather a pleasurable kind.

So *violent* are the contractions sometimes, that the teeth have been broken by them. There is one case related in which the thigh-bones were fractured by the forcible action of the femoral muscles; and another in which the psoas muscles were found, after death, to have been torn across. Dr. Latham tells me that he once saw one of the recti muscles, in front of the abdomen, thus rent asunder.

With all this disturbance of the muscular system, there is commonly very little derangement of the other functions of the body. The disorder is almost always attended with obstinate costiveness; partly, perhaps, from the spasmodic closure of the anus, partly, perhaps, in some cases, from the medicines that are given. When stools are obtained they are usually very offensive and unnatural. There is no fever. The pulse and respiration are quickened, and a sweat frequently breaks out, during the exacerbations, from the pain and anxiety then experienced; but this is not the case during the intervals between the paroxysms. In the last stages of the fatal cases, the pulse becomes quick and feeble, and the sweat is cold, as in other instances of approaching dissolution.

What is still more worthy of observation is that the mental functions are unaffected. There is seldom any delirium, or coma, or disturbance of the intellect. These symptoms only appear (if they appear at all) when other indications of the failure of the powers of life come on.

The mode of death, in this disease, seems to be of a mixed nature. Partly it appears to result from apnoea; the thorax being held as in a vice by the spasm of its muscles, and the breathing for a time suspended, or much embarrassed: partly, and chiefly, it occurs from asthenia: the power of the heart flags and is exhausted by the continuance of the suffering, by the fatigue and expenditure consequent upon the muscular action, and by the patient's inability, in many cases, to take sufficient nourishment. When death occurs suddenly, as it sometimes does, in a paroxysm, it is owing, in all probability, to spasm of the respiratory muscles, and perhaps of those of the glottis among the rest.

Most cases of tetanus may be traced to one of two causes: which are, *exposure to cold*, especially to sudden alternations of temperature, and *bodily injuries*. In many instances both these causes cooperate in producing the disease. When it supervenes upon some bodily hurt, it is called *traumatic tetanus*; when it arises spontaneously, or after exposure to cold, it is held to be *idiopathic*. In this country, and I believe in every other, the traumatic variety of the disease is much more common than the spontaneous. But in what manner soever it may originate, tetanus is far more frequent in hot than in temperate climates and seasons. In this case, however, as in so many others, the heat appears to act as a *predisposing* cause only; the exciting cause, in addition to the wound in the traumatic species, being the application of cold (and particularly, according to Hennen, of *cold air in motion*) after the heat, or during the prevalence of hot weather. Thus it is stated that after the battle of Muskau, in the midst of great heats, very few of the French troops were affected with tetanus: whereas those who were wounded in the battle of Dresden, when the weather was cold and wet, just after a very hot season, were decimated by that complaint; which did not spare even those who underwent immediate amputation.

Idiopathic tetanus is extremely rare in this country. Dr. Gregory, of Edinburgh, used to mention in his lectures the case, seen and treated by himself, of a man who, having fallen asleep in moist grass, awoke with a stiff neck, which afterwards went on into regular tetanus. A good example of well-marked tetanus, arising from exposure to cold, is narrated, in the *Edinburgh Medical and Surgical Journal*, by Dr. Hall, of Berwick.

The history of that species of tetanus which occurs in connexion with wounds and injuries presents nothing constant or uniform. The disease is liable to follow hurts of any parts of the body, and of every kind, degree, and extent; from a slight cut or scratch, to a compound fracture, or a severe

surgical operation. It comes on also in various stages and conditions of the injury. Sir James M'Grigor tells us (in the sixth volume of the *Medico-Chirurgical Transactions*) that in the Peninsular war the complaint supervened "in every description, and in every stage of wounds, from the slightest to the most formidable; the healthy and the sloughing; the incised and the lacerated; the most simple and the most complicated." Sometimes, however, the discharge from the wound has been observed to have been remarkably diminished, or suppressed, at the coming on of the tetanic symptoms: and sometimes the wound has healed completely before the commencement of the attack of tetanus. To show you how very trivial the injury may be, how various in kind and in place, I may mention a few instances that have been collected, in illustration of the manner in which this terrible disorder may originate. It has been known to arise from the sticking of a fish-bone in the fauces; from a slight wound of the ear by a musket-shot; from the mere stroke of a whip-lash under the eye, although the skin was not broken; from cutting a corn; from a bite on the finger by a tame sparrow; from the blow of a stick on the neck and on the hand; from a seton in the chest; from the extraction of a tooth; from the injection of a hydrocele; from the operation of cupping.

Nevertheless there are some sorts of injury, and some parts of the body, more frequently than others concerned in the pathogeny of tetanus. The disorder more often supervenes upon injuries of the extremities, than of the trunk, head, or neck; and upon wounds made by puncture, than upon most other hurts. Penetrating wounds in the sole of the foot, such as are not seldom inflicted by treading upon a nail, or a splinter; and laceration, or other violence done to the muscles that constitute the ball of the thumb, are very apt to be followed by tetanic spasm. Some have supposed that the disease has some special connexion with injuries of tendinous parts; but there can be no doubt that it is essentially a malady of the nervous tissue.

The tetanic symptoms occur at no fixed period after the reception of the injury. Professor Robinson, of Edinburgh, was once at table when a negro servant lacerated his thumb by the fracture of a china dish. He was seized with convulsions almost instantly, and died, with tetanic symptoms, in a quarter of an hour. Such a rapid progress as this is, however, quite out of the usual course of the disease: probably fright had something to do with it. Hennen, in his work on *Military Surgery*, states that terror is frequently the immediate antecedent of the attack. In general the tetanus supervenes between the fourth and the fourteenth day

after the infliction of the injury: some time in the second week is the most common period of all. In the Peninsular war it did not commence later than the twenty-second day. In some rare instances its accession is still longer deferred. "Of the nature of the changes that take place in the interval (justly remarks Dr. Alison) we have no information whatever." The longer, however, that the disease delays its assault, in these traumatic cases, after the reception of the local injury, the milder in general does it prove, and the more room is there for hoping that it will end favourably.

When the disorder arises from exposure to cold and damp, it comes on much earlier; often in a few hours. If, for example, the exposure takes place during the night, the complaint may begin to declare itself the next morning.

Although tetanus may be excited by a wound, independantly of any exposure to cold, or by cold without any injury, there is good reason for thinking that, in many instances, one of these causes alone would fail to produce it, while both together call it into action.

After the disease has set in, its rate of progress is various. Almost all writers divide it into acute, and chronic tetanus. But the difference is merely in the degree of severity. When the spasms come on suddenly, recur often from the beginning, and increase in frequency and violence, the chance of recovery is but small. The patient, in these cases, sometimes dies on the second, and generally before the fifth day. If he lives to the ninth day of the disease, his prospect is somewhat better, and the spasmodic symptoms may gradually abate and disappear. Some, however, have died as late as the sixteenth, the twentieth, and even the thirty-fifth day: but this last is very rare.

The idiopathic tetanus, or that which is produced by cold, although it commences earlier, is more generally of a chronic character than the traumatic: that is to say, the spasmodic contractions take place more slowly; and the paroxysms do not increase in violence, and in rapidity of recurrence, as they are apt to do in the symptomatic variety: and accordingly this form of the malady is much oftener, I dare not say cured, but recovered from, than the other.

Diagnosis.—With respect to the diagnosis of tetanus, there is only one point in which it is at all ambiguous or important. There is no other disease that is likely to be confounded with it, except perhaps that extraordinary disease hysteria, which sometimes mimics its phenomena. I have already alluded to one example of this kind that I myself saw. But there is a form of *poisoning* that may easily be mistaken for tetanus.

The symptoms produced by a poisonous dose of strychnia, or its salts, or the vegetables from which it is procured, are the symptoms of tetanus. And as this drug is now readily obtained, and its noxious qualities are well known, it is not unlikely to be made an instrument of suicide, or of murder. It is necessary therefore that you should be acquainted with the effects of this poison, which constitutes the active principle of the *nux vomica*, the *fabæ St. Ignatii*, and the *upas tieuté*. Dr. Christison has excellently well described these effects as they are observable in animals; and I have once, by accident, had an opportunity of witnessing them in the human body. I shall not be wandering from our present subject if I enumerate the symptoms to be expected from a large dose of strychnia; especially as I have lately been advising you to make trial of it as a remedy in certain forms of disease. Dr. Christison, who had made experiments with it upon animals, tells us that the creature "becomes agitated, and trembles, and is then seized with stiffness and starting of the limbs. These symptoms increase, until at length it is attacked with a fit of general spasm, in which the head is bent back, and the spine stiffened, the limbs extended and rigid, and the respiration checked by the fixing of the chest. The fit is then succeeded by an interval of calm, during which the senses are quite entire, or unnaturally acute. But another paroxysm soon sets in, and then another and another, till at length a fit takes place more violent than any before it, and the animal perishes suffocated."

Some time ago I had occasion to prescribe the strychnia for two patients in the Middlesex Hospital, both of whom had paraplegia. I directed one grain to be intimately mixed with crumb of bread, so that it might be divided into twelve pills: and one of these pills, or one-twelfth of a grain of strychnia, was to be taken by each patient every six hours. Unluckily, through mistake or negligence in the person who was at that time the dispenser, a grain of the poison was administered at once to each patient. It was given about seven in the evening. At half-past seven it began to produce its characteristic effect upon one of the patients. He was suddenly seized with tetanic spasms; his legs were separated widely from each other, and rigidly extended: and his head and trunk bent backwards. He was, in fact, in a state of *opisthotonos*. His abdomen was quite hard, and his limbs stiff, even when the violence of the paroxysms abated. He cried out with the pain at the coming on of these spasms. Any attempt at movement, even the touching him by another person, brought them on. This is just what happens in the *disease*. The opening of a door, a sudden current of air, the smallest bodily

effort, the act of swallowing, nay, even the imagination of these influences, will be sufficient to renew the spasmodic tightening of the affected muscles. My patient spoke of a particular sense of constriction all over the abdomen, as if it were drawn in. His intellect was quite unaffected. He had two extremely violent attacks of the kind I have been describing, in which he thought he should have died: and to say the truth I was myself horribly afraid of the same catastrophe. Afterwards, from half-past eight o'clock to between eleven and twelve, he had several slighter and shorter fits. He was left weak and exhausted by them: but he soon recovered. I may as well tell you that his paraplegia was not a whit benefitted by this violent action of the remedy.

You may suppose that when I found one of my patients in this alarming state, I became very anxious to ascertain the condition of the other, who had taken the same quantity of the strychnia, and lay in another ward. He told me that he had been for a short period very dizzy, and had trembled all over; and at the time when I saw him, he had a weight or uneasy sensation at the nape of his neck, which drew his head backwards; and he experienced some difficulty in opening his mouth, and in articulating his words. But he thought these symptoms were diminishing rather than increasing. He was perspiring profusely. It is stated by Dr. Christison that if the spasms do not come on within two hours after the poison was swallowed, the patient is safe. It was more than two hours since this patient had taken the strychnia. I gave him a full dose of purgative medicine, which acted as an emetic: and, after he had vomited, the unpleasant sensations about his head and neck left him.

I scarcely knew what to do with the other patient, in whom the spasms had commenced. There is nothing satisfactorily made out, that I know of, concerning the mode of treating such cases. Of course, if one saw the patient early, and knew what he had swallowed, the first thing to be done would be to procure its evacuation from the stomach. But here it had had full time to get into the circulation: and no emetic could have withdrawn that part of it, at least, which had already found its way into the blood-vessels. When I reached him, though the spasms were strong, they were less violent than they had been, and their violence seemed upon the wane: but they were brought on by any almost the slightest muscular effort, or change of position. I hoped therefore that the most dangerous period was passing off, (and so it turned out) and I was fearful of doing harm by exciting those movements of the body which accompany the act of vomiting. I recollected too that another patient in the hospital, under the care of one of my

colleagues, had once been attacked with opisthotonos after taking half a grain of strychnia; and that brandy and water had been given to him; and that he got well from that time, without having another paroxysm. So I gave my patient some brandy and water; and he seemed the better for it: but whether or no it contributed much to his recovery I cannot be sure.

Now how are we to tell, when we meet with such symptoms as these, whether they are the result of disease, or of poisoning? The symptoms are the symptoms of tetanus; I know of no means of distinguishing them from the symptoms of tetanus caused by exposure to cold, or supervening upon a wound. Dr. Christison has suggested that the cases of fatal poisoning by strychnia that are quickly fatal, are fatal in a shorter time than the disease ever is. But if the case related by Professor Robinson, in which the negro was dead in fifteen minutes, is to be regarded as a genuine instance of tetanus, this distinction, drawn from the rapidity with which the poison kills, will scarcely hold. Again, persons who have taken an overdose of strychnia sometimes survive the tetanic symptoms, but die afterwards from the irritant effects of the poison upon the mucous membrane of the alimentary canal. This we do not observe in the disease. In suspicious or questionable cases, we must look into the history of the patient; enquire whether he were likely to wish to destroy himself; what he last swallowed, and when it was taken; whether he has lately been exposed to the injurious influence of cold, especially to a stream of cold air while he was perspiring; and whether he has recently received any bodily hurt. By a careful investigation of all the circumstances, we shall generally be enabled to decide the true nature of the case; but it is clearly necessary that our eyes should be open to the possibility of a case of poisoning by some of the preparations of strychnia being palmed upon us as a case of natural disease.

The *pathology* of tetanus is undoubtedly obscure: but not more so, I conceive, than that of those nervous diseases in general which produce violent symptoms, and even death itself, without leaving any traces of their operation inscribed upon the dead materials of the body. Nay, it is not so obscure as several others. I think we may fairly come to the conclusion that the symptoms result from irritation of the *spinal cord*, or of its afferent nerves; and that the *brain* is not involved in the disease. The French (at least some of the most modern writers on tetanus) hold that it is always an inflammatory complaint; and that it consists essentially in inflammation of the spinal marrow: and some of them have sought to remedy it by enormous blood-lettings; from

fourteen to fifteen pounds of blood being taken in the course of a few days by one practitioner; and another bleeding his patient eight times, and applying 792 leeches along the course of the spine, and to the epigastrium. But this doctrine of inflammation being at the bottom of every case of tetanus is contradicted by the plainest facts; and the practice founded upon it has been pushed to a most extravagant and absurd extent. Numberless instances occur of inflammation of the spinal cord and its membranes without any tetanus; and equally numerous examples of tetanus have been met with, when no unnatural appearance at all could be discovered within the vertebral canal. I say we must content ourselves with referring the phænomena of the disease to *irritation*, direct or indirect, of the spinal cord; or its nervous appendages: and I am quite proof against all sneers against the alleged vagueness of that term.

If you *irritate*, mechanically, by means of a pair of forceps, the exposed spinal cord of a recently decapitated animal, a turtle for example, you produce spasmodic contraction of the limbs: what difficulty is there in supposing that some mechanical irritation existing within the spinal canal of a living man may have a similar effect? It may be, and probably is, sometimes, the mechanical irritation caused by the altered state of the blood-vessels under inflammation; for we sometimes find traces of such inflammation in the spinal marrow after death by tetanus.

Again, if you irritate, by pinching, one of the spinal nerves of a turtle whose head has just been cut off—if you irritate one of these nerves in any part of its course, what happens? why the muscles of the limbs contract spasmodically; those on the side to which that nerve belongs, and those on the other side also. That property of the cord comes into play which I have so often mentioned: a property which it possesses independantly of the brain; which it evinces when all communication with the brain is cut off; a property, therefore, which may be manifested without any exercise of volition, and even in spite of efforts made by the will to restrain its manifestation: I mean, of course, the property whereby it is capable of receiving impressions through the medium of its afferent nerves, from parts at a distance, and of originating motion in the muscles of the trunk and limbs through the medium of its efferent nerves. By the courtesy of Dr. Marshall Hall I have been afforded the opportunity of witnessing, in the headless turtle, the phænomena that I have been describing to you. Surely they throw a broad light upon the pathology of tetanus, and of some other affections. We infer from them, most legitimately as it seems to me, that the tonic spasm which characterizes the

disease we are considering may be caused by a diseased state of the spinal marrow itself; or by a morbid condition of the nerves that belong to it. In the latter case, irritation is set up at the free extremity, or somewhere in the course, of incident nerves; along these nerves an influence is conducted to the cranio-spinal axis, in which a process or change takes place, whereby an answering influence is reflected to the muscles along motor nerves: and the whole circle of action and reaction is run through with the suddenness and swiftness of lightning, or of thought. You cannot expect that visible marks of the irritating cause should, in all cases, be left upon the body; any more than you could discern the pinch made by the forceps after they were withdrawn.

When, in the experiments to which I have referred, Dr. Hall plucked or compressed one of the denuded spinal nerves, spasmodic motions were excited in the muscles of *both sides*; and *above*, as well as *below*, the junction of that nerve with the cord. This shews that the change (whatever it be) that is wrought in the cord by impressions made upon one of its afferent nerves, is not necessarily confined to the corresponding *segment* of the cord; but may be instantly communicated, in both directions, throughout its entire course: the whole of this centre of the excito-motory system responding to the influence conveyed by a single nerve, as completely as a tight string vibrates from end to end, when struck at any one point. We frequently, indeed, find that the excited motions are more limited: but it is important to mark this ready consent of the whole cord, under sufficient excitement.

Dr. Hall has given certain distinguishing epithets to tetanus, according to the supposed source and locality of the irritation. When the irritating cause operates directly upon the spinal cord itself, he calls the disease *centric* tetanus: when it resides in some part of the body, distant from the spinal cord, he calls the disease *eccentric* tetanus. These are good and intelligible names; and I shall take leave to adopt them.

Observe now how well this explanation meets the facts of the case. We sometimes find the spinal cord or its membranes inflamed, when there has been tetanic spasm. We then refer the spasm to the centric irritation. But in a far greater number of cases we can detect no marks whatever of disease in the spinal canal, but we know that an irritating cause has been applied to parts at a distance: often we have evidence which is visible that a nerve has been injured, torn across perhaps, or half torn, or compressed in some way or other; just as we might compress a nerve, with a pair of forceps, in a decapitated turtle. That experiment shows us that very slight irritation may be enough

to produce the spasmodic action; and we find that slight injuries, as well as severe, will bring on the disease, when, by the operation of certain injurious agencies, the frame has been predisposed, and rendered morbidly susceptible. There is no part of the trunk or limbs which is not supplied with nerves from the spinal cord; and we find that injuries of various parts, or of almost any part, in an individual predisposed to take on the disordered action, will produce it. The exciting cause may be a wound irritating a particular nerve: it may be exposure to cold, acting upon the extremities of various nerves that proceed from the surface: it may be a bundle of worms, irritating the nerves spread upon the mucous tissue of the alimentary canal; for I omitted to state before that some writers, especially MM. Laurent and Lombard, have maintained that tetanus is almost always, even when it supervenes after wounds, the result of the presence of worms in the digestive organs. They have founded this opinion upon the *fact*, that worms have been very frequently indeed found in the stomach or intestines of persons dead of this disorder. I think this is a point well worth attending to. It is objected that worms infest the human body without causing tetanus: but the very same thing may be said of the operation of cold; and of external injuries. Any of these may probably excite the disorder, when the body is preternaturally susceptible of it. The real mystery lies in this predisposition. We have reason to suppose that a high atmospheric temperature continued for some time is *one* predisposing cause; but how it operates, or what is that state of system in which the increased susceptibility consists, these are points concerning which we are really in the dark.

The disease is common enough in the lower animals: and it is frequently *eccentric* in them; brought on by injuries, mostly of the extremities. Locked-jaw is well known in the nosology of farriers. It is not uncommon in the horse after castration. I remember a mare belonging to my father dying of that disease a few days after foaling. Dr. Parry noticed eccentric tetanus in lambs. "I have often seen lambs," says he, "whose ears, for the purpose of marking them, have been bored with a red-hot iron too near the root, so rigid all over with tetanus, alternating with convulsions, that their bodies would project in a right line with their hind legs, when one held them out horizontally by the hind feet." Dr. Mason Good tells us that parrots also are frequently affected with trismus: a calamity which, supposing the bird to be within ear-shot, it would be difficult to commiserate.

We are not advancing any wild theory,

then, respecting the controverted pathology of this disease, when we lay down the following propositions: that it is essentially a disorder of the excito-motory apparatus; that it results from irritation of a peculiar kind, affecting that part of the nervous system; that the irritating cause may be centric, or within the spinal canal itself; and that it may also be, and often is, eccentric, or situated at the extremity or somewhere in the course of one or more of the afferent spinal nerves; and that a certain predisposition of the body is for the most part necessary, to render it susceptible of the disease under the operation of the exciting irritation.

At one time it was supposed that the physical cause of the disease was detected, in the presence of more or fewer thin scales of bony matter, lying in or upon the arachnoid of the cord. I have myself seen these after death preceded by tetanic symptoms. But tetanus often happens and proves fatal without them: and they are often met with when there has been no tetanus. If, therefore, there be any connexion between these thin plates of ossification and the occurrence of tetanus (which may well be questioned) it must be of this kind; that the scales of bony matter predispose the spinal cord, somehow, to be affected by the exciting causes of the disease.

The doctrines recently propounded by Dr. Marshall Hall, of which the importance becomes daily more apparent, and by which his name will be enduringly connected with the physiology of the nervous system, acquire a strong confirmation from the phenomena of tetanus. They furnish a key to many problems which had previously perplexed the pathologist; and they do this simply by distinguishing the proper functions of the two distinct nervous centres; the brain and the spinal cord. But the practical application of these new views is yet in its infancy.

Treatment.—The *treatment* of tetanus is a mortifying subject. The disease is and has always been a lamentably fatal one. Hippocrates says, ἐπι τραυματι σπασμος ἐπιγενομενος, θανασιον, tetanus supervening on a wound, is mortal: and the aphorism holds true, with very few exceptions, in the present day. Almost all the acute and severe traumatic cases are fatal. Hennen declares that he never saw a case of “acute symptomatic tetanus” recover. Dr. Dickson found all curative measures followed by “unqualified disappointment.” Mr. Morgan uses these words: “I have never yet seen or heard of an instance of recovery from acute tetanus. Another of Hippocrates’s aphorisms is, ὁκοσοι υπο τετανου ἀλισκονται εκ τεσσαρσιν ἡμερησιν απολλυνται; they who are seized with tetanus, die within four days: but he adds ἡν δε ταυτας διαφογωσιν ὑγιεις

γινονται; if they get over this period they recover. And to this we can only add now, that those who survive the first few days, and ultimately get well, recover in a variety of different ways, and under various modes of treatment. But as to the mode of treatment which is to be preferred, or even as to the real efficacy of any mode, there is much room for doubt. Under every plan of management a vast majority die.

Let us briefly pass in review the principal remedies that have been tried, and enquire what degree of success has followed their employment.

One drug from which much benefit has been hoped for, is *opium*. In some spasmodic disorders it is of unquestionable service. Very large doses of it have been given and borne in tetanus; and some have recovered under its use, and more have died.

It is well known that pain fortifies the nervous system against the peculiar influence of narcotic substances. We need not, therefore, be surprised that opium, administered in enormous quantities, in this painful disease, has had but little effect. I was assured by a physician, with whom I formed an acquaintance in Edinburgh some years ago, and who is known, I find, to a student now present, that his own wife, while labouring under a tetanic affection, swallowed, in twenty successive days, upwards of 40,000 drops of laudanum, which is at the rate of more than two ounces a day; in all, more than an imperial quart. The lady recovered. A case is recorded in the 2d volume of the *Medico-Chirurgical Transactions*, in which an ounce of *solid* opium was taken, in divided doses, every day, for 22 days. This appears a more astounding instance than the former; but I am not sure that it was so; for, in this complaint, solid opium does not always dissolve in the stomach. I have heard the late Mr. Abernethy say that he had found enough undissolved pills of opium in the stomach after death, to poison a dozen healthy persons. This fact should teach you, if you resolve on trying opium at all, to exhibit it in a liquid form; laudanum, or a solution of the acetate, or of the muriate of morphia. And it would be well, I think, to combine the external use with the internal administration of opium; to blister the spine, and strew the blistered surface with powdered acetate of morphia, while you give it in solution by the mouth.

It is sometimes a difficult matter to introduce medicine by the mouth, so strong is the spasmodic contraction of the muscles that close the jaws. You cannot get the mouth open. Some persons set to work to heave it open, by levers; and it has been proposed, and I believe practised, to break off or extract a tooth or two, to make a

passage for the introduction of medicine and of nourishment; but I hope you will never be guilty of such clumsy barbarity as this: Food, and physic, may be carried into the fauces or into the stomach by means of a flexible tube: and this may be inserted through the nostril; or through the mouth, by passing it between the jaws, behind the back teeth, where there is always an aperture that will admit a sufficiently large tube.

After all, in respect to the cures that have been ascribed to the opiate treatment, they have all (so far as I know) occurred in cases of the milder or more chronic tetanus; and mostly in the idiopathic form of the disease; and this circumstance makes it a question whether they were *cures* at all: whether they were not simply recoveries.

Dr. William Budd (in the paper already referred to) challenges the propriety, on physiological principles, of giving any opium in this disease. He says "it has been ascertained that the effect of that drug is to excite, and not to quiet, the motor function of the spinal cord: indeed, it is well known that the motor acts of the cord may be rendered much more active and powerful, by giving, before decapitation, opium to animals that are to be subjects of experiment." He considers "these objections, furnished by theory, to be motives sufficient for the future exclusion of opium from the treatment of tetanus."

I had long been aware that the effect of opium upon frogs was to produce tetanic spasms. But in no case of poisoning by opium in the human subject (and I have seen a great many) have I ever witnessed any approach to tetanus: and I much question the safety of arguing, in such matters, from what we know to happen in the inferior animals, to what we suppose would happen in man.

The failure, however, of opium in the severer forms of the malady, and its equivocal utility in any, taken together with these theoretical objections, prevent my *recommending* opium as a remedy for tetanus.

Blood-letting.—What is the result of experience in regard to blood-letting in tetanus? I am afraid that, as a curative agent, it has very little power over the disease. Yet it may be, and probably is, of considerable use, as an auxiliary to other measures. When the disorder bears any aspect of inflammation—when, for instance, fever is lighted up, and pain is felt along the course of the spine, or when the approach of the spasm is marked by the supervention or the increase of pain in the wound—our chance of doing good by venesection is the greatest. Some of the cases that happened in the Peninsular war, were decidedly benefited by blood-letting practised under such circumstances. I need scarcely say that

though the bleeding, when adopted, should be early, free, and full, so as to produce some sensible impression upon the system, yet we must always use this remedy with caution. The tendency of the disease is to exhaust the power of the heart; and if by one over-bleeding we bring that organ to a stand-still, it may refuse to begin again.

In a complaint which depends so much on irritation, and so often on manifest irritation of external parts, we look naturally to the *warm bath* for help. And it has been fairly tried: and some persons have found it useful; and others have found it useless, doing neither good nor harm; and some have condemned it as actually hurtful.

The *cold bath* has been extolled as a much more powerful agent than the warm; and so, doubtless, it is. But it is more potent for harm as well as for good. For example: a tetanic patient, in St. Thomas's Hospital, was plunged into a cold bath, at his own request. "All the symptoms disappeared (says Mr. Morgan) in a moment; and he was almost immediately taken out of the bath: but he was taken out lifeless." Sir James M'Grigor says that, during the campaign in Spain, "the warm bath gave only momentary relief; and the cold bath was worse than useless."

However, the application of cold water to the surface has, in many recorded instances, been of at least temporary benefit and comfort: and, in the West Indies, where the disease is common, the cold affusion still continues, I believe, to be the most favourite expedient. After it, the patient is rubbed dry, put to bed, and has laudanum administered. I have again to observe, of this remedy also, that it is chiefly serviceable in the idiopathic form of tetanus. It has been tried upon animals. Dr. Parry says that it was quite unavailing in the case of certain lambs that had the disease. In a note which I made at the time of Mr. Abernethy's lecture on Tetanus, I find the following statement. "The effect of cold in diminishing excessive muscular action was strikingly shewn in the case of a horse belonging to Professor Coleman, which had tetanus. The animal was slung, and carried out of the stable, and laid on the snow, which was then on the ground: and he was covered over with snow also. A horse affected with tetanus is a curious sight. His legs straddle, and become stiff; his ears are pricked up; and his tail sticks out. In this case, on the application of the snow, his ears sunk, his tail became pliant, and the rigidity of his muscles was removed. He was again taken into the stable, and the spasms returned." Mr. Abernethy said, that were he himself the subject of tetanus, he would desire to have the cold affusion tried. If you are willing to assay the same remedy, do not

plunge your patient into a cold bath, but take him out of his bed on an extended sheet, splash him well with cold water, wipe him dry, and place him in another dry bed. This will often, for a time at least, diminish the spasmodic action; and the patient will sometimes sleep comfortably after it.

ON THE SYPHILITIC DISEASES OF THE LUNG.

BY WILLIAM MUNK, M.D.

Physician to the Tower Hamlets Dispensary.

[Concluded from page 182.]

(For the Medical Gazette.)

As secondary symptoms in general may show themselves at very different periods from the primary contamination, so may chronic syphilitic bronchitis originate at short or distant intervals from the original attack. Its order of occurrence, in relation to other secondary phenomena, cannot, I believe, in the present state of our knowledge, be positively indicated: but the observations I have hitherto made tend to show that it is most usually observed late in the series. I have known it take precedence of the affection of the throat; but far more frequently it succeeds to this, the morbid action creeping gradually and slowly along the larynx and trachea into the bronchial tubes. In such cases the attack presents many of the phenomena, and follows much the same course, as common catarrh. There is first the affection of the throat, indicated by change of colour, slight tumefaction, a sense of roughness and pain about the part, with some difficulty of swallowing. The voice may next become somewhat hoarse; there is tickling or irritation in the larynx; frequent, short, dry, teasing cough, and, after a time perhaps, sense of slight heat within the chest, soreness beneath the sternum, and more or less constriction. The character of the cough becomes now either much modified or altogether changed—a circumstance which depends on the persistence or cessation of irritation in the mucous lining of the larynx. Should it continue, the symptoms depending thereon are combined with those to which the bronchial affection gives rise, and are, according to their severity, proportionally modified; the cough, though

still frequent, short, and teasing, is from time to time more or less bronchial in its character, is attended with expectoration, seems to implicate the whole chest, is deeper toned, more sonorous, and preceded by a tolerably full inspiration. Should the laryngeal irritation entirely subside, the first-named characteristics of the cough cease, the last-mentioned becoming predominant and ultimately existing alone. This, however, is but rarely the case; for the erythematous condition of the larynx commonly continues, and, giving rise to short and hacking cough, with occasional attacks of a bronchial nature, together present that resemblance to phthisis from whence has originated the mistaken diagnosis before alluded to.

From the inner membrane of the air-passages, when once affected, a secretion is soon poured forth; from the larynx at first of a tough clear mucus, expelled with difficulty; from the bronchiæ, of a thinner, but, nevertheless, tenacious fluid, abundant in quantity, and, by its presence, obstructing the tubes, giving rise to wheezing and some shortness of breath. Its physical properties eventually alter; it loses, more or less rapidly and completely, the mucous character, becoming abundant, purulent, and diffuent. This condition of the expectoration, when the attack is fairly established, has been alluded to by most writers. A brief continuance of this abundant purulent expectoration suffices to produce hectic, even if hectic of a syphilitic character has not already appeared. Commonly, however, some traces of feverish disease will be detected ere the expectoration arrives at this stage. In either case, indeed, it will be found that, upon the supervention of purulent expectoration, symptoms of a hectic type will appear, or, if already present, be materially aggravated.

In the sketch I have now given of the progress of this disease, I have taken, as an example, a well-marked case running a somewhat rapid course. In the majority of instances it will be found that the symptoms depending upon laryngeal or tracheal irritation persist for some considerable time alone, and are only gradually and slowly combined with those of bronchial inflammation.

From my own observation I can say

but little regarding the morbid appearances presented by the bronchial membrane when affected with syphilitic inflammation. I am glad, therefore, upon this point to be enabled to adduce the opinions of a gentleman of great experience, whose attention has for some years been given in an especial manner to the effects of syphilis upon the pulmonary tissues. My friend Dr. P. D. Sadowski, in a communication with which he has favoured me, dated Prague, July 18, 1840, states, in reply to some inquiries, that a very common consequence of syphilitic bronchitis is ulceration of the membrane; that the individual ulcers are small in size, but exceedingly numerous, and may, in some instances, be detected over the greater portion of the membrane, even in the smallest tubes. The following case, which proved fatal in September, 1839, and was under my care for the last six weeks of the patient's life, is confirmatory of the opinion above expressed. A young gentleman, then nineteen years of age, contracted syphilis in Paris during the summer of 1837. He was then commencing a tour which it was intended should extend over a great part of the continent, in company with his parents and two sisters. A natural desire for secresy, and a somewhat rapid change from place to place, were obstacles to efficient treatment and proper medical superintendence. He took mercury, but did so both irregularly and inefficiently. The primary symptoms subsided, and on reaching Vienna, where he wintered, he underwent a course of sarsaparilla. By this he was much benefitted; but, in the autumn of 1838, being then in the North of Italy, was attacked with sore-throat. To this there supervened hoarseness, ulceration of the soft palate, cough, a copper-coloured eruption on the skin, nodes, nocturnal pains, and heavy perspirations. Matters became gradually worse, and I saw him for the first time in August, 1839, about three months after his return to England. He was then confined to bed, was exceedingly emaciated and debilitated, had frequent and distressing cough, copious purulent expectoration, together with symptoms which left no doubt of the existence of laryngeal disease. The chest was perfectly sonorous; but an abundant coarse mucous rhonchus was evident over the

whole lung. He was ordered quinine and iodide of potassium, with anodynes, at night, but sank perfectly exhausted on the 15th of September. Numerous minute ulcerations existed on the mucous lining of the larynx; there were none in the trachea; but below the bifurcation they again appeared, becoming more and more numerous the smaller the tubes. In the smallest of the bronchiæ examined there appeared one continuous sheet of ulceration, the individual ulcers having, as it seemed, run one into the other. The bronchiæ were filled with purulent matter, and the inferior lobes of the lung were slightly congested.

The action of syphilis on the pulmonary organs is sometimes evidenced in the form of pneumonia. Of this I have now seen two interesting examples, and some of the recorded cases to which I have above alluded would certainly appear to be of this nature. The case of a soldier, "*Ex male curata lue venerea phthisicus factus*," detailed in the *Acta Medicorum Berolinensium*, was evidently of this kind. In thorace (writes the reporter) "*dexter pulmonum lobus maximam partem erat consumptus pars residua pure scatebat cujus etiam magna quantitas in cavitate hujus lateris apparebat. Sinister pulmonum lobus pleuræ erat adnatus substantia ejus extima apparebit integra interior autem vesiculosa substantia erat purulenta et hinc inde vomicis pure repletis obsita* *."

In the cases of syphilitic pneumonia which have fallen under my observation, both the local and general symptoms have been more severe than in the ordinary run of cases of syphilitic bronchitis. There has been more marked disturbance of respiration, an expression of countenance indicative of considerable distress, with persistent feverishness; among the symptoms of which heat of the surface has been very prominently marked.

The origin and early progress of these cases I have had no opportunity of watching, the disease, in the only two instances I have seen, having

* No mention is made in this case of the existence of tubercles; whilst in the narration next ensuing their presence is particularly referred to. From this circumstance it seems legitimate to conclude that they were not present in the case quoted in the text, and, consequently, that the disease under which the patient laboured was truly pneumonic in character.

existed for some weeks previously to coming under my care. Respiration has in both cases been considerably quickened—a condition which was much increased both by corporeal and mental excitement; there has been frequent cough, scanty and viscid expectoration, pains in the sides and back, worse during the early part of the night, increased by pressure, and having altogether very much of the character of periostitis of the ribs. In one of the cases there were attacks of a spasmodic character, occurring generally during the first sleep, and, in many other respects, imitating asthma. The pulse, in both instances, was small, frequent, and quick; the skin hot, dry, and harsh; and there were partial sweats at night, occurring more particularly about the chest. The physical signs observed were the crepitant rattle, dullness on percussion, absence of respiratory murmur, blowing respiration, and bronchophony. One circumstance has struck me as curious in these attacks, and as well worthy of remark; namely, the length of time during which the crepitant rhonchus may continue, without giving place to solidification and its attendant physical phenomena. I shall proceed now briefly to detail the two cases from which the above description has been drawn.

A man, aged thirty-one, had, when he came under my care (Jan. 1839), been suffering more or less for upwards of three months from cough, difficult expectoration, hurried breathing, much increased by exercise, feeling of tightness and heat within the chest, together with severe pains of the back and sides, worse at night, and much increased by pressure upon the ribs. There was a good deal of feverishness, the pulse being 96, small, and quick; the skin generally hot and dry, but moist upon the chest; the tongue yellowish, white, and furred. A copper-coloured scaly eruption existed on the forehead, and over other parts of the body, and there was a dark redness of the throat, with slight ulceration. The lower part of the chest posteriorly sounded dull upon percussion, and, corresponding to this, there was bronchial respiration and bronchophony. Higher up there existed a well-marked crepitant rhonchus, more extensively diffused over the right than the left side, but equally distinct in both, and giving way gra-

dually to the signs of solidification. This man had a rather severe attack of syphilis twelve months previously; for which he was mercurialized by a surgeon in Liverpool. In August the eruption made its appearance, and, towards the latter end of September, the thoracic symptoms, without, however, his being able to attribute them to any evident external cause. These had precedence for about two months of the affection of the throat. He was cupped between the blade-bones, took two or three saline aperients, and was then ordered blue pill and opium. A good deal of constitutional disturbance was the result, and, although mercury was ordered in different forms and variously combined, its employment was productive of more evil than good, the pulmonary affection being unalleviated; while the bowels were much disordered, and the whole system thrown into an irritable state. Five-grain doses of iodide of potassium, with tincture of hop, in decoction of sarsaparilla, were now given three times a day, and the compound soap-pill at bed-time. Within a fortnight there was an evident improvement; and in about ten weeks from the commencement of this plan he was perfectly recovered: the general symptoms, no less than the physical signs, having entirely subsided.

The next case occurred in a young man, aged twenty-five; and the symptoms under which he laboured were so nearly like those in the former patient, that it is unnecessary to take up the time of the reader by detailing them. The pulmonary symptoms had existed for about six weeks; and the physical signs consisted of a well-marked crepitant rhonchus, heard in the lower lobe of the left side, and in the middle of the chest posteriorly on the right side, the upper and lowermost portions being free. Bronchophony and bronchial respiration were no where detectable. He experienced nearly every night, sometimes after lying down in bed, sometimes after an hour or more's sleep, and from which he was thus aroused, an urgent sense of constriction and tightness across the chest, with difficulty of breathing and a feeling of suffocation. These symptoms, after a short continuance, subsided; and the remainder of the night was passed in quiet and placid sleep. This patient

had suffered from syphilis eight months previously; and the pulmonary symptoms had arisen about the same time, with nodes upon the shin. He took iodide of potassium, an antispasmodic draught at night, and rubbed mercurial ointment along the spine of the tibia. Under this treatment he improved slowly, but gradually; the crepitant rhonchus diminished, and its place was at length supplied by a healthy respiratory murmur.

The anatomical changes effected in the pulmonary tissues, by the long continuance of syphilitic pneumonia, would seem, from the observations of Professor Van der Kolk, to consist in suppuration, and the formation of abscess. "*Etenim non raro,*" says he, "*in perscrutando cadavera siphilicorum qui dum vivebant, phthisici videbantur, inveni in pulmonibus præcipue in medio lobo ulcus quoddam seu pus collectum sine ullo tuberculo cingente, ita ut pus quodam cavo contineretur neque membrana cingens aut induratio posset distingui; in ejusmodi pulmonibus nulla pleuræ pulmonalis exsudatio erat ut externe et tactu etiam pulmones sanissimos crederes; nisi saltem ulcus nimium adesset, nunquam in lobo superiori pulmonum quod in phthisi purulenta semper fere accidit, has ulcerationes inveni sed non longe ab initio bronchiarum, ita tamen ut medium pulmonis parenchyma occuparet.*" —P. 130.

It may perhaps be a matter deserving of inquiry, whether the termination of pneumonia in abscess—a subject upon which there has been much and varied discussion—is not, in many cases, to be attributed to syphilitic pneumonia, or to pneumonia occurring from common causes in an individual contaminated with the syphilitic poison. The opinions of Laennec, Broussais, and Andral, respecting the extreme rarity of this termination, and the statement made by so high an authority as the last-mentioned pathologist, of the facility with which all the appearances of abscess are produced by any thing approximating to roughness or carelessness in the examination after death, have tended, in a very considerable degree, to divert attention from the occurrence in question, and to shake belief in the probability of its existence. The observations of Schroeder van der Kolk may,

perhaps, in the opinions of some, seem obnoxious to the criticisms of Laennec and Andral; but those who, like myself, know the untiring industry with which the Utrecht professor devotes himself to pathological anatomy, and the practical acumen which he evinces in the investigation of morbid structures, will receive with the utmost deference all statements emanating from his pen. These, with the more recent observations of Dr. Stokes, will, perhaps, in some degree, tend to remove the scepticism now existing in reference to abscess as a result of pneumonia.

In the communication from Dr. Sadowski, to which I have before referred, I find the following remarks:—"Does lues ever attack the pleura? I had under my care," says he, "a few years since, a man who was dying from syphilis. He had lost his palate, portions of bone had come away from the nose, and there was venereal necrosis of one tibia. He had for some weeks complained a good deal of his chest; of acute pains at various parts, increased by inspiration, and by pressure between the ribs. He coughed a good deal, but had little or no expectoration. Upon examination after death there was found in the right side of the chest a considerable quantity of sero-purulent fluid. The pleura presented, at different parts, elevations of a circular or oval shape, like blebs, but less prominent, varying in size from half to nearly two inches in diameter. Of these elevations there were nine: they were caused by the effusion of a thick yellowish honey-like fluid beneath the pleura. A similar appearance has been mentioned to me, by a physician upon whose accuracy I can rely, as having been observed by him when conducting the post-mortem examination of a woman who died suddenly, and of whose history nothing satisfactory could be learnt; but who, the autopsy fully proved, was, at the time of her disease, suffering severely from secondary lues."

The circumstances which, in particular cases, determine syphilitic disease towards the respiratory organs, remain yet to be discovered. Baglivi, in treating of this point, says "*pro varietate conditionis, et status personarum lues gallica variis in locis sedem figit, eosque præ aliis vehementius molestat. Musici ob exercitatos cantu pulmones et ob id*

nimum laxatos, lue gallica pulmones perpetuò ludente divexantur*. These observations, which were doubtlessly penned in reference to syphilitic diseases of the larynx, will, *mutatis mutandis*, apply with equal force to the bronchial tubes and pulmonary parenchyma; and, when carrying out the general principle, of which his statement is but an illustration, we affirm that congenital or hereditary liability to pulmonary disorder, previous or existing disease in these organs, will be likely to direct the ravages of secondary lues to the lungs, we keep within the bounds of analogy as regards other morbid poisons, although we exceed the very narrow limits within which actual observation would confine us. A question of high practical importance, well deserving of inquiry, is, whether pulmonary disease, produced by common causes, in an individual labouring under secondary syphilis, is thereby modified;—whether an appreciable character is impressed upon it, altering its course and consequences, and rendering necessary a different line of treatment?

Dr. Graves, addressing his pupils of the Meath Hospital, on the subject of syphilitic pulmonary disease, asks,—“How are you to recognize it? Mainly (he says) by the history of the disease. If the patient’s sufferings have commenced at a period of time after primary sores on the genitals, when secondary symptoms usually make their appearance; if some of his complaints are clearly traceable to this source; if, along with debility, night sweats, emaciation, nervous irritability, and broken rest at night, we find cough; and if this group of symptoms have associated themselves with others, evidently syphilitic, such as periostitis, sore throat, and eruption on the skin, then may we with confidence refer all to the same origin, and may look upon the patient as labouring under a syphilitic cachexy affecting the lungs as well as the other parts.” In this quotation we have a pretty full and very graphic sketch of the circumstances directing diagnosis; yet in the enumeration there is omitted one phenomenon which has been present without exception in all the cases I have witnessed. I allude to the general aspect of the patient, which is often so

peculiar as to declare at once the nature of the disease, or at least to show that the pulmonary affection, whatever be its original cause, is combined with, and its outward indices modified by, the general syphilitic contamination. There is a melancholy and shrunken appearance of countenance; dull, dark, muddy complexion; loss of flesh, particularly manifested in the face, and giving rise to a sunk, haggard, emaciated look. The attitude and manner of the patient, even at rest, are indicative of a want of energy and lassitude; and the actual loss of strength, and consequent incapacity for exertion, is greater than can be accounted for by the loss of flesh. Notwithstanding the presence of all these symptoms, in forming the diagnosis much caution and care are necessary: “we must not draw our conclusion until we have repeatedly examined the chest by means of auscultation and percussion; if these fail to detect any tangible signs of tubercles, we may then proceed to act upon our decision with greater confidence, and may advise a sufficient, but cautious, use of mercury.”

This brings us naturally to the treatment most applicable to the cases in question. Particular cases, according to the difference of their symptoms, demand of course corresponding modifications of treatment; but the point to be now discussed is the relative advantages of the three great remedies of secondary syphilis; to wit, mercury, iodide of potassium, and sarsaparilla; for, in the words of Morton, “ad curationem hujus phthiseos perficiendam omnem merè thoracica et pulmonaria medicamenta sine admixtione antivenereorum nequicquam valere frequenti experientia fretus assero.”

The great armamentarium of almost all the authorities above mentioned is mercury. Morton thus strongly expresses himself:—“Nihil melius excogitari possit quam ulcerum pulmonarium sanationem et crasis sanguinis à fermento venereo eversa restitutionem mercuriali aliqua salivatione tentare. By Dr. Sims it was considered as being the “sole refuge” in this disease, “which never failed performing a cure when taken before the lungs had inflamed and suppurated.” Dr. Graves, it has been shown, advocates no less the employment of this medi-

* Opera Omnia Medico practica et anatomica, 4to. Ludg. 1714. p. 97.

cine. "Under its use," says he, "it is most pleasing to observe the speedy improvement in the patient's looks and symptoms: the fever, night sweats, and watchfulness diminish; he begins to get flesh and strength; and, with the symptoms of lues, the cough and pectoral affection disappear." From mercury I, too, have witnessed most rapid and satisfactory recoveries; but, as regards its employment, my experience has been far less extensive than with the iodide of potassium. I am of opinion that the same rules which guide the practitioner in his choice of mercury or iodide of potassium, as the most appropriate medicine whereon to rely in the treatment of secondary diseases in general, will best direct him in his selection for the cure of syphilitic pulmonary disease; and these being now pretty universally known, need not be dilated on in this place. I would, however, here throw out one suggestion, that mercury should be given only "*priusquam vires a longa morbi protractione jam fractæ fuerint.*"

Accumulated, and yet accumulating, experience attests the value of iodide of potassium in secondary syphilis; and I think it will in none be found more strikingly beneficial than in the cases under discussion. The encomium passed by Dr. Graves on mercury might, I think, with equal justice be awarded to this medicine. It is free from the injurious effects which occasionally result from mercury, and, although not invariably applicable, is, in my belief, taken altogether, a more valuable remedy.

With mercury, or with iodide of potassium, I have generally combined sarsaparilla. The soothing effect of this medicine upon the nervous system, apart from its specific influence, appears to me less generally admitted than it deserves to be. Recognizing its extreme value, as well in primary as secondary syphilis, when given in combination with other medicines, I almost invariably employ it. To those sceptical regarding its virtues I would say of it, in reference to syphilitic pulmonary disease, as did Dr. William Hunter of a small bleeding in threatened miscarriage, "it can do no harm, it may do good, and it ought, therefore, never to be omitted."

?, Finsbury Place South,
April 13th, 1841.

ON THE FORMATION OF ARTIFICIAL ANUS IN ADULTS,

FOR THE RELIEF OF RETENTION OF THE
FÆCES.

BY JOHN ERICHSEN, ESQ.

[Concluded from p. 192.]

(For the London Medical Gazette.)

THE following are the cases which require the formation of an artificial anus:—

1st. Stercoral tympanitis produced by obstruction of the rectum, or of the sigmoid flexure of the colon. Whether this obstruction be the consequence of disease of the intestine itself, or of any of the neighbouring parts, provided we cannot overcome the obstacle from below, and that life consequently is in danger, an artificial anus must be established.

2d. Simple retention of the fæces producing a stercoral tympanitis, which cannot be relieved, and which endangers life.

3d. Scirrhus affections of the rectum, as soon as there is much difficulty in defecation. The establishment of an artificial anus is, in these cases, the only means of retarding the progress of the disease.

4th. Imperforate anus, or rather the absence of a portion of the rectum, when the passage cannot be established in its natural situation.

Amussat states that during his practice he has seen at least seven or eight patients die of stercoral tympanitis, in consequence of his not having dared, in accordance with the views of most surgeons, to open the obstructed bowel.

It was whilst attending Broussais, who died of obstruction to the passage of the fæces, produced by a scirrhus disease of the rectum, that Amussat was led to reflect on the best means of relief in similar cases, and he determined upon putting his project, of opening the colon from the left lumbar region, into execution, as soon as he had an opportunity of so doing; and for this he had not to wait long, as two cases soon occurred to him which fully justified the performance of this operation.

CASE I.—Madame Dubois, aged 48, of a sanguineo-nervous temperament,

had been for several years subject to constipation, her stools being painful, and occurring at intervals of seven or eight days. She has been in the constant habit of using clysters. She always had the appearance of good health, and all her functions were well performed, with the exception of the act of defecation. About the beginning of May, 1839, she was attacked with most obstinate constipation; clysters, baths, and the most powerful purgatives, failed in producing any effect. M. Amussat, who was called in on the 29th, examined the rectum, with a view to remove hardened fæces, which he thought were probably lodged there, but he found the intestine empty. At this time the patient suffered dreadful pains in the bowels, and uttered the most piercing cries, as if she were in labour. There had been no alvine evacuation for twenty-six days, but the desire to go to stool was incessant. Amussat again examined the rectum more carefully than before, and, after a good deal of difficulty, found that, at the upper part of this gut, there existed a round hard tumor, about the size of an orange, and not very moveable; it appeared to be connected with the anterior wall of the sacrum, and completely obstructed all passage through the rectum. The mechanical nature of the obstacle being thus ascertained, it was determined to operate. Accordingly, the 2d of June was fixed for the day of operation, at which Amussat invited me to attend. At this time the patient was labouring under the following symptoms, which closely resembled those of strangulated hernia. She had nausea, with almost constant vomiting and hiccough; the abdomen was painful on pressure, and enormously distended, its circumference being nearly doubled; the face was red and injected; thirst great; anxiety extreme; and she uttered the most piercing cries, calling out for some operation for her relief.

The patient having been laid on her face, with the body inclined somewhat to the right, two or three cushions were placed under the abdomen, so as to raise the left lumbar region. The operation was then performed in the manner that has already been fully described. When the colon, which was reached without any difficulty, was punctured, an immense quantity of gas

and liquid fæces spirted out in a jet; there being sufficient to fill three large wash-hand basins. Injections of warm water were then made in both directions along the intestine, and brought away a large quantity of more consistent fæces, which were covered with a thick mucus: the intestine was then drawn forward by three spring artery forceps, by which the sides of the incision in it had been held open whilst its contents were discharging, and fixed to the sides of the incision in the skin, by means of four points of interrupted suture: only two small arterial branches were divided, the hæmorrhage from which was readily arrested by torsion*. The patient expressed herself greatly relieved immediately after the operation, and said that she felt more comfortable than she had done for several weeks previously. She was then cleaned and put to bed, a poultice having been applied to the wound.

In the after-treatment nothing of consequence occurred; on the 13th of July she was sufficiently well to be able to go into the country. The stools, which were at first glairy, liquid, and abundant, soon became more consistent and regular. The fæces, which are figured, are readily retained by a compress and bandage, so that the patient is not in the least offensive either to herself or to her friends. There is an interesting fact connected with this case, which throws some light upon the physiology of the rectum, namely, that the motions, although figured, and having the same appearance as those which were voided by the natural passage, have little or no fæcal odour.

Five months after the operation the patient was in an extremely good state of health; her complexion was clear, and she had not the appearance of one suffering from any malignant disease; so that probably the tumor in the pelvis, which appears in every respect to be stationary, is of a simple nature. The appetite is better than before the operation, probably because digestion is more rapid, and because

* I may mention incidentally, that Amussat has informed me that he has employed torsion of the femoral artery seven times in amputations of the thigh; of the axillary once, in amputation at the shoulder-joint, and of the brachial, several times, in amputation of the arm, with complete success.

the excrementitious matters remain a shorter time in the intestines.

The inconvenience of the artificial anus is very trifling; much less so than would, *à priori*, be supposed: when the patient forces, or makes any effort, the mucous membrane has a tendency to protrude: this, however, can easily be returned, by means of slight pressure exerted by a compress and bandage.

CASE II.—Mr. T., ætat. 62, has for several years suffered from painful and difficult defecation, the fæces being very foetid, and mixed with purulent and ichorous matters. This state having continued for three years, he became exceedingly emaciated, and when the stools came away he felt excessively exhausted and faint.

On examination, it was found that he had a carcinomatous affection of the upper part of the rectum, consisting of scirrhus vegetations, arising from the inner surface of that intestine, and nearly blocking up its cavity. The finger could be made to pass into the constricted part, but could not reach its upper termination; below there was a prominent ring, feeling like the neck of the uterus, largely opened and cancerous.

It was determined, in consultation, to attempt to break down the tumor: this was done with long forceps, which crushed and pinched its most prominent part; cold water was then injected, in order to keep down inflammation: this was repeated several times, and a considerable portion of the tumor was thus removed. Cauterizations with caustic potass were then had recourse to, and were repeated several times, at intervals of three or four days; by these means the tumor was diminished to half its original size. However, the patient's condition grew most alarming; he had no stools for twelve days, and then with such violence as to lower him till he fainted. His emaciation was extreme, and the symptoms altogether so serious, that, in order to save him from a speedy death, it became necessary to adopt any resource that surgery might offer. The operation for artificial anus, which had lately been performed on the preceding case with success, suggested itself as presenting a double object—namely, in the first place, to relieve the retention of the fæces, and secondly,

to prevent their injurious action on the diseased intestine.

Accordingly, on the 14th of July, 1839, the patient not having had a stool for eight days, the operation was performed. It was begun in the way that has already been described; but when the operator had dissected through the cellulo-adipose tissue covering the peritoneum and intestines, he found that he was too much to the outer side, and that the colon, being strongly retracted upon itself, was concealed under the quadratus lumborum: it thus became necessary to cut across some of the fibres of this muscle, and then, the intestine having been seized with the usual precautions, which have been already described, was incised in the posterior third of its circumference: nothing came away at first but gases, and a few balls of fæcal matter, and it was not until the fourth day after the operation that the contents of the bowels were freely discharged through the artificial anus. As the aperture in the lumbar region evinced some disposition to close, it was necessary to keep it open with tents and sponges. The health of the patient gradually improved, and in a month after the operation he was able to leave for the country. His appearance was at this time greatly changed for the better; his complexion had become clearer, his appetite had returned, and all the functions were well performed; there was no longer any tympanitis, or forced retention of the fæces, regular and figured motions being passed by the artificial anus. The disease of the rectum had somewhat improved, having lost its granular and vegetative feel; if, however, it should retrograde, it will not, as long as it is confined to the rectum, compromise the life of the patient, but if it extend beyond these limits, of course his condition will be altered.

The opening of the artificial anus is now (four months after the operation) rounded, and the mucous membrane is deeply seated: this opening has a great tendency to contract, but it can readily be dilated by tents and prepared sponges.

It is usually said that an artificial anus is a very disgusting and loathsome infirmity; but it is necessary to distinguish between the different kinds

of artificial anus, and also to take into account their situation; for it is evident that an accidental artificial anus, the consequence of a hernia or of a wound, being generally situated in the small intestines, the results of defecation will, in such a case, be very different from those that are observed when an artificial anus is established in the large intestine; so, likewise, an

artificial anus, situated towards the front of the body, will be more disgusting than one situated posteriorly. In the two patients whose cases have been just related, a simple bandage and compress were fully sufficient to retain most completely the contents of the bowels, which were solid, and possessed but little odour.

Table of the Eight Operations for Artificial Anus on Adults.

Date of Operation.	Name of Operator.	Sex.	Age.	Nature of the Case.	Nature of the Operation.	Result.
1776	Pillore.	Man.		Stricture of the rectum.	Incision in right iliac region to reach the cæcum.	Death 28 days after the operation.
1797	Fine.	Woman.	63	Scirrhus of the rectum.	In the umbilical region.	Death in 3½ months.
1818	Freer.	Man.	47	Stricture of the rectum.	Littre's operation.	Death in 8 days.
1820	Pring.	Woman.	64	Stricture of the rectum.	Littre's.	Was living 5 or 6 months after the operation.
1824	Martland.	Man.	44	Stricture of the rectum.	Littre's.	Was living a year after the operation.
1839	Amussat.	Woman.	48	Obstruction of the rectum from a tumor in the pelvis.	Callisen's, modified.	Living.
1839	Amussat.	Man.	62	Scirrhus of the rectum.	Callisen's, modified.	Living.
1839	Velpeau.	Woman.	70	Scirrhus of the rectum.	Littre's.	Death in 2 days.

It will be seen by the accompanying table, that eight operations for artificial anus have been performed on the adult. Although Littre had, in 1710, proposed opening the intestines in cases of obstruction of the rectum, it was not until the year 1776 that his proposal was carried out by Pillore, who was the first to perform any operation of this kind. It will also be seen by the table, that four men and four women have been the subjects of operation: in all of them the disease for which the operation was performed was an obstruction of the upper part of the rectum, or of the lower end of the sigmoid flexure of the colon, probably of a scirrhus nature.

With regard to the operative proceedings that have been employed:—Pillore opened the cæcum from the right iliac region. Freer, Pring, and Martland, adopted Littre's method, as modified by Duret, namely, opening

the small intestine from the left iliac region. Fine, of Gèneva, opened the colon immediately below the umbilicus. Velpeau performed Littre's operation, somewhat modified by himself; and Amussat, in his two cases, opened the colon from behind, in the left lumbar region.

Thus, of the eight operations for artificial anus performed on the adult, we find that in six the peritoneum was opened: of these six cases, three died of peritonitis, in consequence of the operation; whilst the two cases in which the peritoneum was not wounded have been completely successful. Although these numbers are too small to warrant any general deductions, they are nevertheless highly satisfactory.

The following are some of the conclusions that Amussat comes to:—

Ist. The idea of opening the colon in the left lumbar region is not new; but the unsuccessful trials of Callisen

and of Duret on the bodies of children had caused it to be rejected.

2d. Trials on the dead body carefully conducted, prove, however, that the operation is practicable, simple and easy of performance, and that it is founded on positive anatomical facts.

3d. The transverse incision which Amussat adopts in preference to the longitudinal one of Callisen facilitates the operation, and allows us to establish the artificial anus somewhat to the side, and not altogether behind. It thus possesses the triple advantage of facilitating the search for the intestine, of allowing the anus to be made a little more to the side, the patient being thus better able to adjust any retentive apparatus that he may employ, and of enabling the surgeon to avoid wounding the peritoneum.

4th. It is of the greatest importance that the intestine, after having been opened, should be drawn well forwards and firmly fixed to the incision in the skin, by means of sutures, so as to prevent effusions into the loose cellular tissue of the wound.

5th. The artificial anus is a much less disgusting infirmity than would *à priori* be supposed.

6th. Reasoning and fact prove incontestibly, that the preference should be given to Callisen's operation as modified by Amussat.

ON CONGENITAL OPACITY OF THE CORNEA.

To the Editor of the Medical Gazette.

SIR,

WHILE on a recent visit to Aberdeenshire, I was requested by Mr. Proctor, surgeon, Towie, to visit, along with him, a child of three months of age who had been born blind. On examining the eyes the corneæ were found to be of a pearly or bluish-white colour, deeper and more opaque at their centres than their circumferences. The irides were indistinctly seen through them, and, although the pupils were dilated and irregular in form, it was evident from their sluggish motions that they were sensible to the stimulus of light. Both eyes appeared to be small, flat, and *undeveloped*, and were in a continued state of oscillation. Mr. Proctor and the mother of the child

said they were then considerably clearer than they were at birth. The eyes and their appendages were perfectly healthy, and stated to have been so since birth, at which time the opacity was first discovered by the nurse.

Mr. S. Crompton, in the *MEDICAL GAZETTE* of the 12th of December last, records several instances of congenital opacity of the cornea that have occurred both in his own practice and in that of others, particularly in that of Mr. S. Farar, of Deptford; to the two first of which that above related agrees in almost all particulars.

Mr. Middlemore, of Birmingham, it would appear, doubts the occurrence of congenital opacity of the cornea, but the cases now on record, although few in number, leave no question as to the possibility of such an event, and the veracity of Messrs. Crompton, Walker, and Barton*, cannot be called in question, however it may run counter to the preconceived notions of Mr. Middlemore, high as is his authority in ophthalmic surgery. Assuming, then, that congenital opacity of the cornea may, and has more than once existed beyond the possibility of doubt, it appears to me that its occurrence may be explained by three causes, all different in their nature and effects. In the two cases recorded by Mr. Crompton, as occurring under his own eye, Messrs. Allen and Barton agree with him in regarding the eye as in an *undeveloped* state, and in the instance which I have related it was one of the most obvious conditions. Mr. Farar, however, does not say whether in the instances that fell under his observation the eyes were in this state; he merely notices the opacity of the cornea; but from their similarity to those already spoken of, so far as can be learned from his paper as quoted by Mr. Crompton, it is not unfair to infer that they were of the same nature; the opacity originating in the same cause—arrested development. The fœtus, we know, is subject to many diseases in utero, and arrest of development of one or other of its organs is a matter of every-day observation. On what grounds, then, can the eye be exempted from the same laws as frequently influence the other organs of the body? and if this is granted, why deny its

* Vide Mr. Crompton's paper in *MED. GAZ.* Dec. 12, 1840.

influence, or reject it when obvious as a sufficient cause of congenital opacity of the cornea?

Mr. Crompton also quotes a case from Mr. Walker, of Manchester, in which, on the second or third day after birth, the corneæ were found to be "opaque throughout, and unusually large and prominent, so that very little of the sclerotic was discernible. The opacity was of a bluish white colour: there was scarcely any irritation about either eye: nothing like inflammation;" but in the second year the corneæ were "perfectly transparent, and of normal size." It is at once obvious in this case, although there were opaque corneæ, that the condition of the eye was different from that in the cases already commented on. And, if together with the appearance of the eyes, we take the satisfactory progress of the case, so that in two years complete vision was restored, we are led to conclude that the opacity resulted from a redundancy of the aqueous humour, not from congenital arrestment of the development of the eyes.

The due amount of the aqueous humour evidently depends on a happy balance being struck between the functions of its secreting and absorbing organs; hence the *rationale* of the production of the opacity in this instance is easily understood. The function of absorption of this humour being impaired, whilst that of its secretion continued normal, opacity of the cornea, from over distension of the anterior chamber, was the result.

Again, in another quotation from Mr. Walker given by Mr. Crompton, a case is detailed where there is every reason to believe that purulent ophthalmia, ending in destruction of one eye, and great impairment of vision in the other, had run its course in utero. This, although hitherto an unnoticed occurrence, cannot be denied, resting as it does on so respectable evidence as that of Messrs. Crompton and Walker. Intra-uterine pathology is but in its infancy, yet the researches of Graetzer, Simpson, and others, have revealed enough to make us cease to wonder, as year after year witnesses new contributions to this department of pathology, which overturn and root out opinions and dogmas that have "grown with our growth and strengthened with our mind."

From what has been said I would infer, as already stated, that congenital opacity of the cornea may occur in three ways:

1st. From arrested development of the eye.

2d. From loss of balance between the functions of secretion and absorption of the aqueous humour, occasioning over-distension of the anterior chamber of the eye, and consequent opacity of the cornea.

3d. From intra-uterine purulent inflammation of the eye.—I am, sir,

Your obedient servant,

JOHN CHRISTIE, M.R.C.S.L.

50, North Hanover Street,
Glasgow, April 12, 1841.

CASES OF CLUB-FOOT, WITH A FEW OBSERVATIONS THEREON.

BY EDWARD WEIGHT, M.R.C.S., &c.

Surgeon of the Wokingham Union, and formerly Clinical-Assistant at the Westminster Hospital.

[For the Medical Gazette.]

THE Editor of the MEDICAL GAZETTE having been kind enough, in January of last year, to publish an account of a few cases of talipes treated by me, as well as some reflections that occurred to me after a careful consideration of the facts then presented to my notice, I am induced again to trespass a little on the patience of the readers of the GAZETTE, and transmit an account of two cases, together with a few observations, which my further experience has suggested.

CASE I.—Mr. H——, aged 18 years, was born with varus. He was for several years under the care of a mechanist of high repute, by whom the deformity was relieved to a certain extent. When the patient applied to me, the heel was two inches from the ground; the tarsus much arched; the convexity projecting much outwards, not forwards, as in pure talipes. The dorsum, especially over the os cuboides, was covered with a thickened cuticle, indicating the part where formerly rested the weight of the body. The affected foot was altogether smaller in size than the sound one. He now rests his entire weight on the metatarsal bone of the little toe. The entire limb is shorter and more delicate than the healthy. The tendo-achillis is thick-

ened: the inner edge of the plantar fascia prominent and rigid. There is motion between all the tarsal joints. The circulation in the foot is feeble, and the toes are always cold.

April 1st, 1840.—In the presence of Messrs. George May and Bulley, of the Reading Hospital, and of my neighbour Mr. Taylor, I divided the tendo-achillis in the usual way.

4th.—The wound being healed, Stromeyer's board is applied.

9th.—He suffers great pain at the sole of the foot, from the foot-board. The foot can be bent to nearly a right angle with the leg.

15th.—The pain arising from the pressure of the instrument is relieved by the use of air-pads.

16th.—Little's modified Scarpa-shoe was applied yesterday instead of Stromeyer's board, and to-day the patient complains that pain is produced after a little walking. The plantar fascia is very tense, and appears to impede the complete flexure of the foot.

May 5th.—With the assistance of my friend Mr. Bulley I divided the inner edge of the plantar fascia.

I was induced to make this additional section by experiencing a difficulty in moving the foot upwards and outwards. It appeared to me that the plantar fascia was the sole obstacle. He had the power of abducting the foot without producing tension of the adductors. Hence I do not attribute the existing incurvation to the contraction of the tibialis anticus, tibialis posticus, or flexor longus pollicis, but to the deficient length of the plantar fascia. The little wound is covered with a piece of sticking-plaster, and the patient is placed in bed.

7th.—Stromeyer's board is applied to-day.

9th.—He stands more evenly on the os calcis. Little's boot is applied, and he walks about more freely.

18th.—The weight of the body tends too much to the outside of the foot.

June 5th.—The tarsal arch is improved.

January, 1841.—Stromeyer's board has been discontinued for some time. The patient favours the affected limb, and says it is weaker than its fellow. Within the previous four months it has very much increased in point of strength and size.

In this case the mechanical contri-

vances which were employed were successful in overcoming the varus almost entirely, but the operation of Thilenius alone was sufficient to bring down the heel. The patient is now employed in agricultural pursuits. He cannot stand upon the affected limb alone more than a few minutes without support. The flexion of the joint is perfect. The foot has increased in length and in breadth, and it is altogether normal in shape, and, upon the whole, the error of his gait is hardly perceptible.

CASE II.—William Hook, æt. 36, a healthy subject, was born with well-formed feet, and when about three years old was trodden upon by a horse: after this the contraction occurred.

The dorsum of the foot was, when he applied to me, much arched. The astragalus projected considerably. A deep furrow extended across the sole of the foot; the extremities of the metatarsal bones and the os calcis being drawn close together; that is, not more than two inches apart. The heel was raised four inches, and there was no apparent want of power in the muscular apparatus of the foot.

April 1st, 1840.—In the presence of the friends whose names are mentioned in the history of the last case, the tendo-achillis and the plantar fascia were divided in the ordinary way. Sticking-plaster, pasteboard splints, and bandages, were applied.

4th.—The wounds have cicatrized. Stromeyer's board applied.

9th.—The heel is considerably lower, and the arch of the tarsus suppressed. He bears the board without much uneasiness.

May 1st.—The foot may now be brought to a right angle with the leg. He can place the foot flat on the ground.

3d.—The great toe being in a state of permanent extension, I divided the extensor muscle.

6th.—I have had a strong spring placed on the sole of Little's boot, the action of which tends to promote flexion of the foot, whether the patient be recumbent or perpendicular.

8th.—The flexion of the great toe is now permitted.

June 18th.—The patient's gait is much improved. The sole of the foot is nearly of its normal flatness. He wears a laced-up boot with a cork sole.

July 10th.—The foot is now nearly

natural in its appearance, and he can walk about the whole day without pain.

REMARKS.—In my former communication I was led to observe that “too favourable an impression of the facility of this operation, and the celerity of the cure, had been conveyed to the public, and I did not, therefore, feel justified in keeping in the shade the difficulties and delays which inevitably occur in the management of these deformities by operation.” My subsequent experience has tended to confirm me in the opinion which I then expressed. I feel convinced that many cases have been undertaken which were not susceptible of improvement by surgical treatment, and that too great results have been expected in *all* cases.

When the age of the patient is somewhat advanced, and the deformity aggravated, no benefit can reasonably be expected from this operation, unless the patient possess an excellent constitution, great intelligence and power of endurance, and an unlimited amount of time at his disposal.

In all the cases of this malady, there can now be no doubt that the muscular power of the antagonist of the contracted muscles is either very much diminished or annihilated, and that even after the restoration of the limb to its proper symmetry, an artificial force must be employed to counteract the preponderant action of the contracted muscles. I understand that Dr. Little and his colleagues now adopt this plan in every case.

Although I do not choose to blink the difficulties which constantly occur in the treatment of these cases, I am far from wishing to detract from the real merits of the operation. It is without doubt well adapted to relieve to a greater or less degree a great variety of deformities, and will prove a blessing of great value to the rising generation.

A great many requisites are demanded for the general success of this therapeutic scheme. Expensive apparatus, intelligent attendants, time and patience, are indispensable in all cases; and in many instances the want of these has precluded the adoption of this means of cure. I have had the good fortune to act under an exceedingly liberal and humane Board of Guardians, who have, without stint, supplied me with every thing I required for the

treatment of my pauper cases. But I fear few of my professional brethren are likely to meet with equal encouragement in this respect. I think it therefore a matter of congratulation that an orthopedic hospital has been established, where all the means which science can suggest are supplied for the use of patients afflicted with deformities. I think that the institution has a claim for a subscription from every parish in the kingdom in which a club-foot or deformed limb may be found. It ought not to excite jealousy in the regular hospitals, since the cases it undertakes require a minuteness of attention on the part of the medical attendants which is not to be expected, because it is impracticable, at these institutions. Such an institution, if well conducted, would not in its beneficial influence be restricted to the poor alone. The improved modes of treatment to which it would give rise would be directly applicable also to the relief of the afflicted amongst the wealthy.

Wokingham, Berkshire,
April 2, 1841.

MEDICAL GAZETTE.

Friday, April 30, 1841.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”
CICERO.

MANAGEMENT OF THE POOR IN SCOTLAND.

SOME subjects, says Horace, will please but once, while others will gratify the reader or spectator, though repeated ten times*. Scarcely any topic will bear frequent discussion with more advantage than the one which we have placed at the head of this article—a topic which involves the happiness of a nation, and is alike interesting to the physician, the statesman, and the philosopher. Such a subject, if handled with ordinary skill, must excite the attention of every man not destitute of

* Hæc placuit semel; hæc decies repetita placebit.

human sympathies; what must it effect, then, when discussed by a writer like Dr. Alison, whose good sense kindles into life under the breath of good feeling, and in whose hands dry figures, no longer inanimate, are made to speak the language of humanity and religion?

Besides those capital essays which, on former occasions, have been the subject of our commentary, Dr. Alison has more lately published a summary of his views in a pamphlet of forty-seven pages†.

It is extracted from the Quarterly Journal of the Statistical Society of London, and will, we trust, have a circulation as great as its merits.

Some points are, of course, brought out more prominently in this than in the former essays; but the subject gains by this alternation of interest; for truth may be looked at, with advantage, in a variety of lights.

A singular circumstance, and well worthy of note, is, that the Scottish law, in theory, is humane; it is the practice only which has left the misery of the indigent unrelieved. This neglect has been carried to such a pitch that, in many places, "the allowance they receive can be regarded as nothing else than *a system of protracted starvation*." These words are extracted from a report made in the town of St. Andrew's by a committee consisting of the Provost and three members of council, Sir David Brewster, the episcopal and two dissenting ministers, &c., and unanimously approved by the Town Council. They are speaking, indeed, of their own town only; but, unfortunately, the reproach applies to Scotland generally. More than half the permanent paupers receive an allowance of one shilling a week each,

to provide themselves with food, clothing, fuel, and lodging. If this is not a mockery of relief we do not know what is. The poor wretches waste away under the infliction of their innutritious diet; and "their feeble attenuated appearance bears sufficient testimony to the inadequate manner in which they are supplied with the necessities of life." In plain English, they are starved, and look starved.

In the small town of Tranent, matters are just in the same state. A shilling a week is the usual pension for the old age of those whose youth and strength have been spent in serving the community. They would die of cold and hunger did not benevolent persons, *generally working people*, assist them with money and food. Suppose a collier to be reduced by disease or accident, does society lend him a helping hand? Hear Dr. Somerville Alison. "So urgent is their distress, that many of them, even in this frail state, go out to the colliery and do a little work. *I have known men so situated work occasionally to within a day or two of their death*; and have little doubt the lives of many are thus shortened." Does not this sound like an account of the condition of negroes on a bad estate, in the worst times?

Mr. Barclay, the Sheriff Substitute of Perthshire, last year drew up some observations on vagrancy, which do honour alike to his head and heart. The distinction, indeed, which is commonly made between those sentiments which are the offspring of pure reason, and those which are the effusion of benevolence, is often a groundless one, and separates what is naturally intermingled. The cold economist who reasons algebraically on the progress of population, and calculates happiness by logarithms, is apt to make more foolish deductions than a child ten years old; while more genial lawgivers, sympathizing with the

† Illustrations of the Practical Operation of the Scottish System of Management of the Poor. By W. P. Alison, M.D. &c. [Read before the Statistical Section of the British Association, 18th Sept. 1840.]

hopes and joys of their fellow-creatures, and without dread of the multiplication of mouths, have arrived at truer conclusions, and established the frame of society on a firmer basis, than the most subtle analysts.

Mr. Barclay is well aware, that to sweep indigence from the face of the earth is a scheme merely fit for a stark-mad economist, who sees the world only on paper. It is just barely possible—and hardly so, we should hope—to sweep it away from the sight of the rich. To pretend to relieve the pauper with a shilling a week, and then hunt him back to his hovel, if he appeals to the kindness of his neighbours for real relief, is so shocking to the common sense of mankind, that, though often tried, it never succeeds in any age or country. It was in vain that Henry VIII. and his successors tried martial law and executions on the largest scale; the excesses caused by misery are stopped by relief, not punishment. English indigence was softened, like the traveller in the fable, by the sun, and not the storm; the 43d of Elizabeth succeeded, where the provost-martial had failed.

Mr. Barclay mentions a county (Perthshire, we suppose) where vagrancy and crime have attained such a pitch, that it is difficult “to reside in the country districts with peace and safety, not to speak of enjoyment.” Curiously enough, some people attribute the evil to begging, and this again to indiscriminate charity, as if nature had forbidden these reasoners to see an inch beyond the surface. This enormous vagrancy, so strange to English ears, is caused by the “very stinted parochial relief given to those on the Scotch poor’s list.” To the unspeakable astonishment of many prosperous gentlemen, the poor cannot live on a shilling a week, and will not die. There is a well-known story of a Frenchman who

on being asked by a minister of state what could have induced him to write a certain infamous lampoon, answered *Monsieur, il faut vivre*. To which the minister replied, *Je n’en vois pas la nécessité*. The gentry of these uncomfortable country districts would seem to have been smitten with the same blindness as the minister in the story; they cannot see why unemployed rustics should refuse to starve quietly on their weekly twelpence, and they condemn the criminal benevolence of those who encourage these annuitants in their expiscatory wanderings. We wish these economists would just turn to the History of England, A.D. 1540—1600, and learn the consequences of leaving the poor to their own resources, as it is termed in Malthusian cant; in other words, to famine and fever. They will easily see that justice is the best policy, and that benevolence to the poor is the best security for the rich. We are glad to find that at this end of the island, the cold paroxysm of Malthusianism, under which so many laboured a few years since, is fast passing away, and that truth, decency, and good feeling, are regaining much of their lost ground. Thus, the Morning Chronicle, a Whig oracle of no mean note, confessed, about a fortnight ago, that it was absurd to expect an agricultural labourer to lay by a provision for his old age, from his scanty wages; and that it was cruel to shut him up in a workhouse at sixty, instead of giving him out-door relief, after he had added considerably by his labour to the capital of his country. Other similar confessions from economic quarters, as well as the turn of the late poor-law debates in Parliament, give the friends of humanity the best grounds for hope. It is to be expected that we shall not much longer hear the mean threat that the ploughman, who has saved nothing

from his wages of eight shillings a week, shall be condemned to pass his last years in the dismal shades of a Union workhouse. It is said, or *was* said, by certain scribblers, that the man who saved up nothing when young was not entitled to an annuity when old. But these purblind sophists forgot that the labourer's wages do not really represent the value of his labour to society; and that if every ploughman who lived to sixty had a pension of ten pounds a year, the community would still be a great gainer by its bargains with the race.

Mr. Barclay concedes too much, we think, when he says, "In England it may be that the pauper is (or at least *was*) in a situation more enviable than the labourer, and so industry was fast merging into pauperism."

The pauper in a workhouse never was in a more enviable state than the labourer at large. To prove this, it is not requisite to weigh their several portions of bacon in the critical balance, and show which of the two lived on the scantiest fare. The confinement and discipline of a workhouse always turned the scale against the pauper; so that a run upon a workhouse, and the desertion of hard-earned potatoes for eleemosynary soup, were never yet heard of, save in the morbid dream of a Malthusian. To go to prison in order to improve one's diet is so rare a whim, that no laws are requisite to guard against it. Nor was "industry fast merging into pauperism." The southern countries of England have often been described in the most moving terms as the very hot-bed of pauperism; nay, M. le Vicomte Villeneuve de Bargemont, formerly Counsellor of State, and *Préfet du Nord*, attributes the excess of poverty in the north of France to the proximity of England, which, he says, has inoculated the rest of Europe with *le*

*véritable pauperisme**. Any one, at a distance, might imagine from these piteous complaints that paupers had eaten up the south of England, like a flight of locusts; and that the labourers having discovered the charms of gratuitous broth, had left the land to produce its own crops. Yet neither Kent nor Sussex has been a wilderness within our memory, but both have always looked more like gardens than deserts; so that the case seems to resemble Swift's story of the nobleman of Lagado (under the Flying Island), whose estate was exquisitely cultivated, but who was ridiculed and despised by his countrymen "for managing his affairs no better, and setting so ill an example to the kingdom."

We are delighted to have occasion again to thank Dr. Alison for his services to humanity, and to find that the clearness with which he states his case is equalled by the perseverance with which he advocates it.

DR. ELLIOTSON AND HIS PUPILS.

"SUPERB and doubly honourable presentation of plate from the grateful and admiring students to the eminent and magnanimous Professor."—Such are the grandiloquent terms in which the above fact is announced in one of the evening papers, and, taken with the manner in which it is printed (the whole being in capitals) they tend to produce a very imposing effect.

In plain language the facts are, that on the 17th a deputation from Dr. Elliotson's pupils, at University College, waited upon that gentleman, and presented him with a handsome piece of plate, in testimony of their admiration of his zeal and talents. To their address Dr. Elliotson made a reply at some length, which has been published with his signature attached, and which we therefore presume to be correct. The most re-

* Alison, Illustrations, p. 37.

markable passage is one referring to phrenology and animal magnetism, on which subjects the Doctor's opinions remain unchanged, as will appear from the following extract:—

“Gentlemen,—This is the most gratifying day of my existence. * * * * In advocating phrenology as I have done for nearly five-and-twenty years, and mesmerism as I have done for three years, I have adhered strictly to facts, and so shunned all speculation that those phrenologists who are more prone to listen to authority, and more prone to speculate than to examine nature, regard me as little advanced in phrenological knowledge; and many mesmerists pronounce me a sceptic; and, indeed, the facts are so wonderful and unlooked for, that I have affirmed none, and will affirm none, which I have not myself satisfactorily witnessed, however high the authority that declares them. Phrenology has now at least twenty thousand supporters in this country; and mesmerism is daily gaining ground among able and scientific persons, and is undergoing investigation in very many quarters.”

CLINICAL LECTURE,

Delivered at University College Hospital.

BY SAMUEL COOPER, Esq.

Senior Surgeon to the Hospital, &c.

GENTLEMEN,—It seems to me, that, in the generality of works on surgery, you will not find the diffused popliteal aneurism described with all that care and accuracy which the importance of the subject demands. On this account I am glad to avail myself of the opportunity, which the case of Jeremiah Tomkins affords, to call your attention to the diagnosis and treatment of a form of aneurism which is often followed by the loss of the patient's limb, or life.

CASE I.—*Diffused popliteal aneurism.*

Jeremiah Tomkins, æt. 35, admitted under Mr. Quain, Oct. 15, 1841: a labourer in the coal trade, and accustomed to drink freely.

About five weeks previously he first noticed a swelling towards the inner and back part of the thigh, and about the junction of the middle with the lower third of this part of the limb. It throbbed and was tender, notwithstanding which he did not desist from work until a fortnight before his admission.

The swelling continuing to increase, he now felt pain in his knee, extending down the back of his leg to the ankle and foot, and also up to the hip.

Three days ago, he observed an increase in the swelling of the parts about the knee, especially in the popliteal space, but extending down to the ankle and foot; and attended with numbness and inability to move the leg.

The following was the state of the case on the patient's admission. In the popliteal space, or rather just above this, and over the course of the femoral artery, was a hard firm moveable mass, discoloured over a portion of its surface, and marked by enlarged superficial veins. The patient experienced throbbing pain in it; the leg was considerably swollen, the foot benumbed, and the knee, which was stiff, was the seat of a pricking sensation. The patient, before his entrance to the hospital, had been salivated, on the supposition that his complaint was rheumatism. The patient, still imagining that his disease was rheumatism, placed himself under Dr. Williams, but was transferred to the care of Mr. Quain on the 17th, as soon as the case had been ascertained to be a secondary diffused aneurism. The original circumscribed aneurism was situated at the commencement of the popliteal artery, and the swelling of the limb below this point, and especially in the popliteal space, had augmented very much during the last two days. The tumor, which was tender, and painful on pressure, could be felt with the hand to be attended with a weak pulsation in it, and I observed some livid discolouration of its surface. When the leg was touched, or pinched, the patient was scarcely conscious of what was done, so great was the degree of numbness in it.

Directly I placed my hands upon the swelling, I remarked to Dr. Taylor and Mr. Morton, who were present when I first saw the case, that its solid feel at once made me certain that the case was not an abscess; and that the view which had already been taken of its true character was the correct one—a point which, had there existed the shadow of a doubt, would have been rendered perfectly clear by the history of the swelling; its throbings in the early stage of it; its sudden increase when it became diffused, and the reduction in the force of the pulsations accompanying this latter very important change. Then another sign, particularly adverted to by Mr. Morton, was the sensation imparted to the surgeon's hand when it was applied to the swelling, at the moment of pressure being removed from the artery in the groin; for then the fresh and sudden rush of blood to the tumor could be distinctly perceived in it, leaving no doubt of the swelling arising from, and communicat-

ing with, the artery. This is one criterion which, when the pulsation is feeble and obscure, deserves well to be remembered, though I do not know that any distinct notice has been taken of it in works of surgery, with reference to aneurism attended with extravasation of blood. Yet it is a circumstance of great value in elucidating the diagnosis.

For reasons which I shall presently explain, a popliteal aneurism in this stage is one that does not admit of delay, and therefore, after a few minutes' consultation, the operation of tying the femoral artery was performed by Mr. Quain, at one o'clock.

The first incision began about two inches below Poupart's ligament, and was continued downwards three inches. As the lower part of the wound exposed some of the effused blood, the artery was dissected down to in the higher part of it; and the fascia lata and sheath of the artery having been opened, the ligature was applied. The man was afterwards removed to bed; and the limb having been placed on an oblique plane, so as to facilitate the return of the venous blood, it was next covered with blankets, to maintain its temperature and promote the circulation.

As soon as the oozing of blood had ceased, the wound was closed with sutures and isinglass plaster.

18th.—Temperature of the limb below the aneurismal tumor found to be higher than that of the opposite limb, or than what it had been before the operation. The discolouration from the extravasated blood increased. Pulse 135; no appetite; bowels open; pain in the groin, in the course of the artery, and in the popliteal space, with a pricking sensation in the leg. Tumor less tender.

19th.—Has slept well; pulse 120; general swelling less.

20th.—Edges of the incision nearly united; but there is suppuration in the deeper parts around the artery, some puriform fluid having been pressed out. The temperature of the limb 88 just below the wound, 86 at the sole of the foot, and 90 in the opposite limb.

Some castor-oil prescribed, to open the bowels; and the following medicine, which the patient had been taking, was continued.

R Potassæ Bicarb. ʒiv.; Acid Tart. ʒiiss.;
Vin. Ant. Tart. ʒss.; Aq. ʒxij. Ft.
mist. Cochl. magn. ij. alternis horis
sumend.

21st.—Pulse 102: sensibility in the limb somewhat improved; no pain, not even on pressure; tumor diminished.

25th.—Œdema of the leg has disappeared, and the circumference of the thigh, where the aneurism is situated, measures less, by one inch, than on the day of the operation; the

temperature and numbness not altered since last report; discharge from wound lessened.

Nov. 3d.—Ligature came away.

15th.—Limb bandaged, and compress applied over tumor. Swelling decreasing slowly; sensibility of limb perfect again; wound healed; health undisturbed.

25th.—Bandage thought to be serviceable in promoting the absorption of the effused blood.

Nov. 30.—On removing the bandage, a slight discolouration observed over the instep, about the size of a shilling; also two or three smaller spots over the upper part of the tibia. These, it is reported, had certainly been produced by the pressure of the roller.

Limb to be kept more elevated, and all pressure on the discoloured parts to be prevented with pads of lint suitably arranged.

Dec. 5.—The discoloured spots have now become open sores, to which the water dressing is applied. Bandage discontinued.

25th.—Swelling nearly all gone, without further use of bandage. Ulcers healed.

31st.—Bandage has been applied again during the last two days; patient sits up two or three hours daily; a slight hardness the only vestige of the original disease.

Jan. 13, 1841.—Since last report, two sloughs have occurred on the great toe; one on the upper part of it; the other over the tarso-metatarsal articulation; and perhaps ascribable to the effect of the severe weather, as they formed after the bandage had been for some time discontinued. Water dressing applied.

30th.—Slough near extremity of toe has been detached, and the ulcer is left superficial. A portion of the other has also separated.

REMARKS.—This case affords a very good illustration—

1st. Of the symptoms of a diffused popliteal aneurism; the sudden increase of the swelling on the sac giving way; the discolouration of the skin; the solid feel of the tumor, caused by the blood; the numbness and pain in the leg and foot; the œdema; the feeble and obscure pulsations; the thrilling sensation communicated to the surgeon's finger on removing pressure from the artery in the groin: all these points were open to your remark.

2dly. Of the prudence of operating under such circumstances without delay; for if time had been lost, on the old and exploded notion of waiting for the enlargement of the anastomosing vessels, the quantity of blood, effused in the cellular tissue of the limb, would soon have become greatly increased, and the circulation so much impeded from this cause, that gangrene would have been

the consequence, attended with the loss of limb, or even of life. At all events, you may depend upon it, that the risk of the occurrence of mortification is very much in proportion to the extravasation in the cellular tissue, which state, joined to the impediment to the due supply of blood in the leg, caused by the aneurism itself, must be, in every point of view, disadvantageous, and a source of peril. In a case which I attended in private practice, where the effusion of blood into the cellular tissue, from a very large popliteal aneurism, was four times as copious as in this instance, the limb was speedily seized with mortification, and the patient's life was only saved by amputation. Here, indeed, the sac gave way on the side towards the popliteal space; the blood gushed suddenly into the cellular tissue in prodigious quantity, so as to fill and distend nearly the whole of it, from the lower part of the thigh, down to the heel. In fact, there was a large quantity of extravasated blood on each side of the tendon of Achilles.

3dly. The case which you have had the opportunity of studying will also teach you to avoid making pressure on the limb with a tight bandage after the operation; as it is apt, like any thing else that tends to obstruct the free return of blood in the weakened state of the circulation in the limb, following the operation, to cause sloughing and ulceration. Fortunately, in the case before us, such mischief was only superficial, and not of serious consequence. The bandage was applied in consequence of the very slow progress in the absorption of the effused blood. The ulceration produced by it over the tibia and tarsus, however, was confined to a small extent, and of little importance. The most troublesome ulceration occurred near the end of the great toe; but as this came on during the intensely cold weather of January last, and some time after the bandage had been removed, the latter probably had no share whatever in its production. The plan of keeping the limb in a comfortable temperature, and with the foot and leg raised on an inclined plane, to favour the return of venous blood, was, on every account, right and judicious. The decision with which Mr. Quain proceeded to the operation without the least delay, and the skill with which he executed it, were the means of saving both life and limb.

CASE II.—Dislocation of the fifth cervical vertebra on the sixth, without fracture.

You know that a dislocation of the dorsal and lumbar vertebræ, unaccompanied by fracture of the bodies or articulating processes of those bones, is prevented by various anatomical resistances, especially those arising from the size, shape, and direction of the articulating and spinous processes, and the

strength and tightness of the ligaments. But, in the cervical portion of the spine, such an accident is occasionally met with. The complete form of it, however, as distinguished from a displacement of an articular process of one side, is so rare, that the following case well merits your attention.

John Green, aged 51, admitted Dec. 18, at half-past 7, P.M., under the care of Mr. Quain. About 22 hours previously, whilst carrying a sack of coals, he slipped backwards from the step of a door, and his neck came against the edge of the step. The man was unable to rise, his lower extremities being almost completely paralytic; and he felt "sensations of pins and needles" in his upper extremities. The paralysis of the lower limbs increased, and at 12 o'clock, P.M. his breathing became difficult; and, at a later period, he found himself unable to cough. His bowels have not been acted upon: he has been bled twice; and his urine has been drawn off with a catheter.

On admission, he complained of sharp and aching pain about the sixth cervical vertebra; there is no external mark of injury or crepitus to be perceived, and no apparent displacement. His mental faculties are unimpaired; pulse 54, rather full, but soft; respiration 22, and wholly diaphragmatic; he speaks with an effort, his respiration then becoming more hurried, and he is almost unable to cough. His abdomen is somewhat tympanitic. His whole surface is of the natural temperature. The power of sensation in the upper extremities is much impaired, but that of the head and neck, above the clavicles, remains perfect. In descending from this point to about the ninth rib, it becomes less, and is here completely lost; the remaining part of the trunk and lower limbs being wholly paralysed. The loss of sensation extends somewhat higher posteriorly than in front. The loss of motion has a corresponding extent and degree: some power over the movements of the upper extremities still continues, but the man cannot direct them accurately, as if the action of some muscles was completely paralysed. The parietes of the chest, except the diaphragm, are quite immoveable. His bladder is not distended. He has no priapism, nor seminal emission. He was placed quietly in bed, and some mild stimulants were administered to him.

9 o'clock.—He is restless, and desirous of changing his position; his breathing more difficult; pulse slower; he is very desirous of drink, which he swallows with facility. In this condition he remained until half-past 4, when his breathing became more difficult, and he passed a very copious stool. He died at half-past 6, 34 hours after the accident.

Examination of the body, ten hours after

death.—No external mark of injury. On cutting down to the cervical vertebræ, some blood was found extravasated between the muscles. The fifth cervical vertebra was found dislocated forwards on the sixth; the articular processes of the former having passed in front of those of the latter. The capacity of the spinal canal was greatly diminished, the cord compressed, and blood effused into its substance. The nerves passing out between the fifth and sixth vertebræ were compressed; the anterior common ligament was stretched, and partially torn; the intervertebral substance was entirely torn from the body of the sixth vertebra; the articular ligaments were lacerated; the supra-spinous was entire, but stretched; the ligamentum subflavum was torn, and partially detached; the blood throughout the body remained fluid, and occupied chiefly the venous system.

The lungs were generally emphysematous in feel and appearance; there were some points of ecchymosis in their substance; and the bronchi contained a great deal of frothy mucus. There were also some spots of punctiform and stellated redness in the course of the intestinal canal.

The particulars now explained, including the symptoms during life, and the post-mortem appearances, cannot fail to interest every student desirous of gaining a knowledge of the accidents to which the spinal column is liable. This is the second example of a complete dislocation of the cervical vertebræ, unaccompanied by fracture, which I have witnessed within the last six or seven years. By the favour of a gentleman, formerly attending my lectures, I was enabled, a few sessions ago, to exhibit to the surgical class of University College a specimen of a complete dislocation of the middle cervical vertebræ, without fracture. The man from whom it was taken was killed instantaneously, by his head being forced against the top of a gateway, as he was sitting on an omnibus, which was going along at a rapid rate. The body was conveyed to the Middlesex Hospital. In the late Mr. Howship's museum might be seen another example of a complete dislocation of the cervical part of the spine without fracture.

COMPOUND CATHETER.

REPLY TO MR. FOULKES AND CHIRURGUS,
BY A PUPIL OF DR. BUCHANAN.

To the Editor of the Medical Gazette.

SIR,

HAVING been a pupil of Dr. Buchanan at the time he had his stricture instrument

constructed, and having been, therefore, upwards of ten years familiar with its form and application, I was certainly not a little surprised at Mr. Foulkes' attempt to pass off that instrument as an invention of the present day and his own. If the principle of the two instruments, merely, had been the same, it would be unfair to call in question Mr. Foulkes' originality; but that he should have hit upon not merely the principle, but the whole details of the construction of Dr. Buchanan's instrument, is somewhat too much to be believed on this side of the border. It is sometimes difficult for a man to find out the origin of his own thoughts; and, without insinuating that Mr. F. owes any thing to the itinerant catheter-maker, I am persuaded that if he will take the trouble to recal his first ideas on this matter, he will find they did not originate solely "in the torture endured by his patients" under the usual treatment: I make this supposition merely that I may not be obliged to bring against any member of a liberal profession a charge so odious as that of deliberate plagiarism; although, I confess, Mr. Foulkes' last letter does not call for much delicacy of reply. The wrath which he manifests is not unlike that of a person who feels sore on being stripped of his borrowed plumes. His abuse of the whole Scotch nation, and his endeavour to enlist the sympathies of all Englishmen on his behalf, are peculiarly absurd. If he had been an Irishman, he certainly would have urged the matter as an additional reason for the repeal of the Union!

Mr. Foulkes denies the identity of the two instruments. There certainly are differences; but every difference is a fault. Mr. Foulkes' instrument is an awkward imitation, by a person who has not thoroughly understood the uses of the original. Thus, his small catheter is imperforate at the point, which deprives him of the use of the silver probe inside. There is a clumsy head-piece on all of the catheters, which prevents their slipping over each other when we wish to withdraw any of the larger pieces, leaving the smaller behind—a most important practical application of the instrument. Correct these defects, and Mr. Foulkes can no longer deny that his instrument of 1841 is in every respect that of Dr. Buchanan's of 1831.

One word to Chirurgus. If he compares M. Gerdy's apparatus and that of Dr. Buchanan, he will find that, while they resemble each other to a certain extent in principle, they differ completely in the details of structure and mode of application. M. Gerdy certainly employs several canulæ passing within each other, *but these canulæ are all of the same length*; the smaller

(cannules de remplissage) being merely intended to fill up the void in the larger, and form a *single conductor*, which guides down to the stricture a small sound or bougie, by which alone the dilatation is effected. How different is this from a series of catheters, of *different lengths*, passing over each other, and all contributing on the principle of the wedge to effect dilatation. Instead of being mere conductors, the larger canulæ are most powerful instruments of dilatation, and by far the safest, for they take the smaller canulæ, which have passed on beyond them, for their guides, in forcing their way through the stricture. Chirurgus' own proposal is liable to the same criticism. I may add farther, that a single conductor, while it is of no use as an instrument of dilatation, does not meet the difficulty presented in the majority of bad cases of stricture, in which there is not one only, but two or three contractions of the urethra. In such cases you must have two or three conductors, of different lengths and diameters, which would form, together, an apparatus on the same principle as Dr. Buchanan's.

I hope my regard for an old teacher will be held motive sufficient for my thus adventuring to break a lance in his just quarrel.

I remain, sir,

Your most obedient servant,

"SUUM CUIQUE."

April 6, 1841.

GUY'S HOSPITAL CLINICAL REPORT SOCIETY.

ON Thursday evening, the 22d inst., the tenth general meeting of this society was held in the medical theatre: Bransby B. Cooper, Esq. in the chair.

This society, which is peculiar to Guy's Hospital, was founded by the late lamented Mr. Blackburn, and has been carried on with the most distinguished and almost unexpected success. The object of the society is implied by its name: each member has a certain portion of the Hospital assigned to him, all the cases in which he is bound to report. The members meet weekly, and discuss any remarkable cases which may present themselves.

At the close of each session, the honorary certificate of the society, stamped with the great seal of the Hospital, is awarded to that gentleman whose reports are considered the most perfect.

At the meeting lately held, after the report had been read by Dr. E. L. Birkett, the secretary, two honorary certificates were presented; one to J. R. Bedford Esq. late secretary, to whom the society is most deeply indebted; the second to Mr.

D'Auvergne, for the superiority of his reports. The Chairman presented these honourable testimonials, accompanied by most feeling addresses, to each gentleman.

Resolutions were moved and seconded by Messrs. Purchas, Menzies, Pizey, Veasey, Clegg, Gull, Longmore, Noyes, Chapman, and Francis, and the meeting separated, having been conducted and characterized throughout by the utmost harmony of sentiment.

REMARKABLE ŒDEMA IN THE ARM OF A FEMALE,

ALTERNATING WITH THE STATE OF THE
MENSTRUAL DISCHARGE.

DR. Post related a singular case in the N. Y. Hospital of œdema of the right arm in a female, which began three years ago, and was produced by putting it in hot water after it had been very much chilled. This was followed by swelling of the arm, and by cessation of the menses, which were regular before. The elbow became stiff, and she has not been able to bend it since. She has menstruated two or three times since it happened, but never perfectly; and at these periods there was a remarkable diminution of the swelling, so that it nearly disappeared; at other times it is so hard that it will not pit on pressure. There is always an increase of pain and swelling on the approach of the menstrual period. The forearm bears the mark of extensive incisions, the cicatrices being more distended than the sound skin, so as to form cylindrical prominences. Various modes of treatment have been employed. After menstruating last month, the swelling of the forearm nearly subsided. The arm can only be kept with the fingers hanging down when walking, as she is not able to support it in a sling; while sitting, she has it supported on a pillow. She has recently menstruated more freely than at any time previous to her becoming affected, and the swelling has nearly subsided; she has menstruated two months in succession, which had never been the case before since her attack; she had suffered from vomiting, but has now recovered from that. A seton was placed on the back of the wrist a month ago; she had before been partially relieved by an issue. The case is remarkable for the long continuance of the œdema, and for its connection with menstruation.—*New York Journal of Medicine and Surgery.*

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns, April 13, 1841.)

	PRICE.		DUTY and 5 per cent.	DUTY PAID.	
	£ s. d.	£ s. d.	s. d.	In 1841, to last week.	Same time in 1840.
Aloes, Barbadoes, D.P. c	15 0 0	to 30 0 0	{ B.P. lb 0 2 } { F. lb 0 8 }	35,659	24,214
Hepatic (dry) BD. c	5 0 0	10 0 0			
Cape, BD. c	2 10 0	3 5 0	F. lb 1 4	—	463
Anise, Oil of, German, D.P. lb	0 5 0	0 5 6	E. I. 1 4	255	322
E. I. lb	1 10 0	3 10 0	c 6 0	—	39
Asafoetida, B.D. c	0 1 0	0 1 1	lb 0 1	988	2,887
Balsam, Canada, D.P. lb	0 2 6	—	c 4 0	212	281
Copaiba, BD. lb	0 4 6	—	lb 1 0	286	123
Peru, BD. lb	25 0 0	50 0 0	c 4 0	39	11
Benzoin (best) BD. c	10 0 0	11 5 0	c 1 0	257	154
Camphor, unrefined, BD. c	6 3 3	—	lb 1 0	4,639	5,951
Cantharides, D.P. lb	0 9 0	—	lb 4 0	455	951
Caraway, Oil of, D.P. lb	3 10 0	—	lb 0 1	11,350	807
Cascarilla or Eleutheria Bark, D.P. c.	0 10 6	—	lb 1 4	765	774
Cassia, Oil of, BD. lb	0 0 4	0 0 10	c 1 3	{ 1,911	2,290
Castor Oil, East India, BD. lb	—	—	—		
West I. (bottle) D.P. 1½ lb	0 17 0	0 18 0	{ lb 0 6	235	298
Castoreum, American lb	0 18 0	1 0 0			
D.P. Hudson's Bay lb	0 10 6	—	{ c 1 0	12,086	24,010
Catechu, BD. Pale c	1 2 0	—			
Dark c	0 2 0	0 3 6	{ lb 0 1	4,070	21,094
Cinchona Bark, Pale (Crown) lb	0 2 0	0 4 0			
BD. Red lb	0 4 0	0 4 4	{ lb 0 2	4,630	2,528
Yellow lb	0 1 6	0 2 9			
Colocynth, Turkey lb	0 1 0	—	lb 0 2	6,677	5,697
D.P. Mogadore lb	0 12 0	1 15 0	lb 0 6	8,371	16,810
Calumba Root, BD. c	3 15 0	—	c 4 0	15	18
Cubebs, BD. c	5 0 0	15 0 0	c 4 0	156	148
Gamboge, BD. c	1 10 0	—	c 6 0	3	1
Gentian, D.P. c	0 1 0	0 3 0	{ c 6 0	2,910	2,935
Guaiacum, D.P. lb	12 0 0	13 0 0			
Gum Arabic, Turkey, fine, D.P. c	7 0 0	7 10 0	{ c 6 0	2,765	1,327
Do. seconds, D.P. c	1 19 0	2 0 0			
Barbary, brown, BD. c	5 10 0	—	c 6 0	5,437	7,236
Do. white, D.P. c	2 5 0	2 14 0	c 6 0	20	22
E. I. fine yellow, BD. c	1 15 0	2 5 0	lb 0 1	1,995	1,925
Do. dark brown, B.D. c	3 0 0	—	lb 1 0	3,492	2,653
— Senegal garblings, D.P. c	8 0 0	12 0 0	lb 0 6	12,614	12,877
— Tragacanth, D.P. c	0 0 2½	0 0 3	{ lb 0 3	5,285	4,865
Iceland Moss (Lichen), D.P. lb	0 1 3	—			
Ipecacuanha Root, B.D. lb	0 2 0	—	oz 6 0	450	610
Jalap, BD. lb	0 2 3	0 2 6	{ c 6 0	31	81
Manna, flaky, BD. lb	—	—			
Sicilian, BD. lb	1 0 0	3 10 0	lb 2 6	412	136
Musk, China, BD. oz	5 0 0	14 0 0	lb 1 0	8,845	14,485
Myrrh, East India, BD. c	2 0 0	11 10 0	lb 4 0	736	2,032
Turkey, BD. c	0 8 0	0 9 0	lb 0 1	87,823	117,581
Nux Vomica, BD. lb	0 9 0	—	lb 1 0	8,629	3,821
Opium, Turkey, BD. lb	0 12 0	0 13 0	{ F. lb 1 0	6,364	9,108
Peppermint, Oil of, F. BD. lb	0 3 11	—			
Quicksilver, BD. lb	0 5 0	0 7 0	{ lb 1 0	128	800
Rhubarb, East India, BD. lb	0 8 0	0 9 0			
Dutch, trimmed, D.P. lb	0 7 6	0 8 6	lb 0 6	43,408	41,039
Russian, BD. lb	—	—	{ lb 2 6	2,627	4,776
Saffron, French, BD. lb	1 10 0	—			
Spanish lb	0 1 0	0 1 9	E. I. lb 0 6	27,844	28,424
Sarsaparilla, Honduras, BD. lb	0 2 0	—	{ Other sorts 0 6	28,212	20,236
Lisbon, BD. lb	—	—			
Scammony, Smyrna, D.P. lb	0 18 0	1 0 0			
Aleppo lb	0 0 3	0 0 4			
Senna, East India, BD. lb	0 1 6	0 1 8			
Alexandria, D.P. lb	0 1 0	0 1 3			
Smyrna, D.P. lb	0 1 0	0 1 3			
Tripoli, D.P. lb	—	—			

‡§ B D. In Bond. — c. Cwt. — B. P. British Possessions. — F. Foreign. — D. P. Duty paid.

REMARKABLE EFFECTS OF
RHEUMATISM.

DR. WASHINGTON mentioned an instance of the remarkable effects of rheumatism which he had witnessed in a young man 19 years of age, brother of one of the teachers of the Deaf and Dumb Asylum. He has been suffering from it ten years, and lost a brother who had it the same length of time. His knees are dislocated, so that the condyles of the femur are anterior to the head of the tibia; the bones of the spine are ankylosed; the chin is fixed within two fingers' breadth of the top of the sternum, so that he cannot move his head without moving the whole body; the union of the bony surfaces is apparently perfect. The sight of the right eye has been destroyed by iritis; and the pupil of the other eye is so contracted by the same disease, as to prevent his reading. There is great emaciation of the whole system, and particularly of the limbs, so that his thigh is not larger than the wrist of an ordinary sized man. There is now active disease in both wrists, which are tender, and have a doughy feel. His digestive organs are in good order, his tongue slightly furred. He is of a cheerful disposition, and continued to read as long as the state of his eyes permitted. Dr. W. never saw anything approaching such a degree of deformity.—*New York Journal of Medicine and Surgery.*

RECEIVED FOR REVIEW.

A few Hints addressed to Medical Students about to visit the Parisian Hospitals, By a Physician.
The Touchstone of Medical Reform: in three letters. By Joseph Henry Green. F.R.S. &c.

A Practical Treatise on the Venereal Disease, founded on Six Lectures on that subject. By F. C. Skey, F.R.S:

Mr. Lee on English Mineral Springs, Bathing, &c.

APOTHECARIES HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED
CERTIFICATES.

Thursday, April 22, 1841.

George Buckley Crowther, London.—John Starling Day.—John Gregory Forbes, London.—James Golby Rusher, Banbury.—Joseph Sugar Baly, Leamington.—John Ayling Blagden, Petworth.—Frederick John Butler, Winchester.—Thomas Baynton, Radstock.—Francis Buckell, Chichester.—James Raney Leach Allott, Barnsley. John Buck, Burnham Market.—William Magor Boyle, Cornwall.—John Philip Trenick Hele, Ashburton.—James Forster, London.—John Randall, Yoxford.—Robert Smith Davison, Newcastle-upon-Tyne.—Edward Dunn, Durham.

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Friday, April 23, 1841.

William Stevens Butler.—Edward Harley.—Frederic Henry Secutan.—Henry Ellis.—Thomas Austin Robinson.—William Augustus Hillman.—George Bird.—John Royston.—Maxwell Jennyns.—George Thomas Gauntlett.—William Batley.

A TABLE OF MORTALITY FOR THE
METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 17th April, 1841.

Small Pox	18
Measles	3
Scarlatina	3
Hooping Cough	42
Croup	9
Thrush	4
Diarrhoea	8
Dysentery	3
Cholera	0
Influenza.....	9
Typhus	21
Erysipelas	3
Syphilis	2
Hydrophobia.....	0
Diseases of the Brain, Nerves, and Senses ..	152
Diseases of the Lungs, and other Organs of Respiration.....	294
Diseases of the Heart and Blood-vessels	23
Diseases of the Stomach, Liver, and other Organs of Digestion	46
Diseases of the Kidneys, &c.....	5
Childbed	4
Ovarian Dropsy	0
Diseases of Uterus, &c.	2
Rheumatism	6
Diseases of Joints, &c.	1
Ulcer	0
Fistula	0
Diseases of Skin, &c	0
Diseases of Uncertain Seat	112
Old Age or Natural Decay.....	57
Deaths by Violence, Privation, or Intemperance	18
Causes not specified	3
Deaths from all Causes	848

METEOROLOGICAL JOURNAL.

	THERMOMETER.		BAROMETER.	
April.	from 34 to 54		29.79 to 29.85	
Wednesday 21	36	51	29.82	29.74
Thursday . 22	39	46	29.44	29.53
Friday . . 23	30	53	29.64	29.52
Saturday . 24	43	58	29.60	29.73
Sunday . . 25	51	65	29.91	30.06
Monday . . 26	50	72	30.04	30.09
Tuesday . 27				

Wind, N.E. on the 21st and two following days; since S.W.
On the 21st, clear. The 22d, generally cloudy. The 23d, evening clear, otherwise overcast; raining very fast from half-past four A.M. till three P.M. The 24th, evening overcast, with rain; otherwise clear. The 25th, generally cloudy. The 26th, afternoon clear, otherwise cloudy; rain fell in the morning. The 27th, clear.
The sudden change in the temperature on the evening of the 26th and following day, is worthy of particular notice.
Rain fallen, .55 of an inch.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, MAY 7, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXXIII.

Treatment of Tetanus, continued. Wine; mercury; purgatives; digitalis; tobacco; musk; prussic acid; belladonna; carbonate of iron; oil of turpentine; strychnia; surgical expedients; general rules. Hydrophobia.

IN the last lecture we considered the symptoms, the nature, the causes, and to a certain extent the treatment, of that terrible malady, *tetanus*. There is good reason for believing that it is essentially a disorder of the excito-motory apparatus; that it is caused by irritation of a peculiar kind, affecting that part of the nervous system, and producing tonic spasm of the voluntary muscles; that the irritating cause may be centric, situated within the spinal canal, and applied directly to the cord; or eccentric, situated out of the spinal canal, applied to some part of one or more of its afferent nerves directly, and thus influencing indirectly the cord itself, and through it the reflex motor nerves; and that a certain ill-understood state of the system is necessary, a certain aptitude to take on the disease, before the exciting cause can be efficient; and that one circumstance which has been ascertained to tend to the production of such an aptitude, is a long-continued high temperature of the atmosphere.

I mentioned several remedies and plans of treatment which have been fairly tried, and mostly tried in vain, for the removal of this disease. The severe cases, and especially the severe traumatic cases, almost all prove

fatal; the less severe cases, those in which the paroxysms are less violent and less frequent, and which run on for several days, sometimes terminate in health: whether in consequence of the measures employed, or whether in spite of them, it is not easy to say. The idiopathic cases, as they are called, those which appear to be produced by exposure to cold and wet, are usually the least severe, and the more hopeful. The remedies that have been tried, and which were mentioned in the last lecture, are opium; blood-letting; the warm bath, the cold bath. I showed you that, under each of these remedies, a great number of patients died, and some recovered; and that the recoveries had been almost exclusively among those patients in whom the disease appeared originally in its milder form. So that whether the complaint was actually cured in any of these cases, whether, *i. e.* any single patient recovered, or recovered sooner, from using any of these remedies, who would have died, or in whom the disease would have been protracted, if he had not used them, is a matter of uncertainty.

The celebrated American physician, Dr. Rush, regarding the disease as essentially a disease of debility, and looking probably at its common tendency to death by asthenia, wrote a paper to recommend the employment of bark, and wine, and spirits, in full doses. It is curious enough, but quite in agreement with what has been already stated of opium, that how much wine soever may be swallowed by the patient, nothing like intoxication is produced by it. The system resists the ordinary influence of the alcohol. In one instance related by Dr. Currie, the disease lasted six weeks, and in that space of time the patient drank 110 bottles of port wine. The same author mentions a remarkable case, in which a horse, which was attacked by tetanus, and happened to be a great favourite with its master, was treated with wine, and got well, after swallowing more port wine than he was worth. Whenever

this plan has appeared to do good, it has been in the more chronic variety of the complaint.

Mercury, you may be sure, has not been left untried. It is said that the system is slow in submitting to its influence, in this malady. The specific effect of mercury upon the gums is not, however, so strongly resisted as that of wine or opium upon the nerves. Nor can we be surprised at this, when we consider that in tetanus the functions of organic life, are, comparatively, but little involved. It is clear that there is not time for any effectual exhibition of mercury in those severe cases that are early fatal. In its more chronic form the disorder has been known to yield upon the mouth becoming affected. This happened, if I mistake not, in Mr. Mayo's patient, mentioned in the last lecture. Tetanus has sometimes, however, commenced while the patient was in a state of salivation. Dr. Wells has recorded three instances of that kind. The experience of the military surgeons who were in Spain is, upon the whole, against the reputed efficacy of mercury. We must take care not to conclude too hastily that because a patient uses a certain remedy and recovers, he recovers through the operation of that remedy: any more than we should conclude, if he recovered during a general election, that the election had cured him. Yet this absurd and unsafe mode of reasoning is for ever employed in respect to disease, by the public; and too often, I fear, by ourselves.

Purgatives have been much given in tetanus; and often with manifest advantage: I mean in the less severe cases. But very large doses are commonly required to produce evacuations from the bowels. Whether the torpor of the intestines be always the effect of the disease, or whether it may not sometimes be, in part at least, the consequence of the opium that is given, I am not sure. When they do act, very unnatural motions are frequently produced: Mr. Abernethy tells us of a hospital patient of his who recovered under the use of purgatives: they were long before they had any effect, and when they did at last operate, such foetid stuff came from him that no one who could crawl out of the ward would remain in it. He says also that the nurses, in other cases, have reported the stools to be more like sloughs than fæces. Enormous quantities of drastic purgatives have been given. You may read an instance of this in the second volume of the *Medico-Chirurgical Transactions*. It is related by Mr. Harkness. There is a still more extraordinary case detailed by Dr. Briggs, in the fifth volume of the *Edinburgh Medical and Surgical Journal*. In little more than 48 hours, the patient in that case took 210 grains of scammony, 89 of gamboge,

80 of calomel, an ounce and four scruples of jalap, and $2\frac{1}{3}$ pints of what we call *black dose*, the infusion and tincture of senna: and all this without either sickness or griping; but on the contrary with the most decided benefit. In the first week of his disease, the patient swallowed—of calomel, 280 grains; scammony, 260; gamboge, 110; jalap, 3 ounces and 10 grains; infusion of senna, $5\frac{5}{8}$ pints. And altogether in the first 25 days—of calomel, 320 grains; scammony, 340; gamboge, 126; jalap, 5 ounces and $7\frac{1}{2}$ drachms; infusion of senna, $10\frac{2}{3}$ pints; besides an ounce and a half and 35 grains of the colocynth pill. I mention all this to shew what the system will bear, under the bondage of the disease; not as an encouragement to you to prescribe such doses.

It is certainly proper and necessary to clear out the bowels, and to endeavour to correct unhealthy secretions; yet numerous evacuations, the act of going to stool often repeated, should be avoided. Under such obstinacy of the bowels, the croton oil would perhaps be the most eligible purgative.

Foxglove and *tobacco* are two medicines, or rather poisons, which have been used; both, probably, upon the same principle. Their effects, when full doses have been given, are much alike: sickness, faintness, feebleness and fluttering of the pulse, coldness of the surface, with that slack and passive state of the muscles which belongs to syncope. But if we consider that the influence of these substances upon the involuntary muscles, especially upon the heart, is more certain and decided than upon the muscles of voluntary motion, which are the muscles involved in the tetanic spasm, and if we take also into account the strong disposition observable in tetanus towards death by asthenia, we shall scarcely be prepared to expect any good, but the contrary, from digitalis, or tobacco: especially in the later periods, when, so far from obviating the tendency to death, they would seem to co-operate with the disease in extinguishing life. However, if the result of experience were clearly in their favour, we should not be warranted, by mere theoretical views, in withholding these drugs. The army surgeons, some of them, have fancied digitalis useful. Sir James M'Grigor mentions a case in which it caused a relaxation of the spasms; but the man died afterwards, apparently from the effects of the remedy. And this is just what I find with digitalis. When given in large doses (and small ones here must be useless) it becomes unmanageable. Certainly we have no such accounts of its salutary power as would induce me to give it with much expectation of success, or to give it at all.

The tobacco is not given by the mouth, but thrown up into the rectum: either the smoke of its burning leaves, or (what is probably as efficacious, and I should think more uniform and less unsafe) an infusion of them in water. Mr. Curling, after analysing a large number of cases of tetanus, thinks tobacco the best remedy we at present possess. Mr. Travers is of the same opinion. However, I should recommend great caution in the use of this ticklish remedy. You ought to know that, when injected in other emergencies, in strangulated hernia, for example, mortal syncope has followed such enemata.

Musk, in large doses, has been strongly recommended by a Frenchman, Fournier-Pescay, who has written on this disease. He gave ten or twenty grains, at intervals; so that a drachm, or even two drachms, were taken in the course of the day; and he declares that he found it more efficacious than any thing else that he had tried.

Prussic acid and belladonna are said by Dr. Elliotson to have been freely employed, and to have failed: whether in his own hands, or in those of others, I do not know.

There is another remedy which the same physician has employed; and employed not without success: *the carbonate of iron*. Reflecting, he tells us, upon the good effect of this medicine in another complaint which has some points of analogy with tetanus, viz. chorea, of which I shall soon speak, and considering how miserably narcotics had failed, he determined to give the carbonate of iron a fair trial, upon the first opportunity. He has published some account of its effects, in tetanus, in the *Medico-Chirurgical Transactions*. In the first case in which he used it, the tetanus supervened upon a compound dislocation of the great toe. The method in which the remedy was administered was this: the carbonate was made into an electuary by mixing it with twice its weight of treacle. The electuary thus made was well mixed with beef-tea just as the patient was about to drink it. He took this every two hours, as much as he could swallow: and he got well. The next case is described as being a very severe one; it resulted from a contusion of the thumb. Dr. Elliotson says that he never saw a case, *which did well*, that was more severe. This patient also took the carbonate of iron, as much as could be got down; and that was about two pounds a day. He had injections twice daily, to keep the bowels unloaded: and the iron is described as having come away in large red lumps, in shape like horse-dung. This man recovered. In a third case, where a chilblain above the heel was the exciting cause, the boy died within 24 hours of the time when the remedy was first prescribed. To produce its effects upon the system (Dr. Elliotson observes, very truly)

iron must be given *for a few days*: nay he holds that months sometimes elapse before it has any effect. So that if it really be useful in tetanus, we cannot expect much good from it in the more acute cases: and these are the cases for which we want a remedy.

Oil of turpentine is one of the many substances that have been praised as useful in tetanus. Now bearing in mind its power (which I shall hereafter describe, but which you must at present take for granted) as a worm-killer, and also the frequency with which worms are met with in the stomach and bowels after death by tetanus, this is one of the drugs which I should employ as a *purgative*, taking my chance of whatever good might possibly arise from its specific or anthelmintic qualities. It may be given in such cases either by the mouth, or in an enema, or at both ends together: but it must be given in large doses, not less than an ounce at a time; and it may be mixed with an equal quantity of castor oil. The one oil dissolves or becomes incorporated in the other.

Strychnia has been suggested as a remedy for severe tetanus; not in infinitesimal doses, as Hahnemann would, I suppose, prescribe it, but in sufficient quantity to produce a sensible effect. The principle upon which this has been recommended is the same with that on which the nitrate of silver ointment is applied to the inflamed conjunctiva in purulent ophthalmia. We know that strychnia acts upon the spinal cord, affecting apparently those parts and those functions of the cord which are affected in tetanus: and in so fatal a malady, it would be justifiable, I conceive, to give the strychnia, in the hope that it might occasion a morbid action which would supersede the morbid action of the disease, and yet be less perilous and more manageable than it. But it would be right to try such a remedy as this, in the first instance, *in corpore vili*; upon one of the lower animals. This, were it successful, would be a cure, according to the Hahnemannian doctrine—*similia similibus curantur*—a doctrine much older, however, than Hahnemann. But the opposite maxim, *contraria contrariis*, has been suggested. Mr. Morgan proposes to give such poisons as are known to cause paralysis, with the view of countervailing the undue action of the muscles in tetanus. He produces artificial tetanus by inserting a poison brought from Java, called “chatic,” into a wound, and then relieves the tetanic symptoms by a North American poison, the *ticunas*. Professor Sewell, of the Veterinary College, has tried this principle in one case at least, where the tetanus was the result, not of any poison, but of disease. Not having had an opportunity of getting the

particulars of this case from Mr. Sewell himself, I give you Mr. Mayo's account of it. "A horse, suffering from a severe attack of tetanus and locked-jaw, the mouth being too firmly closed to admit the introduction of either food or medicine, was inoculated on the fleshy part of the shoulder with an arrow point coated with the wourali poison. In ten minutes apparent death was produced. Artificial respiration was immediately commenced, and kept up about four hours, when reanimation took place. The animal rose up, apparently perfectly recovered, and eagerly partook of corn and hay. He was unluckily too abundantly supplied with food during the night. The consequence was over-distension of the stomach, of which the animal died the following day, without, however, having the slightest recurrence of tetanic symptoms." I had fancied that the death had resulted from some injurious effect upon the lungs, produced by the artificial breathing. But I have little doubt that Mr. Mayo derived his statement from Mr. Sewell himself. The experiment deserves to be carefully repeated.

I have but little to say concerning what may be called the surgical treatment of traumatic tetanus. It was a natural thing, the source of the irritation being supposed to be the wound, to expect relief from amputation of the limb. But that will not arrest the morbid action after it has once been fairly established. Dr. Elliotson says he has searched scores of books, and found only one case in which the limb and the disease were lopped away together. However, Mr. Blizzard Curling, in his *Essay on Tetanus*, refers to seven instances of recovery, after the injured part had been amputated. Yet he states that "it is almost impossible to ascertain with certainty how far the amputation, in these cases, was of service." I believe I cannot offer you better advice on this subject than may be gathered from the concluding remarks of a very distinguished and philosophical surgeon, in his lectures on the disease. I allude to the late Mr. Abernethy, whose pupil I had the good fortune to be. He said, "The state of the part injured is not the sole cause of tetanus. In cases of tetanus I have often amputated injured fingers; and though I did not thereby save my patients, yet I think that the symptoms were mitigated after such amputations. In such cases then, I would not amputate any considerable member; nor even a small one, unless I thought that, from the injury sustained, it would prove useless to its possessor, even though the case should terminate favourably."

The tourniquet has been applied to the hurt limb; but not, so far as I know, with any good effect. The most promising expedient which surgery offers is the division of

the principal nerve proceeding to, or rather from, the seat of the injury. This, supposing the nerve to be known, and accessible, is less formidable, less severe, less hazardous, less maiming, and, if we may judge from past experience, more effectual too, than amputation of the part. Dr. Murray has recorded (in the 11th volume of the *MEDICAL GAZETTE*) a very interesting case in which the operation was followed by most decided and instant relief. The patient was a young midshipman, who having trodden on a rusty nail, which pierced the sole of the left foot, had kept watch the same night upon deck, the weather being very cold. The disease began the next day, and the symptoms ran high. It was a case, therefore, of severe or acute tetanus. Without loss of time the posterior tibial nerve was divided. The limb was previously cold, and as the patient said, dead, and he had little power of moving it. He could not articulate distinctly, on account of the closed state of his jaws. The nerve was cut through by one stroke of the scalpel; and "immediately (says Dr. Murray) he opened his mouth with an exclamation; and on looking at his countenance I was astonished at the striking improvement in it. I asked him how he felt, and he said he was already much better, and that his leg had come to life again." Some stiffness of the jaws and neck remained for a day or two; but he soon recovered. Dr. Murray refers to another case mentioned by Baron Larrey, in which division of the nerve had a similar result.

Probably, to be successful, the operation must be *early*; before the morbid condition peculiar to the disorder has had time to root itself in the nervous system.

Although, in the present state of our knowledge, there is no one remedy or plan on which we can rely for the cure of this fearful malady, we may with much confidence lay down certain general rules, the observance of which will secure to the patient the best chance of a favourable result.

Since any the smallest movement, or impression made upon the surface, or upon the senses, will bring on the severer degrees of spasm, it is of primary importance to protect the patient against these sources of trouble, so sure to aggravate his sufferings, and so likely to augment his danger. Hence if blood-letting should be thought advisable, it should be done early, sufficiently, and once for all. There should be no repetition of venesection or of cupping, or of leeches, unless the circumstances and progress of the case plainly demand them. The same remark applies to the frequent use of purgatives. The bowels should be well cleared in the outset, and then let alone. The patient should lie in a darkened room; from which noise also should, as far as may be possible,

excluded. He should not be surrounded by a multitude of friends or attendants. He should be enjoined to speak, to move, to swallow, as seldom as he can. In the severe traumatic cases, the nerve (in my judgment) should be promptly divided. And in all cases, there being no special indications to the contrary, I should be more inclined to administer wine and support, in large doses, than any particular drug. If the tendency to mortal asthenia can be staved off, the disturbance of the excito-motory apparatus may, perchance, subside or pass away.

There is a form of this complaint called *trismus nascentium*. As the name implies, it occurs in newly-born children. It is very frequent, and very fatal, in the West Indies; coming on usually in the second week after birth. Hence it has been called, "the ninth-day disease." Another of its names in the British settlements there, is the "jaw-fall;" from the circumstance that shortly before death the lower jaw, which had previously been firmly pressed against the upper, drops on the breast. It has been said that a fourth of the infant negroes in Jamaica used to die of this disorder. Some persons refer it to the irritation produced by the retention of the meconium in the intestines; others to irritation from the wound made by dividing the navel-string. A dose of purgative medicine appears to be the most hopeful remedy.

Tetanic symptoms sometimes occur (but I should think very rarely) in ague. Or paroxysms of tetanus return at regular intervals, and terminate by profuse perspiration; the patient being well during the intermission. When such phenomena arise, the treatment proper in severe forms of ague must be adopted: what that treatment is, I shall, in no long time, be able, I hope, to lay before you.

Again, tetanus is occasionally a symptom in hysteria: and then the treatment applicable to hysteria must be had recourse to; especially enemata of oil of turpentine, or the same medicine given by the mouth; and the cold affusion.

If the disease of which I have been speaking be dangerous, and very often fatal, in spite of all remedial measures, that which I propose to bring next under your attention is still more appalling; for I believe that, hitherto, it has been uniformly mortal. I know not that any one has ever been rescued by art, or saved by the efforts of nature, from *Hydrophobia*, after that frightful disease has once declared itself by its characteristic symptoms. The nature of those symptoms, and the absence of all constant or satisfactory traces of organic change in the dead body, sufficiently mark the disease as belonging essentially to the nervous

system, and as being essentially a *spasmodic* disease also.

What are the symptoms, stated in broad outline? These. Excessive nervous irritability and apprehension; spasmodic contractions of the muscles of the fauces, excited by various external influences, and especially by the sight or sound of liquids, and by attempts to swallow them; and extreme difficulty, amounting sometimes to impossibility, of drinking.

This is one of the diseases which are produced by animal poisons; and its course will be most conveniently traced if we include in our description of it the very first step towards the complaint—the application of the specific poison to the body. A man is bitten by a dog. After a time the symptoms proper to hydrophobia come on. After another interval the man is dead. Before we advert to the many very interesting points of enquiry which arise out of the contemplation of this malady, let us follow the tragedy from its commencement to its closing scene.

A person is bitten, then, by a mad dog. Does the existence of rabies in the animal modify in any way the injury thus inflicted? No; the wound that is made behaves just the same, to all appearance, as it would have behaved if the dog had not been rabid; and it gradually heals. After an uncertain interval—which lies, *for the most part*, between six weeks and eighteen months, and which has been called the period of *incubation*—these symptoms begin to be noticeable. The patient experiences pain, or some uneasy or unnatural sensation, in the situation of the bite. If it has healed up, the cicatrix tingles, or aches, or feels cold, or stiff, or numb; sometimes it becomes visibly red, swelled, or livid; on one occasion a papular eruption took place around it; sometimes it opens afresh, and discharges a peculiar ichor. The pain or uneasiness extends from the sore or scar towards the central parts of the body: *i. e.* if the bite has been inflicted on a limb, the morbid sensations extend towards the trunk. All this gives fearful notice of what is about to happen. This period is called the period of *recrudescence*. I believe it seldom fails to occur, although it sometimes is not noticed; the attention of the patient, and of his medical advisers, being absorbed by the horrible sequel. Very soon after this renewal of local irritation—within a few hours perhaps, but certainly within a very few days, during which the patient feels uncomfortable and ill—the specific constitutional symptoms begin: he is hurried and irritable; speaks of pain and stiffness, perhaps, about his neck and throat; unexpectedly he finds himself unable to swallow fluids, and every attempt to do so brings on a paroxysm of

choaking and sobbing, of a very distressful kind to behold; and this continues for two or three days, till the patient dies exhausted; in the way of asthenia.

I have seen only two examples of this terrific malady: one in St. Bartholomew's Hospital, in the year 1826; one much more recently in the Middlesex Hospital. As they constitute the whole of my personal experience in the matter, I shall relate these cases.

The first occurred in a coachman, the back of whose right hand had been struck, ten weeks previously, by the teeth of a terrier dog: but, as both the patient and his fellow servants declared, there was no wound made, no blood drawn, no breach or lifting of the skin; but merely an indentation, shewing where the animal's teeth had pressed. He was brought to the hospital on a Tuesday. On the preceding Thursday his hand had become painful, and swelled a little. On Friday the pain extended into the arm, and became more severe. His wife stated that he had been in the habit of sponging his head and body every morning with cold water, but that, on this morning, he refrained from doing so, on account of some feeling of spasm about the throat. His own remark on this was, that "he could not think how he could be so silly."

On Saturday the extent and the severity of the pain had still farther increased. On this and the preceding night he got no sleep. He felt ill and drowsy on the Sunday, but drove the carriage to Kensington Gardens: he was, however, obliged to hold both whip and reins in his left hand. The pain extended to the shoulder. He was then bled. A slop-basin full of blood was taken, with much relief to the pain; and purgative medicine was given, which operated well.

The next day he complained of "feeling very ill all over," and he told his medical attendant that he could not take his draughts, because of the spasm in his throat. That gentleman (Mr. Macdonald), concealing his own suspicions as to the true nature of the disease, said, "Oh, you don't like the taste of your physic! drink some water." But he declared he had the same difficulty with water. The next day he came to the hospital. When there, water was brought and placed before him in a basin, for the alleged purpose of allowing him to wash his hands. It did not seem to disturb him, nor to excite any particular attention. Water was then offered him to drink, which he took, and carried to his mouth, but drew his head from it with a convulsive shudder. After this, on the same morning, he was much questioned by several persons about the supposed cause of his illness; and water was again brought him, which agitated him, and he became exceedingly distressed and unquiet, complaining of the air which blew upon him.

I first saw him myself soon after this. He was then, to all outward appearance, well; lying on his back, without spasm, without anxiety; his face was somewhat flushed. He said he had a little headache, but no pain in the arm. His pulse was 132, full, and strong; his tongue moist, and slightly furred. He appeared to be a very quiet good-tempered man; and smiled generally when he was spoken to.

I was naturally much interested by this case, and at nine in the evening I visited the patient again. He was composed and quiet. Gruel was mentioned, and then he sighed two or three times deeply; then sat up, and, after a moment's look of serious terror, took half a spoonful of the gruel in a hurried gasping manner; and said he would not take more at a time, lest *the sensation* should come on. He was desired to drink the last portion of the gruel from the basin. He accordingly seized it with hurry, carried it to his mouth with an air of determination, and then a violent choaking spasm of the muscles about the throat ensued, the sternocleido-mastoidei starting strongly forwards. Most of the gruel was spilled over his chin; and he observed that he had been too much in a hurry, or he should have managed it.

The treatment consisted in full doses of opium, repeated at frequent intervals. On this visit to him I noticed that while attempting to take some of the gruel with a spoon, he seemed inclined to doze as he sat. Otherwise there were no signs of his being overwhelmed, or even sensibly affected by the opium; except that his general quietness might have been the consequence of it. He was quite calm and rational, except when attempting to take fluids.

On the Wednesday, at noon, he was nearly in the same state, but said he was better. In the course of the night some bits of ice had been given him: with considerable effort he swallowed two or three of these; the third or fourth caused so much spasm, however, that he was obliged to throw it out of his mouth: but so great was his resolution that he seized it again, and, by a strong exertion, succeeded in swallowing it. He complained now that his mouth was and had been clammy; and he champed much, and spat out a good deal of tough mucus. At his own request, and (as he said) that he might injure no one, a straight-waistcoat was brought, which he assisted in putting on. But he was perfectly calm then.

I now had an opportunity of seeing him take some arrow-root. He sat up in bed to eat it; and before attempting to do so, he made hurried inspirations, and sobbings precisely resembling those which occur when one wades gradually into cold water. He took small quantities of arrow-root eight or nine times, with hurry and difficulty, and

with sighs that succeeded each other rapidly. He said that he felt the upper part of his throat narrower than it should be. He continued to take laudanum mixed with sugar and bread into a kind of pulp.

By the evening of that day the disease had not made much farther progress. He again sat up and tried to take some thinnish gruel. While taking the basin into his hand, he drew back his head to a distance from it, apparently involuntarily. He took one half-spoonful with effort and difficulty, then sighed deeply and rapidly, or rather his breathing consisted of a succession of sighs at short intervals: he gave up the basin, and sank back on his pillow still sighing. In the course of that night he ceased to take the laudanum; he could no longer attempt it. The next day he was still composed, though more easily irritated; and it was found that he had lost the power of moving the left arm. His pulse was 140, and much weaker than before, and his mental powers were failing. He gradually sank, and died in the evening, having repeated the Lord's Prayer an hour previously. During the last hours of life he had been moaning, and tossing from side to side: his bowels were purged; fluid stools ran from him, and distressed him greatly. His lower extremities first became cold, and the coldness extended by degrees up to his chest. He hawked up in the course of the day a considerable quantity of ropy mucus, and much frothy saliva came from his mouth towards the close. As his wife was wiping this away, whether by convulsive accident or otherwise, his teeth came in contact with her finger, and drew blood. The part was cut out; and no bad consequence followed that I know of.

The examination of the body threw no satisfactory light upon the essential nature of the disease. Blood and serous fluid escaped on the removal of the calvarium. The vessels of the membranes were full, and the brain itself was mottled somewhat by its vascularity. There were a few spots of ecchymosis on the heart. The back part of the tongue was very vascular. The stomach presented the most notable appearance. There was a quantity of brownish-coloured mucus on its inner surface, and the mucous membrane had disappeared from a space about four inches in diameter at its larger extremity. That space alone was diaphanous; its edges sloped inwards; and a segment of this thin place looked exactly like a piece of china: on a white ground, there were inosculating vessels, some of them blue, and some of them of a coffee-coloured brown. I conclude that this appearance was produced by the action of the gastric juice after death.

This was in some respects a remarkable case. It was remarkable for its duration. Dr. Bardsley, in the article on Hydrophobia

in the *Cyclopædia of Practical Medicine*, states that the patients "invariably go on from bad to worse, and finally die before the sixth day." Now if we reckon that stage of the complaint here referred to by Dr. Barlow to have begun on the morning of Friday, when he was obliged to omit his sponging because of the spasm about his throat, this patient did not die till the middle of the seventh day. In fact it was a very protracted case; and the symptoms were less violent than usual: whether this was owing to the opium he took or not, it would be difficult to determine.

In the second of the two cases which it has been my lot to witness, the characteristic symptoms of hydrophobia were more faintly pronounced than is usual.

On going to the Middlesex Hospital, on Thursday, the 5th of October, 1837, I was told that a patient had been admitted (under one of my colleagues) labouring probably under hydrophobia. He had applied at the hospital in the middle of the night; but was then sent away, after receiving some aperient pills, with assurances that he was only feverish and nervous. On his reapplication in the morning he had been admitted.

I found him in the ward: a man twenty-five years old, of dark complexion and hair. He expressed his conviction that he was afflicted with hydrophobia; and said he was prepared for his fate. I observed that every now and then he suddenly sighed in a very peculiar manner; just as I had seen the former patient sigh. This would happen sometimes in the middle of a sentence, while he was speaking. He told us he had been bitten by a dog in the latter end of July; the dog was swimming, and like to drown, in a canal, and upon his reaching over to lift him out of the water, the animal seized upon his hand. After dragging the dog out, he beat him for his ingratitude; and then the dog ran off, and was pursued by a mob of boys, who had previously been pelting him as a mad-dog. There was a scar on the middle finger of the right hand; the nail of that finger had (he said) been torn through, and each of the two adjacent fingers had been more slightly bitten. His pulse was 84; but varied in frequency at short intervals.

He acknowledged that after receiving the bite he was uneasy as to its possible effects, and read books about hydrophobia at the time: but he affirmed that he had afterwards ceased entirely to think about it. He had persuaded himself that the dog could not be mad, from its being in the water. On Tuesday, if not earlier, he had been uncomfortable and restless; and on Wednesday he found he could not swallow liquids. On one of these days he experienced a slight pricking sensation, without any redness or tenderness, in the site of the scar; his right arm and leg seemed to himself hotter than the opposite

limbs; and the arm, though not tender, felt raw, and he could not bear the light contact of his clothes upon it. He became feverish also. From time to time a slight expression of terror passed across his features, and then he made a sudden, deep, sighing, inspiration: at other times his breathing and appearance were perfectly natural. It was said that when some water was brought him he drew himself back from it with horror. He talked a good deal.

I saw him eat rice, made pulpy with milk. He took it without looking at the spoon, from which he averted his eyes, and ate several mouthfuls, in a gulping manner, and with evident effort. His bowels had been purged by the pills, and he declared that the noise of the water in the water-closet had distressed him. The sound of some water poured from one vessel into another by the patient in the next bed, had also agitated him. So did the contact of my cold hand on his arm; and currents of air, even the breath of any one speaking to him; so that he insisted on conversing with the apothecary in such a position that the chin of each was on the other's shoulder. But there was no actual or apparent spasm.

At this time he affirmed that the presence of company cheered him, and did him good; and begged that he might not be removed into a separate room. And he wished for some amusing book that he might read.

In the evening I again went to see him. He did not seem worse, though he said "his symptoms were increasing." He had taken a dose of musk, and some morphia.

The next day I found the hospital in some confusion. Between eleven and twelve o'clock in the preceding night some of the officers of the hospital had gone to his bed, while he was apparently asleep, and certainly very quiet. They asked him if he would like some water. This seems to have greatly excited him; and immediately after their departure he rushed out of bed, (terrified, he said,) became furious and unmanageable, and was never again quiet till he died, about the same time the next night. He was now put into a room by himself; and, taking advantage of the momentary absence of the nurse, he bolted himself in alone; and declared he would admit no person but her. The door was at length forced, and a straight-waistcoat was put on him. He then became quieter in his manner; begged that no unnecessary violence might be used; asked to be poisoned; spat at some of the bystanders, and reproached them, talking rapidly and wildly like an insane person; yet loudly and angrily imposing silence on every one who addressed him. He said he could not bear to hear any one speak; that he did not like my bass voice. Then he would sneer at the students, and say they showed bravery

enough now he was confined: "was it right for young gentlemen of education to stand there gazing with curiosity on a dying man?" asked for bread soaked in water, and when it was held towards him, snatched it in his mouth in a savage manner; spoke of his "poisoned tooth," and talked perpetually. He took a fancy to one of the students, and begged that he might remain with him.

About this time he vomited some yellow fluid, and thought he felt the better for it, and asked for an emetic; and some tartarized antimony was exhibited. He was now pale, and his lips were livid; but none of the distinctive spasmodic attacks occurred: indeed water was not at this time suffered to be brought near him. This circumstance it was, this absence of the peculiar spasmodic paroxysms which characterize hydrophobia, that induced several medical men of much sagacity and experience to doubt, and even with some positiveness to deny, that the patient was suffering under that disease at all. They supposed him to be hysterical, half-crazy, or on the brink of delirium tremens. But though slightly expressed, the symptoms were unlike any thing I had ever seen, except in the previous instance. And the closing scene was quite distinctive.

It appeared, and he spoke of it as a thing which distressed him, that when he was most excited, his urine passed involuntarily.

In the evening I found his father with him. He had recognized him, and kissed his mother-in-law; but soon began again to rave, and to be apparently occupied with absent persons. He was pale and weak, and lay with his head over the bed, spitting continually upon the floor, which was thus made quite wet. He wished to have his hands at liberty that he might "clear his mouth." He was soliloquizing when I went into the room, in this way. "Monsters—monsters—see that monster Susan—take her away." (It appeared that he was now speaking of a young woman who had had a child by him.) "I thought they would do much for science, but never supposed they would inflict such agony as this;" and so on.

A little later Mr. Arnott visited him. He had then no pulse at the wrist. The waistcoat was removed. He sat up, and used some water, brought to wash his hands, without apparent distress. Soon after he sunk back exhausted; and expired.

His father corroborated what the patient had said of the dog: and told us his son was clever, and better educated than many of his rank (he was a tailor), and always exceedingly nervous.

The body was examined the next day. Its posterior and undermost surface was very livid. The blood every where quite fluid. The veins of the spinal cord, on its posterior part, were turgid; not at all so on the anterior. The substance of the cord was

quite natural. There was some fluid in the theca. The brain appeared to me, in every part, quite sound and healthy.

The head and face, which had been hanging over the table while the spinal canal was opened from behind, were deeply purple, as though universally bruised. This colour diminished rapidly after the corpse was placed supine, and the head raised somewhat above the level of the body. The papillæ at the back part of the tongue were greatly exaggerated, and looked like large vesicles. The cartilage of the epiglottis, at its lower part, was red. At about the middle portion of the œsophagus there was an appearance as if the cuticle was abraded. The mucous membrane of the stomach was soft, and red here and there, with a dotted injection resembling ecchymosis, especially on its rugæ. The air-passages were apparently healthy.

Generally, the disease, when it has once set in, and shown the peculiar hydrophobic symptoms, runs a short and fierce course. The nervous irritability becomes extreme; the peculiar paroxysms of choking spasm, and sobbing, are excited, not only by attempts to swallow liquids, but by the very sight or sound of them; Dr. Elliotson mentions a boy who was thrown into a state of violent agitation by hearing a dresser who sat up with him make water: the passage of a gust of wind across his face, the waving of a polished surface, as of a mirror, before his eyes, the crawling of an insect over his skin, is often sufficient to excite great irritation, and the peculiar strangling sensation about the fauces, in a hydrophobic patient. These circumstances were but little observable in the men whose cases I have related; the first of them indeed was remarkably calm and tranquil under the disease. In general the patient is dreadfully irritable, and apprehensive and suspicious: and in most cases there is a degree of mania or delirium mixed up with the irritability; the sufferer is very garrulous and excited. In this respect there is a marked difference between hydrophobia and tetanus. In the latter disorder the mental faculties are clear, and the patients serene, and what is called heartwhole, to the last. The two diseases differ in another striking particular: the spasm in the one case is tonic, in the other clonic. In tetanus, again, there is no thirst, and seldom any accumulation of tough and stringy mucus in the fauces and about the angles of the mouth: in hydrophobia both these symptoms are almost always present. So also is vomiting; but vomiting in tetanus is rare. The nervous irritability in hydrophobia is doubtless a part of the disease, and is very rarely absent even now-a-days. Some time ago it might perhaps have been plausibly attributed to the treatment adopted. I allude to that period in which it was believed that these miserable

persons had both the power, and the inclination, to impart the disease to others by biting them; and when, under pretence of shortening *his* sufferings, but really, I am afraid, with the cowardly view of protecting *themselves*, his friends were accustomed to smother the unhappy patient between two feather beds, or to open a vein, and to leave him to bleed to death. Any person, suspecting what was the matter, and foreseeing such a termination to his disease, might well be nervous and irritable. But now that this barbarous practice has been exploded, and the dread of being smothered does not occur to the mind of the patient, he is still found to be exquisitely irritable and timorous. The foam and sticky mucus that gather in the throat and mouth, these patients make great efforts, by spitting and blowing, to get rid of; and the sounds they thus produce have been exaggerated, by ignorance and credulity, into the barking and foaming of a dog. In the same way the paraplegia which sometimes takes place, rendering the patient unable to stand upright, has been misconstrued into a desire on his part to go on all fours like a dog. The pulse, though it may be strong and hard at the outset, becomes, in a short time, frequent and feeble, and the general strength declines with great rapidity. Death occasionally takes place within twenty-four hours after the commencement of the specific symptoms. Most commonly of all it happens on the second or third day; now and then it is postponed to the fifth day; and in still rarer instances, of which my first case was one, death does not occur till the seventh, or eighth, or ninth day. In most cases, the paroxysms, becoming more violent and frequent, exhaust the patient; but in a few instances the symptoms undergo a marked alteration before death. The paroxysms cease, the nervous irritability disappears, the patient is able to eat and drink, and converse with ease; those sights and sounds which so annoyed and distressed him before, no longer cause him any disquiet. In this state he often sinks into a sleep, and suddenly wakes from it to die: sometimes his existence is put an end to by a sudden and violent convulsion.

It is needless for me to go into a minute account of the morbid appearances that have been met with in persons dead of hydrophobia. They are various, uncertain, unsatisfactory. In some bodies, the most careful examination has discovered nothing amiss. In others, vascularity of the brain, or of the spinal cord, has been noticed. And in not a few instances the mucous membrane of the fauces, œsophagus, and stomach—or of the larynx and trachea—or of both these tracts—has been found red, and covered with adhesive mucus. But we must take care not to attribute undue importance to these last appearances—not

to conclude that they have been the cause of the symptoms, when in truth they may have been the effect of the disease. That we should find the parts in the throat red and congested is what we might naturally expect, when we consider the violent straining spasmodic action of these parts for some time before death. The morbid anatomy of this disease throws but little light upon its nature, or its proper treatment.

There are many very interesting questions connected with hydrophobia. I will state the principal of these, as shortly as I can.

1. You will be surprised when I tell you that some persons have made it a question whether there is any such disease at all. I have known such. The late Sir Isaac Pennington, who was Regius Professor of Physic at Cambridge, had never seen a case of hydrophobia, and nothing could persuade him that any one else had seen any thing more than a nervous complaint produced by the alarmed imagination of the patient, who having been bitten by a dog reputed to be mad, and having the fear of feather-beds before his eyes, was frightened into a belief that he had hydrophobia, and ultimately scared out of his very existence. Now if you meet with such incredulous persons, and think it worth your while to argue the point with them, you may object to their unbelief, the improbability that so many persons who have been bitten by mad dogs should have suffered so precisely the same train of symptoms, and at last have died, from the mere force of a morbid imagination; you may urge them with the fact that many of these persons have been under no apprehension at all, until the disease has seized upon them; that many also have been men of naturally strong and firm minds, not at all likely to be frightened into believing that they were seriously ill unless they really were so, and still less likely to be terrified into their graves. And if this has no weight with such reasoners, you may bring forward the conclusive facts that the disease has befallen infants and idiots, who had never heard or understood a word about mad dogs or hydrophobia, and in whom the imagination could have had no power in calling forth the complaint. And if they are proof against this, you must give them up: I can suggest nothing more.

2. Allowing that the disease exists as a real, and not a merely imaginary disease, and also that it is caused by the bite of a rabid animal: this important question arises—has it any *other* cause?

Setting aside that quibbling application of the term hydrophobia which some writers have chosen to make, to diseases in which, from some painful affection of the throat, the patients have been unwilling to attempt to swallow fluids, there are cases recorded, exactly resembling hydrophobia in their symptoms, and occurring in persons who

were never known to have been bitten by, or even to have been in the presence of, a rabid animal. The celebrated and accurate Pinel has given the history of such a case. There is another by Savirotte, in the *Journal des Savans* (August 1757). Now it is just possible that this disease may sometimes develop itself in the human body without any contagion having been applied: and it is also possible, and much more probable, in my judgment, that the poison may have been applied without the person's being aware of it. We shall see, by and by, some very possible ways in which that might happen. All that we need concern ourselves with practically, is this—that in 999 cases out of 1000 the disease in the human body is derived from a rabid animal. If it ever be spontaneous, we cannot reckon upon meeting with such a case: indeed, many men pass through life without witnessing the disorder at all.

3. Granting, then, that the disease, in man, is the result of an animal poison, the next question is, from what animals may he receive the infection?

We are sure that the disease, by the inoculation of which hydrophobia may be produced in man, is common in the *dog*, and that it has been communicated to the human animal by the fox also, the wolf, the jackal, and the cat. Mr. Youatt says that the saliva of the badger, the horse, the human being, have undoubtedly produced rabies, and some affirm that it has been propagated even by the hen and the duck. The same author mentions a case in which a groom became affected with hydrophobia from a scratch which he received from a horse that was labouring under the disease. All animals, even fowls, are susceptible of the disease when bitten by the rabid dog. Of course it is an important question to have resolved, whether the saliva of all these is capable of conveying the malady. The case just now mentioned on Mr. Youatt's authority would seem to settle the question as respects the horse: but as horses, cows, turkeys, &c. do not generally bite, we have not many opportunities of supplying a positive answer to the general question: there can be no doubt about the *cat*, the *fox*, the *wolf*, and the *jackal*.

The late Duke of Richmond died, abroad, of hydrophobia, contracted from a tame fox, which was rabid. In the 13th volume of the *Medico-Chirurgical Transactions*, an account is given, by Mr. Hewitt, of several cases of fatal hydrophobia from the bite of a wild and rabid jackal. Many examples are on record of the production of the disease by the bites of mad cats and wolves.

The first case which I have spoken of, as having been seen by myself, would seem to prove, if all the facts were correctly stated at the time, that the saliva of the dog may be sufficient to produce the disease, when it

is merely applied to the unbroken skin. It was affirmed by various persons that the teeth of the terrier did not break the cuticle. But we must take care not to draw a hasty general inference from a single case. Mr. Youatt, who has seen more of the disease probably both in man and in other animals than any person alive, does not think that the saliva of a rabid animal can communicate the disorder through the unbroken cuticle: he believes that there must be some abrasion or breach of surface. He holds, however, that it may be communicated by mere contact with the mucous membranes.

Of its harmlessness on the sound integument he offers this presumption—that his own hands have many times, with perfect impunity, been covered with the saliva of the mad dog. He mentions some singular instances in which the disease has been transmitted by contact of the saliva with the mucous membranes. “A man endeavoured to untie with his teeth a knot that had been firmly drawn in a cord. Eight weeks afterwards he perished, undeniably rabid. It was then recollected that with this cord a mad dog had been confined. A woman was attacked by a rabid dog, and escaped with the laceration of her gown. In the act of mending it, she thoughtlessly pressed down the seam with her teeth. She died.” If these cases be authentic, they are conclusive of this question; unless indeed the lips of those who perished happened to have been chopped or abraded. But Mr. Youatt's own opinion is that the virus cannot be received on a mucous surface without imminent danger.

The disease is said to have been caused by the *scratch* of a cat. But as we know that cats as well as dogs frequently apply their paws to their mouths, especially when the latter part is uneasy, (as it clearly is in mad dogs), this fact, of the production of the disease by a scratch, if thoroughly made out, would not *prove* that the disease can be introduced into the system in any other way than by means of the slaver.

CLINICAL LECTURES,

BY DR. CORRIGAN,

*Delivered at the Hardwicke Fever Hospital,
Dublin,*

During the Session, 1840-41.

LECTURE VI.—FEVER, No. 4.

Mode of surveying a Case of Fever. Cerebral Function, lesion of. Want of sleep, and its Effects. Treatment by local depletion. Observations on cold and opium.

IF, as we stand at the bedside of a patient ill of our present epidemic fever, we keep

the definition in mind laid down in one of our previous lectures, viz. that the case before us consists of a disturbance of a number of the vital functions, we take, I believe, the best practical view for treatment. If the pathological analysis we have made of fever be correct, this must be our view; and in accordance with it, when we come to the bedside, let us just look down upon the stricken being beneath us as one whose existence depends upon the continuous performance of those functions, and the danger to whom is just in proportion to the amount of derangement in any one function, or to the sum total of derangement in all. Let us, in short, look upon the existence or life before us, as compounded of several functions, and then examine in detail the derangement of each, or estimate the total amount of derangement in all. The functions which are the most important for us to consider in thus regarding a case of fever, are the cerebral and cerebro-spinal, the respiratory, the circulating, and the functions of nutrition and secretion; the latter including the functions of the intestines, kidneys, skin, &c. Let us just look, for practical treatment, at the individual existence before us as consisting, as it were, of those functions, in successive order for examination. By doing this we not only, I think, get the best practical view of the case, but we also acquire a steady and orderly method of examining the case as to its symptoms, and a rapid power of detecting the lesion of the particular function from which the principal danger is to be apprehended. If we still keep this mode of viewing a case of fever before us, we can understand some parts of practice that may seem, at first sight, to be a strange mingling of treatment; for instance the exhibition of wine, and the application of leeches, both going on at the same time in the same patient. But the analysis we have made will enable us to understand this; the leeching is for the derangement of one function, the wine for the debility of another: we should, however, anticipate, were we to take this point up at present.

Let us now, keeping this analysis still before us, go into the explanation of the treatment we are at present adopting, and the first function we shall consider is the cerebral function. The immense importance of the healthy state of the function of the brain and spinal cord needs not to be dwelt upon. The derangement of the function of the brain seemed to some writers, as Clutterbuck, of so great importance as to constitute the very pathology of fever, and although we do not retain such an opinion, we consider the derangement of the cerebral function as one of the most alarming we have to contend with. There are two very opposite states in which we meet this function in

fever: the first, where its disturbance consists in an aberration from its natural state, marked by a total want of that natural rest which this function requires, so that a patient is for several successive nights without rest. In every case, without a single exception, when this occurs, there follows delirium or derangement of the cerebral function, so that we have at first want of the natural rest of the function, and then invariably the result of that increased excitement of it, or delirium, and then we have what appears to have been the effect of the loss of the natural rest of the function—delirium reacting to increase the very cause that gave rise to it. We had a very good example of the progress of derangement of this function, from loss of its necessary rest or sleep up to the most violent delirium, in the case of Murphy, the policeman, who was in No. 1. He laboured when he came in, for some days and nights, under total want of rest. He then began to rave, and at last furious delirium set in. The cerebral functions are too high in importance in man to suffer derangement long, without that derangement involving danger to life, and the cerebral function cannot continue without serious disturbance, if deprived of its natural rest for many days. Hence it is that we regard with such alarm the want of sleep in the commencement of fever; that we dread such ultimate danger when this symptom exists. The order is, want of sleep, delirium, and last, if unchecked, nervous exhaustion and death. Keeping our attention now in the first place on this function, our first inquiry is as to its state, and our best information as to the amount of its derangement is derived from the answer given to our query as to whether the patient has slept. If the patient has passed a second night without sleep, we have allowed the derangement of this function to proceed as far as we dare, without directing our measures to rectify it. After having the head shaved, wait for no other symptom of derangement of the cerebral function than the want of rest. Apply some leeches to the forehead; the number may be few; we seldom use more than four or six. The result is almost always gratifying: in some cases there is good sleep at once procured, in others only a snatch the first night; the repetition of the leeches is again made the next day, and there is again rest: it is seldom necessary to repeat them more than the third time. There are so many instances in the wards, of almost daily occurrence, illustrating the effect of this treatment on the derangement of the cerebral function, that it is not necessary to point out any in particular. Let us now pause here to ask when we are to use the local depletion, how often, &c.

Suppose we see a patient on the third or fourth day of fever, without any thing very remarkable about the head or face, as to flushing, and with a pulse not only not stronger, but often, as in our fever at present, weaker than natural, are we to use the local depletion? I am sure we ought. There are few points of practice on which I would venture to be so positive, as to our treatment of fever, as the absolute necessity of local depletion to remedy that derangement of cerebral function, want of its proper rest or sleep, which, if allowed to continue, must end in delirium, probably in death. I know of nothing that should prevent us from employing this local depletion, except such an extreme state of depression of the vascular system as does not very frequently exist at the early period of fever, when want of rest is a prominent symptom. The propriety and good effect of local depletion in such cases are strengthened by what we see occasionally take place where epistaxis occurs. The effect of this natural depletion is almost always beneficial. Even where the state of pulse is such as to make us dread the application of but a few leeches, the cerebral irritation rapidly subsides on the epistaxis occurring. A case in which epistaxis occurs may be debilitated by it to an alarming degree, so as to require wine to a large amount, but still the cerebral irritation subsides, the patient sleeps, and the danger of death, if it occur in such a case, is from another cause than the derangement of the cerebral function.

Our present maculated fever has a remarkable tendency to assume a remittent form. We see it in the crisis even when taking place by urine, the urine alternating for many days; portions marking the crisis depositing the lithates alternating with portions containing the floating cloud, so characteristic of the fever being still present. The same character is seen in the affections of the cerebral function, the patient remaining free from headache, and the pain of head returning every evening, or twice a day, for many days in succession. When this occurs, the patient is relieved by the application of two or at most four leeches; and provided we succeed in obtaining rest and alleviation of the pain by this means, and we most often do, we have nothing further to do with the cerebral function: mere heat of head need not be interfered with. There is a practice in high inflammatory fever which is very judicious—the application of ice caps to the head. It is not necessary in our maculated fever as at present existing. I think I may go farther, and say it is not only not necessary, but it is injurious. The character of the fever is not such as to make the circulating function any where carry on its action

well under the depressing influence of severe cold. You may perhaps naturally, at first thought, suppose that local depletion from the head, and ice caps, may act in the same way, and that where one is proper so must be the other. A little reflection will, I think, convince us that they are not so much alike as you may at first sight suppose. In local congestions, for instance, some forms of phlegmonoid erysipelas, as well as other local affections, we use local depletion by incision or by leeches; but instead of following this up by the benumbing effect of cold, we endeavour by heat to give action to the overloaded and enfeebled vessels, while we relieve them by the local depletion. So, in the instance before us, we may relieve the local congestion, which is probably the state of vessels in the brain in our present fever; but it does not follow that because we do this we should keep those vessels under the benumbing effects of continued cold at or below the freezing point. I have seen long-continued application of intense cold to the head in our maculated fever produce slough of the integuments of the forehead and temples. Some of our very worst forms of cerebral disturbance, too, or derangement, are in cases where the scalp is not at all hotter than natural; and we may, I think, aggravate this bad form, by an abuse of the remedy which, in some cases of high febrile action and great vascular energy, is one of our best aids. I have said that the state of the vessels of the brain is probably a state of congestion or passive hyperæmia, to which moderate local depletion is useful, although long-continued cold would be injurious. Analogy may be fairly, I think, brought to bear on this. We find, in nearly all our cases of maculated fever, the capillary vessels passing into a state of passive congestion or hyperæmia. We have positive evidence of this taking place in the lungs by the local signs; there is crepitating rattle resulting from the effusion into the air-tubes. That it is not an active state of inflammation or pneumonia, we have abundant proofs, which we shall come to in another lecture. For the present it is sufficient to say, that this state of vessels in the lungs sufficiently shews its nature by getting well under wine. And if we find the capillaries in this state in the lungs, where we have satisfactory evidence of their state, I do not think it is stretching analogy too far to suppose them in the same state in the brain. In fever the function of circulation shews a tendency to the same derangement through the whole system, and the same functional derangement that is seen in the depending surfaces of the body during the last periods of sinking in fever is seen after death in the hypostatic congestion of

the lungs and internal organs. The case of Toner, who has been very ill for the last few days, but is at present recovering, is instructive on this point. The maculæ are of a deep livid hue, his extremities and lips of a purple colour, and the conjunctiva is of a deep dark red; his pulse excessively feeble and tremulous. There is no local disease, no local cause in the extremities to account for this state of them, apart from the enfeebled state of the function of circulation generally. When we see the circulation in this state, where we have it directly under our observation, we have a right, I think, to believe that such too is more or less its state in internal organs. Thus, if we can in fever get in any organ satisfactory evidence of the state of the function of circulation, we shall scarcely err in assuming the same state to exist in an internal organ from which the signs might otherwise be doubtful.

We have only used opium twice within several months to obtain rest; one was in the case of a female who suffered under great irritation of the mucous membrane of the bowels: there was diarrhoea, gargouillement over the cæcum, very rapid pulse, and delirium, from which she was easily recalled, but into which she as quickly relapsed. A full dose of black drop gave her sleep, and stopped the delirium. The second was the case of a man named Toole, admitted on the fifth and sixth day of fever, but not maculated. He was delirious: his eyes were somewhat suffused, but the pupils were dilated; his tongue was white and moist; his skin was warm, but it did not give that pungent sensation to the hand laid on it that is often felt from a skin in maculated fever, even when not so warm as the skin in this case. His delirium was of very high excitement. He sung loudly, and was very noisy. He got 15 drops of black drop, and slept after it: this occurred on the 10th of Jan., and he left the hospital well on the 23d. A comparison of the local cerebral derangement with the general symptoms led us to the employment of opium in this case. Were the local derangement dependent on inflammatory action, there would be more accordance with this state in the symptoms derived from the tongue, skin, and pulse; but while there was so much local cerebral excitement, without any accompanying correspondent severity, the conclusion arrived at was, that the local cerebral derangement was mere irritation, and the practice adopted after a little deliberation was the exhibition of an opiate. There was immediate sleep, and the fever then went on through the remainder of its progress without any urgent symptom.

USE OF SPLINTS IN CHOREA.

To the Editor of the Medical Gazette.

SIR,

I BEG to forward you some remarks on the treatment of chorea, and shall be obliged if you will give them a place in your excellent journal.—I am, sir,

Your obedient servant,

GEORGE SOUTHAM,
M.R.C.S.L.

7, Crescent, Salford, Manchester,
March 31st, 1841.

There are few diseases more obstinate and tedious to cure than chorea, and few that have given rise to greater variety in their treatment. It is an extremely common affection amongst the children admitted into the Manchester Royal Infirmary; and as clinical clerk in that institution for upwards of two years, I had extensive opportunities of witnessing the different forms of treatment recommended, but frequently found that a line of treatment, which appeared singularly successful in some patients, was just as inefficacious in others. This discrepancy no doubt arises from our imperfect acquaintance with the pathology of the disease; also, in a great measure, from a want of attention to the condition of the patient, and the stage of the complaint when the remedies are applied. Looking over the cases that have come under my notice, I have generally observed that where the disease was recent, and no means had been adopted for its removal, the convulsions have been attended with derangement of the alimentary canal, as evinced by capricious appetite, constipated bowels, with scanty and scybalous evacuations. When the disease had been of longer continuance, and allowed to go on uncontrolled, in addition to the derangement of the stomach and bowels there has been more or less disturbance of the cerebro-spinal system: whilst the cases which had existed some time, and where the usual remedies had been resorted to, the general health was improved, but there was only partial mitigation of the convulsions.

These remarks tend to prove the truth of Dr. Hamilton's view of the nature of chorea—that debility and spasmodic action, previous to his time so much insisted on, are not its leading

characters, but that it depends upon a prior derangement of the stomach and bowels. In some instances this doctrine is very probably correct; but there is generally a predisposition to the affection which, as Cullen well expresses it, depends on a state of mobility in the system, that renders the body so excitable as to cause it to be readily affected by any irritation applied to it. One of the laws regulating nervous actions is, that its irritability is increased in proportion as its energy is diminished: consequently we find that the peculiarity of constitution in childhood renders the nervous sensibility so predominant as greatly to conduce to the development of this and other convulsive diseases; just as a relaxed and debilitated frame in adults produces a disposition to the nervous temperament, and makes them so highly sensitive to the actions of stimuli, both physical and moral. On this account we frequently find chorea connected with debilitated and relaxed habits, especially in those who, from their humble circumstances, have been accustomed to scanty and poor diet, and also of its more frequent occurrence in females than males. In such individuals we may readily conceive how any of the exciting causes of chorea may occasion the disease. The difficulty consists in ascertaining the exact seat of the affection, increased as it is by the variety of opinions concerning its pathological state, owing to the fatal cases having been generally complicated with other diseases.

The influence of habit in prolonging nervous complaints is very remarkable, and few diseases appear more affected by it than chorea; for in most of the obstinate cases which have resisted the various forms of treatment, there being no organic derangement, and the general health good, there can be little doubt that the convulsions remain almost entirely from habit. The conducting power of the nervous filaments being increased with the frequency of their excitement, these convulsions are continued from the slightest cause, just as in lively persons, when speaking, the movements of their hands are repeated on the least occasion. In fact, their frequent occurrence generates in the muscles a great aptitude to their repetition, as practice induces a facility in performing dexterous mechanical opera-

tions: thus a habit is acquired which assists the power of the original cause, and continues the convulsions a considerable time after the cause has been removed.

The tendency to imitation in children must also operate in keeping up that chain of actions in the body which has so long existed, whilst the absence of the "vigour of manhood" renders it extremely difficult for the child to exert himself in endeavouring to control the affected muscles. A similar condition may occur in mature age, in consequence of the balance and harmony of the functions of life being enfeebled and less vigorously performed, from a want of energy in that resilient power which enables us to bear the varied exertions of youth, acting also with such surprising vigour in restoring us to health after the depression of a severe illness. When, therefore, any of the ordinary powers destined to preserve the equilibrium between sensation and motion are disturbed, we may easily understand how the influences of habit and imitation may operate in prolonging this affection, by rendering the different sets of muscles independent of the will, which were previously of a voluntary character. If the automatic movements thus induced are not early checked, there will be some difficulty in subduing them, as we experience in attempting to avoid habits unconsciously fallen into, when the body is in a state of health. It was evidently with the intention of overcoming these influences that Dr. Darwin (*Zoonomia*, vol. ii. p. 506) suggested we should make perpetual, slow, and repeated efforts to move the limb in the designed direction, in expectation of the same good effects which follow such efforts in stammering: and Dr. Mason Good (*Study of Medicine*, vol. iv. p. 472) remarks, "it is very possible that, in some instances, a long and punctual discipline of the affected limbs, where the disease is not very severe, to regular and measured movements, may progressively recal them to their wonted order and firmness." The effect of dancing and music in curing the original chorea *Sancti Viti*, by diverting the automatic movements to others more regular and under the control of the will, ultimately breaks through a habit produced generally from imitation, and some defect in that power which keeps

the muscular system in its normal state of tension.

In old-standing cases, where the will can check the convulsions, but only so long as it is directed for that specific purpose, these means may occasionally be of service; but from the difficulty of keeping up their effects a considerable time must elapse before a cure can be established. To obviate this inconvenience I was led to try the experiment of confining the affected limb in splints, having frequently observed, in those instances where the convulsions had continued in spite of all remedial means, that if the patient's attention be directed to the deranged muscles, as for instance when in the hand and arm, causing him to grasp the sides of a chair, the motions comparatively cease; and this, when frequently repeated, proved a useful auxiliary in removing the disease.

Upwards of two years ago a case came under my notice where the convulsions had existed in the hand and arm for six months, though a variety of means had been adopted for their removal: believing them to depend on habit, from the control the patient had over the motions, and his apparently excellent health, I had recourse to the splints. For the first few days the twitchings, from their violence, caused some difficulty in keeping the splints firm, but they soon diminished in force, and in three weeks had entirely disappeared.

I have tried the same treatment in four other cases. In three, after attending to the bowels, I applied the splints, and continued them until a cure was established, which occupied in none more than a month. The convulsions were confined to one of the upper extremities, with slight dragging of the leg of the same side; but as the symptoms left the arm, the power over the leg returned.

The other case occurred in a youth fifteen years of age. The convulsions were more severe, and rendered it extremely difficult to keep the splints firm with an ordinary bandage: to obviate this inconvenience, I used the apparatus recommended by Velpeau for fractures, with the addition of small splints to keep the limb extended and at rest during the consolidation of the apparatus. This was removed in a week to allow of exercise; and the severity of

the convulsions being subdued, a common bandage and splints were again used. Though the convulsions affected both arm and leg, and splints were only applied to the former, the boy had recovered the use of both extremities in six weeks.

We see the same effects produced in the worst forms of chorea, where the convulsions are so severe that the patient has to be held down in bed: whilst the restraint is continued the violence of the motions is soon sufficiently relieved to enable the child to walk; but when it is removed, a length of time frequently elapses before a corresponding improvement is noticed.

In each case, considering the affection to have originated from some irritation in the alimentary canal, as hardened fæces, &c., I began with the administration of purgatives, repeated during the progress of the complaint as they were required; and whilst the splints were had recourse to, small doses of carbonate of iron were given to improve the system. Every three or four days the splints were removed, and the limb exercised, to prevent stiffness of the joints, and to assist in recovering the power of the will over the affected muscles. When they were discontinued, periodical exercise and the cold bath were used to re-establish the necessary vigour of the frame, and thus prevent a return of the disease, which not unfrequently occurs.

From what has been stated, it might be inferred that this plan of treatment is chiefly applicable to those cases where the disease exists almost solely from habit. By considering the general principles of physiology, we shall find that it may be used with advantage in recent cases, after the exciting cause has been removed, being peculiarly adapted, by allowing of the regular application of exercise and rest, for restoring the affected muscles to their healthy condition, just as sleep recruits the faculties of feeling and motion, wearied by the exertions of the day. For we are well aware that muscular energy depends on the contraction of the muscular fibres having intervals of relaxation; that in proportion as their action is increased and accelerated, subsequent rest is required to restore the same quantity of power that has been consumed; and that, by the regular alternation of exercise and

rest, power is acquired and sustained. On the contrary, if their action is prolonged, or too frequently repeated, their power of contraction is diminished, and, for a time, no effort of the will can restore it.

This is familiarly illustrated in severe and old-standing cases of luxation, where, by long and continued extension of the limb, the muscular energy becomes exhausted, and the bone is readily replaced. We know also that rest does not restore a part seldom brought into action in the same proportion as when it has been accustomed to regular exercise; and if uncombined with exercise it weakens it. A certain degree therefore of excitement and rest is necessary for the support and increase of the muscular energy. These facts are well exemplified in squinting, and had they been attended to in the early treatment of that affection would have prevented the necessity of having recourse so frequently to the lately discovered and ingenious operation; especially in those cases arising from imitation, or where the tendency to it is first produced from the nurse carrying the child in one position, or from allowing some part of the dress to interfere with the axis of vision. The consequence is, that the axis of one eye is directed to some prominent object out of the field of vision of the other, which being frequently repeated destroys the harmony between the two eyes: the one most exercised acquiring strength, the other, from want of it, becoming weaker. By this means the eyes are adapted to vision at different distances, which, if not checked, renders the faulty eye perfectly useless. So far as my experience goes, it is in cases thus produced that the operation appears to have been most successful. If, however, in the early stage of the affection, a plan of treatment had been adopted in accordance with the above principles, a cure might have been established without having recourse to the knife.

The same remarks are applicable to those cases of spinal curvature depending on muscular contraction which have received partial benefit from division of the affected muscles. If, in the commencement of the complaint, instead of rest and the recumbent position (the means formerly adopted in almost all cases of spinal distortion), the individual had been placed under the influence

of Calisthenic exercises, a more effectual and permanent cure would have been produced than we can ever expect to obtain from the operation.

The value of exercise in subduing those diseases whose duration and intensity are increased from habit, is also well shewn in stammering, which frequently depends on a want of influence of the will over the muscles of respiration; or, as a writer in the *Gazette Médicale* observes, "Its essential character consists in a convulsive condition either of a tetanic or choreic character of the respiratory function, the effect of which is either to disturb more or less, or altogether prevent, the articulation of sounds. What takes place in the lips, tongue, and throat, is altogether accessory, and according to which of these parts is chiefly affected the different varieties of the infirmity are produced."

—Johnson's Review, 1839. The accuracy of any theory concerning a disease is best tested by its effect upon the treatment. Now of all the numerous methods proposed for the cure of stammering, those are the most successful which are founded on the above views. Thus it has been long known that by taking a deep inspiration at the commencement of each sentence, and marking the time in speaking by regular movements, as in music, stammering may be cured. But these two means, simple as they appear, are very difficult to bring into operation, especially in children, for reasons previously mentioned. Sometimes spontaneous cures are said to take place, as age advances; but I believe that upon careful inquiry they will be found to have depended in a great measure on the individual's own exertion in overcoming the affection. It is, then, with the intention of increasing the influence of the will over the affected muscles, that gymnastic exercises have been so strongly recommended. "Indeed," as the above mentioned writer remarks, "it has been by their influence in regulating the act of breathing that the efficacy of the various means which have been recommended and found useful, in the treatment of stammering, is to be accounted for. In this way the practice of declaiming on the sea-shore, with a pebble in his mouth, cured, it is said, Demosthenes of old. As a further illustration of habit in prolonging disease in nervous and excitable indivi-

duals, I may mention that one of the children, previously to the attack of chorea, had a very obstinate cough, which seized her in paroxysms not unlike the whooping-cough. After being under treatment for some time, with but slight improvement, I ascertained that the attack usually came on when her father entered the house to his meals, and that when she was suddenly surprised it frequently ceased. I accordingly ordered some nauseous draught to be given whenever she coughed. In six days it was reduced to a slight hacking, which very soon disappeared. Other instances of the effect of habit in protracted diseases might be mentioned, but the object of this communication is chiefly to direct attention to the treatment of chorea.

The plan recommended may be easily combined with the means usually adopted, viz. purgatives and tonics. The former will remove any irritative matter that may be collected in the alimentary canal, and regulate its function; the latter will strengthen the system, and assist in correcting that irritable condition of the body which has rendered it prone to the affection; whilst the splints, by allowing of the regular alternation of rest and exercise, will restore the influence of the will over the affected muscles, also break through that habitual disposition which nervous and spasmodic actions are so liable to produce.

Of course the splints can only be used when the disease is confined to one or two extremities. When the whole body is affected, some other plan must be adopted. In the severest cases, as I have already stated, the violence of the convulsions is soon subdued by tying the patient down in bed with the straight-waistcoat. Perhaps if this remedy was continued for a longer time, the rest and restraint may sufficiently overcome the severity of the affection to permit the application of the splints.

The comparative infrequency of chorea in private practice must be my apology for this premature communication. Should any of my professional brethren be induced to try the means suggested, I should be obliged if they would favour me with their results*.

* It would be premature to make any remarks on the operations recently recommended for the cure of this affection, until surgeons of known reputation shall have fully investigated them.

CASES, WITH OBSERVATIONS.

By C. M. DURRANT, M.D., Ipswich.

[For the Medical Gazette.]

CASE I.—*Pneumonia, with secretion of air into the cavity of the pleura.—Recovery.*

L. D., æt. 34, of leucophlegmatic habit, after exposure to cold, was seized with rigor, followed by the usual symptoms of acute pneumonia in the lower lobe of left lung. On examination, the general and auscultatory phenomena were well marked, the disease having reached the stage of hepatisation, around the margin of which the existence of crepitation further indicated its progressive extension. He was twice bled from the arm, leeches, put on the antimonial treatment, and subsequently blistered. These remedies quickly relieved the more urgent symptoms; respiration was gradually returning in the diseased portion of lung, when, from a slight indiscretion in diet, or from some unknown cause, the disease relapsed with a considerable aggravation of the symptoms.

The dyspnoea was very urgent; the pulse 96, and small; cough very frequent, and hacking; expectoration in small quantities, of a yellowish-green colour, muco-purulent, and very offensive; constant pain in, and total inability of lying on the affected side: on percussion a tympanitic clearness was elicited between the fifth and seventh ribs of same side: respiratory murmur at this spot inaudible—in other portions of lung natural, with an occasional large mucous rhoncus: distinct bronchophony, almost amounting at times to pectoriloquy, over diseased portion, heard both anteriorly and posteriorly. A bellows sound was now audible with both strokes of the heart, but did not extend widely over the chest, and existed only during one day.

He was again put on the antimonial treatment without relief; the symptoms somewhat improved, but the cough, pain in the side, and foetid expectoration, still continued; the pulse ranging between 90 and 100, small and irritable. The stethoscope indicated the disease in the same stage, but now become latent, and at this period was heard beneath the angle of left scapula imperfect ægophony.

Local depletion by leeches, followed by a blister, was again had recourse to; the mercurial treatment was now substituted for the antimonial, in combination with digitalis, Dover's powder, and the iodide of potassium. The mercury (Hydrarg. c. Cretâ) rapidly affected the system, producing considerable tenderness, and finally ulceration of the gums, a coppery eruption on the legs, and

subsequently salivation. Under this treatment the symptoms gradually subsided, the ægophony merging into bronchophony, while the natural vesicular murmur was heard progressively resuming its distinctness throughout the diseased portion of lung. From this period the patient slowly recovered.

OBS.—Without entering in detail on the individual pneumonic symptoms which presented themselves in the above patient, I have nevertheless deemed it a case of considerable interest, and one prompting a few remarks both in reference to the symptoms and treatment. Previous to the relapse, the disease presented no feature worthy of note: subsequently, however, we find a few points to which especial attention may be directed.

First, the expectoration. This, in all cases of pulmonary disease, is a symptom of decided value, and one requiring daily and careful examination. The expectoration in pneumonia, as stated by most authors, is considered pathognomonic of the disease: that such is the case when coexistent with other general and stethoscopic phenomena, I admit; but, as in the present instance, and in many others, as is well known to the practical observer, it is liable to frequent diversities, both in colour, quality, and odour. In the present case, the greenish muco-purulent sputa, with the faintly gangrenous odour, were of importance, as indicative of severe pneumonic disease, with accompanying bronchitis, and an evident tendency to gangrenous degeneration.

The next point of interest is the tympanitic sound as elicited on percussion over the seat of the disease. This phenomenon was first pointed out by Dr. Graves, of Dublin; in whose case it evidently proceeded from an effusion of air into the serous cavity, generated by secretion. That such was the case in the present instance I am fully satisfied; and from its position over the solid portion of lung, we obtained at the same time the two somewhat opposite signs; viz. tympanitic clearness on percussion, and resonance of voice, with absence of râle or murmur on auscultation. The appearance of this preternatural clearness, as usual in these instances, was sudden; but its subsidence, contrary to the ordinary termination as remarked by authors, was gradual. That it was not stomachal

clearness is evident, from its situation as well as from the differential sounds produced by percussion. In this case I fully verified the remark made by Dr. Stokes, when alluding to the sound caused by a distended stomach, and that by an accumulation of air, who styles the one "a tympanitic dulness;" the other, "a tympanitic clearness."

These cases have been confounded with abscess, fistula, &c.: by marking the position and extent of the clear sound, together with the absence of the other signs of these lesions, we shall, I think, find no difficulty in establishing a correct diagnosis.

In the above patient a distinct bruit de soufflet accompanied both sounds of the heart, but existed only during one day. A similar circumstance is noticed by Dr. Graves as occurring in the case of tympanitic clearness alluded to above. In that case the bellows sound was not confined to the region of the heart, but was audible over the front of the chest. In my patient, on the contrary, it was limited to the cardiac space. Dr. Graves mentions another symptom as occurring in the same individual; viz. throbbing of the chest, synchronous with the heart's action: this phenomena did not obtain in the case under consideration.

Dr. Stokes attributes the bruit de soufflet in Dr. Graves's case, to pericarditis, or endocarditis, rendered latent by being complicated with pneumonia. In the foregoing case I attributed the bellows sound to increased irritability of the heart, in the same manner as is so frequently heard in nervous irritability, functional affections of the organ, &c. and unconnected with existing disease in this viscus.

The last symptom I shall notice is the occurrence of the ægophony. It is now admitted by the greater number of practical authors on thoracic diseases, that the signs of ægophony and pectoriloquy are, if taken singly, and without comparison, the two most fallacious of all the auscultatory phenomena. These signs are only valuable as occurring singly and without coexisting symptoms in the opposite side. The necessity of comparative examination in all stethoscopic investigations cannot be too strongly enforced, since it is by comparison alone that we are enabled to form anything like a just estimate of

the nature, extent, and progress of the disease. In some individuals, for instance, in perfect health, there exists loud resonance of voice under both clavicles: again, instances have been recorded in which the voice beneath both scapulæ constantly preserved the ægophonic character. Such being the case, no prudent physician, however versed in stethoscopic practice, will venture to pronounce his diagnosis, without due comparative examination.

Ægophony is occasionally heard also, as stated by Stokes, and others, during the resolution from hepatization, as well as in the stage of purulent infiltration. In the present instance the ægophony was evidently the result of sudden but slight effusion; since, immediately on the detection of the ægophony, the patient admitted he could *now* lie easier on the affected side: still, however, on attempting to recline on the healthy side, there was neither dyspnœa, nor any aggravation of the more prominent symptoms.

That the resolution of solidity is not always accompanied by a return of the crepitating râle, the above case demonstrates: this fact, though contrary to the statement of Laennec, is noticed by Dr. Stokes in his valuable treatise on diseases of the chest. In the above patient, the *rhonchus crepitans redux* of Laennec was distinctly audible previous to the relapse: subsequently to this occurrence, however, although the solidity has slowly and progressively resolved, no rhonchus of any kind, indicating this change, could be detected, but a gradually increasing return of the natural vesicular murmur.

Contraction of the chest is not an unfrequent occurrence during the progress of chronic pneumonia. It appears to exist principally in cases in which the lower lobe of the lung has continued for a considerable period in an indurated and impervious condition. In the preceding case the chest was measured at the time of commencing the mercurial treatment, when the diseased side, at its lower portion, measured an inch and a half less than the opposite one. During convalescence it was interesting to observe the progressive expansion of the affected side, in perfect accordance with the gradual reappearance of the respiratory murmur.

In the treatment of acute pneumonia, there appears little diversity of opinion

respecting the utility of blood-letting; but in reference to the period for abstracting blood, the quantity drawn, and the repetition of the measure, practitioners are by no means so fully agreed. Without entering on the various opinions of authors, I shall merely briefly state the summary of my own views in reference to general and local blood-letting.

In an attack of acute pneumonia, if we are fortunate enough to see the patient at the very onset of the disease, even before the crepitating râle is fully developed, and local excitement of the organ is the chief symptom, this undoubtedly is the period in which venesection will produce the most decided impression; next to this, and the one in which we are most frequently called upon to act, is during the stage of sanguineous congestion.

At the first bleeding I have seldom found it necessary to abstract beyond twelve to sixteen ounces: this measure I generally follow by the application of at least a dozen leeches, which, in this instance, I prefer to cupping: on their removal a large cataplasm applied over the bites affords in general much relief.

A repetition of the venesection will be called for, should the symptoms, after the lapse of some hours, still continue with equal intensity: from nine to twelve ounces in the second bleeding will suffice. Seldom is it necessary, in my opinion, to repeat the *general* blood-letting more frequently than as above directed: not so, however, with respect to the local abstraction of blood by leeches, which should be repeated daily, or at intervals of two or three days, until, directed by the stethoscopic and general signs, we are convinced of a permanent mitigation of the symptoms; even should we, as is frequently necessary in the adynamic or typhoid variety, be compelled at the same time to support the strength with food and even stimulants.

After bleeding, the next most important remedies to be considered are antimony and mercury; these, with venesection, have been not inappropriately termed the "physician's sheet anchor." In the selection of either of these remedies, very much must doubtless depend on the peculiar features and circumstances of the case. If the

patient be young and vigorous; if the disease be recent, and assume an inflammatory type; if the crepitating râle be still audible, either generally, or around the solidified portion, indicating an extension of the disease, then in general we shall find the antimonial treatment attended by success. But if, on the contrary, the case be one of relapse, and especially if it have in the first instance been treated by antimony; if the hepatization remain stationary, without signs of resolution; if the powers of life be low, or the disease be complicated, or assume a typhoid character, in these cases the mercurial treatment is decidedly to be preferred and adopted. The manner in which the patient has borne the previous bleeding, added to the firmness of the coagulum, will afford material assistance in the choice of the remedy. My experience does not, I confess, bear out the utility of prescribing such large doses of tartar emetic as are recommended by many authors. I have seldom found it necessary to order larger doses than one-third to half a grain repeated at intervals of three or four hours; the tolerance has soon been established, and the disease in general yielded readily. In the adoption of the mercurial treatment, the system should be affected as early as possible, and in the mildest manner: small doses of calomel and opium, or Hydr. c. Cretâ, with Dover's powder, will in general fulfil every intention required, the absorption after bleeding being usually rapid and easy. In the use of this remedy, as of the former, I am averse to the heroic doses of ten and twelve grains of calomel, as recommended by some writers on pneumonia.

Dr. Christison, in a recent paper*, strongly recommends the revival of the practice of treating acute internal inflammation by full doses of opium after blood-letting.

The adoption of counter-irritation, and especially blistering, has given rise to occasional discussion, and some difference of opinion. Without analyzing the views of authors on this subject, I may briefly state, that after the removal of the inflammatory symptoms, when the pulse is soft, and the

* Edinburgh Monthly Journal of Medical Science for Feb. 1841.

pungent heat of skin is somewhat subdued, and especially in all cases of typhoid and complicated pneumonia, the application of a blister is attended by the happiest results. Large blisters, in the first instance, are preferable to small, since the irritation produced by them is by no means commensurate with their size, and the sanative effect is much increased. When, however, the disease has become latent, or chronic, the repeated application of small blisters is of the greatest service in promoting its resolution.

During convalescence I often find very considerable benefit accrue from ordering the entire affected side to be sponged night and morning with a turpentine liniment, a portion of which being absorbed, acts with double efficacy.

A few words on diet and regimen during convalescence, will close the remarks I have to make on the treatment of this affection. Few diseases require the vigilance of the physician more constantly during convalescence, in reference to diet, than pneumonia, since the slightest indiscretion, on the part of the patient, serves in many instances to fan the waning disease again and again into an active state. After the reduction of the inflammatory symptoms, the food must at first be strictly farinaceous, to which we may add a light vegetable diet of asparagus or sea-kale: if the desire for food progressively increase, light puddings, with the weakest mutton or chicken broths, may be allowed. From this we may ascend the scale to boiled sole, haddock, or whiting; mutton, beef, and game, are the most digestible of meats; after which the patient may gradually return to his usual fare. The same cautions must be strictly adhered to in reference to tonics and stimulants. The temperature of the sick-room should never exceed 60° Fahrenheit; flannel should be worn next the skin on recovery; and, as early as convalescence permits, change of air, if compatible with the patient's circumstances, will prove of especial benefit. I need scarcely add, in cases of latent or chronic pneumonia, the necessity of constant and accurate stethoscopic exploration, with a view to guard against and detect the earliest appearance of tubercle, especially if phthisis be hereditary in the family.

CASE II.—*Hydruria, with deposition of Phosphates.*

Jane Barker, æt. 38, residing in the country, married, has had three children. Complained, on application, of general debility and depression of spirits, want of sleep, with great disinclination for any kind of exertion; almost constant pain in front part of head, together with occasional severe pain in right hypochondrium, not increased on pressure, with considerable weakness and "gnawing" sensation in loins, pubes, and thighs. Her expression was care-worn and anxious, with an evident tendency to view all her affairs in a dark light.

States she is the subject of slight prolapsus uteri, with more or less leucorrhœal discharge. Catamenia have lately been very irregular, sometimes returning at the fortnight in profuse quantity, amounting to menorrhagia; at which period she suffers pain, but passes no clots; has almost constant desire to evacuate the bladder. Complexion sallow; skin dry and unperspirable; eyes dull and heavy, the lower lid bounded by a broad, dark, semicircular line; some cough, with palpitation of heart; pulse 86, small, and feeble; tongue moist, with white coating; clammy unpleasant taste in mouth; no thirst; appetite much impaired and variable; occasional vomiting; bowels constipated; urine limpid, in large quantity, of a pale-straw colour, becoming slightly flocculent on cooling, and forming a pellicle on the surface; does not affect litmus paper; sp. gr. 1.005; not coagulable by heat or nitric acid. The quantity of urine voided in twenty-four hours could not be ascertained, on account of the patient residing in the country. On examining the chest, respiration was normal, but weak; action of heart irritable, free from adventitious sound, and otherwise natural; right lobe of liver evidently somewhat enlarged.

The indications for treatment being clearly the improvement of the general health, after premising the use of the perinæum cushion and bandage (as recommended by the late Dr. Hamilton, of Edinburgh,) for the prolapsus uteri, she was ordered a stimulating embrocation to be applied to the side night and morning, and the following medicine:—

Rx Pil. Rhei Comp. gr. viij.; Ex. Hyoscyam. gr. iij. Ft. pil. duæ om. n. sumend.

Rx Sodæ Carbon. ʒj.; Inf. Gentian c. ʒv.; Tinct. Rhei Co., Syr. Zingib. aa. ʒiv. M. sumat cochlearia duo magn. ter in die.

In a few days she reported herself better; bowels now regular, no return of menorrhagia; urine still in large quantity, and pale; sp. gr. 1.006; appetite somewhat im-

proved; has slept better; skin continues dry, harsh, and unperspirable.

Contin. Mistur. \mathcal{R} Pil. Rhei Co. gr. v.;
ft. Pil. om. nocte sumend. \mathcal{R} Pulv.
Ipecac. Co. gr. x.; ft. Pulv. o. n.
sumend.

Did not return for some days, when she stated she felt much more languid and depressed both in mind and body, all her symptoms having returned, with the addition of a copious deposit of "white powder" in her urine, which still continued to annoy her, both by its quantity and the frequent desire to empty the bladder. On examination it was now alkaline, of the same low specific gravity, not coagulable, and the deposit soluble on the addition of nitric acid.

\mathcal{R} Tinct. Opii 3j.; Acidi Nitr. Dil.
3vij. M. Sumat. gtt. xx.; ter die
ex aquâ. Contin. P. Ipecac. Co. om.
noct.

On next application she was much improved in health, and in better spirits, though still irritable; quantity of urine decreased; sp. gr. 1.008.

Contin. Medicament. Aug. Tinct. Opii
ad 3ij.

From this period her health was considerably restored; she was gaining flesh and strength, and expressed herself very grateful for the benefit received. The urine still remained in somewhat abnormal quantity; sp. gr. 1.012; was ordered to continue the medicine, did not again apply, and I have since heard she "is doing finely."

OBS.—The above case affords a good illustration of the general symptoms of hydruria, coexisting with those which so frequently obtain towards the close of the menstrual secretion. The absence of thirst in the present instance deserves notice, since the desire for drinks, in all cases characterized by an increased flow of urine, is in general one of the most prominent, and frequently a very distressing symptom, especially as, in reference to the treatment, it is necessary to regulate the quantity of fluids taken, with a view to curtail the function of the kidneys as much as possible.

Another point deserving attention is the low specific gravity of the urine. This symptom remained constant, and, as far as I could ascertain (the patient residing in the country), did not vary, as it frequently does, when taken at different periods of the day.

In treating this affection the principal indications are the improvement of the general health, and the diminution of the excessive irritability which

pervades the system. With a view to the first intention the diet must be carefully regulated, no more food being allowed than the stomach will easily and completely digest; and this should be selected with reference both to lightness and nutritious quality. The quantity of fluid allowed should be no more than is absolutely requisite; and must not be permitted to be taken with a view to quench the sometimes immoderate thirst.

A warm bath taken on alternate nights, is frequently of great service. The medicines most useful in this affection are, the vegetable and ferruginous tonics, combined with opium. Where the phosphatic diathesis prevails, the dilute mineral acids, with opium, as prescribed in the above case, are especially indicated. Ten or twelve grains of Dover's powder should be taken every night at bed-time, which may be repeated with advantage in small doses during the day. The bowels must be carefully regulated by mild aperients: a combination of aloes, rhubarb, and hyoscyamus, with from half to a grain of ipecacuanha, will in general fulfil every intention. In the foregoing case the aloes, for evident reasons, was omitted. Mercurials, in this affection, are injurious. Flannel should be worn next the skin.

CASE III.—*Dysuria following an attack of measles.*

William Capper, æt. 10, from exposure to infection was attacked with rubeola in its simple uncomplicated form. In a few days, however, the symptoms assumed a considerable degree of severity, being complicated with sore-throat, and a smart attack of acute bronchitis. Complained of headache; pulse 90, small and feeble; bowels confined; tongue red at apex and edges, coated in the centre; eyes suffused; urine natural in quantity, high coloured, strongly acid, depositing a red sediment, not coagulable. Leeches were applied to the chest, general blood-letting being deemed inadvisable from the great vital depression; purgatives and diaphoretics were administered, with small doses of carbonate of soda, to neutralize the hyperacidity of the urine. Under this treatment the symptoms were speedily removed, and the patient became rapidly convalescent. Urine still remained acid. The eruption had entirely disappeared, and the case was progressing favourably, when the patient complained suddenly of difficulty in making water, accompanied with pain at the neck of the bladder, sensation of heat in the urethra,

and some pain in the lumbar region. Pulse 86, small and weak. On examining the hypogastric region, slight pain and tenderness were experienced on pressure; the lips of the urethra were swollen, and its lining membrane redder than natural; the small quantity of urine passed was of a deep orange tint, strongly acid, transparent, but depositing a little sediment; on cooling, sp. gr. 1.027, not coagulable.

Was ordered — Hirudines xii. perinæo; subsequently to remain in a warm bath, 98°, for half an hour; statim sumat. Ol. Ricini ʒvj.; Tinct. Opii ℥xx. M. To have Haust. Salin. Aper. c. Antim. Pot. Tart. gr. $\frac{1}{8}$, quartâ quâque horâ.

Bowels were freely opened. The pubes and perinæum to be assiduously fomented. Urine passed more freely, both while the patient remained in the bath, and on leaving it. Six more leeches were applied to the perinæum, and the following prescribed.

℞ Sodæ Carbon. ʒij.; Tinct. Hyoscyam. ʒj.; Inf. Sennæ Co. ʒij.; Mist. Camph. ʒiv.; Syr. Simp. ʒvj. M. Cochl. duo mag. 4ta qq. horâ.

℞ Pulv. Ipecac. ʒi. Co. gr. x. om. noct. s.; bis in hebdom. descend. in Baln. Cal. Linseed tea and barley water to be drank freely.

Urine now became less acid, and was passed without difficulty. Sp. gr. 1.025. Was ordered — Tinct. Ferri Sesquichlor. ℥x. ter die, and from this period he gradually recovered.

OBS.—Difficulty of making water occurs more frequently during the progress of, than subsequent to, an attack of rubeola; it is an affection of much rarer occurrence than many other complications of this disease, yet at the same time one requiring vigilance for its early detection, and prompt treatment for its cure.

In the above case, the occurrence of the symptoms, and mode of attack, are well marked, together with the effect of the remedies employed. Probably, had general blood-letting been had recourse to in the first instance, on the supervention of the acute bronchitis, the dysuria would not have occurred; such, I confess, is my present opinion; but so great was the general debility and depression caused by the measles, that, as before stated, venesection at that period was not thought advisable. The cause of this affection appears to consist in an inflamed condition of the vesical mucous membrane, and espe-

cially in a congested state of the vessels of that membrane, more particularly in the portion surrounding the neck of the bladder. The irritable state of the parts renders this viscus incapable of containing more than a very small quantity of urine, and this irritation extending to the sphincter of the bladder and Wilson's muscles, increases the difficulty of making water, and strangury, or even complete ischuria, occasionally result.

In the foregoing case, the phlogistic state of the mucous membrane appears to have been of metastatic origin, for immediately on the removal of the bronchial inflammation we find an attack of a similar nature set up in the mucous membrane of the bladder.

The introduction of the catheter has been justly reprobated in the treatment of this affection; it should be used only as a *dernier ressort*, other remedies having proved ineffectual: "since then," says Mr. Syme, "the additional irritation thus occasioned is more than counterbalanced by the good effect of emptying the bladder, which, when stimulated by distention, reacts upon the spasmodically contracted muscles at its neck, and increases their resistance." If the introduction of this instrument be found necessary, its withdrawal should be immediately followed by the exhibition of an opiate enema, and after a proper interval a full dose of castor oil.

A similar treatment to that adopted in the preceding case will in general effect a cure. Blood-letting, especially locally from the perinæum, hot fomentations, the warm bath, full purging, followed by saline antimonial aperients, will usually afford speedy relief, without the aid of the catheter. Should complete ischuria supervene, the Tinct. Ferri Sesquichlor., administered in small and repeated doses, so as to keep up a continued feeling of nausea, is often of signal service.

A FEW OBSERVATIONS ON INSTINCT, &c.

To the Editor of the Medical Gazette.

SIR,
THE following short memoir originated in memoranda which I made whilst engaged in reading an essay, or series

of essays, a considerable time past, in Blackwood's Magazine, and having been then too busily occupied in other matters, I neglected and for a long time forgot the subject: the writer in "Blackwood" tried to prove that there were neither feelings nor sensations in the insect tribe! which strange opinions I hope to refute, although he has brought very powerful reasoning to support his side of the question.

Amongst his first and leading propositions he advances this one, which, most indubitably, if he could prove, would very soon settle all dispute in the case: "that there can be no sensation without distinct nerves of sensation." Then I answer, that if such hold good, there can be no objection whatever why the following should not be added as a counterpart of that proposition, namely, "that there can be neither motion nor digestion without distinct nerves in accordance with these respective functions." So how this dilemma is to be got clear of is rather beyond my comprehension at present; because what would become of the Entozoa, Polypi, and such like creatures, which exist in the lowermost grades of animal life? they are long well known to possess neither distinct nerves of motion nor digestion, nor in fact distinct nerves for the performance of any other living process in the animal economy. Consequently, according to the opinion brought forward above, these beings should not be possessed of either digestion or motion; but, rather unfortunately for the soundness of his deduction, these little creatures are well known to possess both motion and digestion; and on equal grounds I would conclude that they—even they, although so much inferior, so to speak, to insects—are possessed of sensation also. A German author states that in several animals, particularly among insects, an acute sense of smell seems to exist, although no part can be pointed out in the head which analogy would justify us in describing as a nose; so, of course, no distinct nerve of smell, when there is no organ apparently for it; yet notwithstanding all that, they are proved to be endowed with the function of smell.

He next advances, "that motion may be without pain, and that pain

may be without motion:" but if we apply to a living organism (any part which is sensitive in a living animal being) an exciting cause that amounts to irritation, we are reasonably to infer that pain is produced, because violent action is called forth in trying to get rid of the cause of annoyance; and how is the living being to call those parts into action unless it have some powers consistent therewith; unless, in fact, it have nerves, or something similar, whether distinct to our eyes or not? It should be recollected that the only way to account for a nervous system being present in the lowest grades of animal existence—where the most active digestion (and motion in some) go on, although neither brain nor nerves of any kind are to be found, is, that the nervous matter is combined in some hitherto unknown way with the general mass of tissues; and motion may be the more inseparable from pain, in those very classes of which he speaks, just because of the imperceptibly and inextricably close junction and interlacement, or even amalgamation, of the nervous filaments of motion and sensation. I may mention that the author to whose essays I allude has wished to be understood, if I mistake not, that the insects, &c., which anglers make use of, suffer no pain whatever—no matter their motions should indicate the contrary when placed upon the fishing-hook; and this, forsooth, by reason of his not being able to find any distinct nerves of sensation in them. Now, by parity of reasoning, I think I have shewn that myriads of animated beings should neither digest, move, smell, &c. &c.; but the conclusion of the argument is, they are found to do all these things, although we cannot see in them any distinct nerves to answer for the performance of such operations. Perhaps it may be requisite for me to premise, that in man, &c., there are distinct nerves appropriated respectively to motion—that is, to call the various muscles into action—and others distinct for sensation, which means the common sense of feeling or touch over the body; like as others distinct for vision, hearing, &c.: but we are not for a moment to infer that if this high state of organization is not

found in all animals, such animals consequently must be in complete want of certain functions or powers: no; we find the organs changed and varied almost indefinitely, to suit the adaptation for the varying circumstances, medium, and contingencies, to which each family of living beings is subjected; and although the organs may be equally changed, yet their functions are found nearly always similar.

It appears singular that any person should for an instant question the validity of the generally received fact, that insects, &c. possess the sense of touch, and if they possess that sense, which is evidenced by the application of excitable causes, as surely must pain be produced in them by subjecting them to irritating stimuli; and this is soon perceived by the violent efforts on their part, and these observations are applicable to worms, &c.

The antennæ, or feelers of insects, seem to be the very concentration, so to speak, of sensitiveness, and the sense of touch in spiders is well known to be extremely acute. Now if we were to reason in full analogy with ourselves, we would say that whatever part is highly endued with common sensation, such must in equal ratio be susceptible of pain; but there are, no doubt, great diversities in amount of sensitiveness, whether general or local; as for instance the horny cuirass of the insect compared with the antennæ; or our own teeth and nails in a diseased state compared with their healthy condition: so that a part which may have very little common sensation at one time and in one state, may soon become quite reversed; or a part which has considerable touch, and which in the healthy state may be stimulated without causing much pain, yet if it become diseased, and then stimulated, will give dreadful pain. There are other varieties of sensibility, as that of the stomach, &c.; and these vary very peculiarly, and would be unknown to us by any *à priori* reasoning; such as that in caterpillars, whereby the poison of nettles is innocuous; or that in birds, by which they can eat the berries of the mezereon.

What is instinct? It appears inscrutable. A bee returns home before evening, as his instinct directs, and

numerous other instincts the bee is well known to possess, many of which are wonderful and almost incredible; the instincts of the bee also far outnumbering those of even the elephant or dog, which must render it a problem of rather difficult solution to the phrenologist, it being a generally received opinion that the larger the encephalon or cerebral ganglion (which means the brain respectively in the upper and lower classes of animals) there will be found the more intelligence and instinct. This, however, does not always hold true, either relatively or absolutely; as instance the enormous brain of the dolphin, the canary, &c. and the diminutive brain of the wild boar, and many fish, &c., in none of which is exemplified either a great amount or marked deficiency of instinct.

If the author before mentioned do not admit such things as *feelings* in the lower tribes, how will he explain why a mouse should be frightened and horror-struck, almost out of its very existence, by seeing a serpent, although it had never seen, or perhaps *heard*, of one before? How is fear produced in him by remembering any thing hurtful? May it not be produced by instinct, as is above exemplified, and that equally as where memory suggests? There are many instinctive actions performed where memory could never have been exercised, or even almost had time to be called into action. What shews instinctive fear, as also cautious boldness, more than a flea? What could possibly exhibit more the pleasure and delight obtained by the eyes from light than a moth, as observed rushing to and gyrating round the flame of a candle with all the glee imaginable, until it become fascinated so far that it is induced to go too near, and so, through its over desire for enjoying the light, loses its life? We are not required to look on these as what, in our bounded and finite conceptions, we might almost consider mental operations; such may not be, and is not, a proper term. But we should recollect that instinct is an all but undeviating intuitive source of the motives to action in the lower grades, or rather in all grades: in some few matters purely instinctive, not even man excepted, such as the sucking of the infant, &c.; and when we descend in the scale to insects, who

have only what is termed a ganglionic system of nerves, and who have, properly speaking, no brain, and whilst we find among these little creatures the most marvellous instinctive actions—actions far more requiring, in common parlance, a brain, according to phrenological computations, than do the comparatively few instinctive operations of the generality even of the mammalia—we are obliged to stop short, and look on with admiration and amazement, because the largely-developed cerebra or brains of the mammalia are adduced as criteria and proofs of their several amounts of intelligence: then if such were absolutely requisite for the development of instinct, &c. how will it be explained that a superior and greatly extended series of instincts exist, as in the insect tribe, where there is nothing that can properly be said to correspond with the brain as it exists in mammalia, birds, &c. almost at all?

An eminent author has lately given us the following distinguishing trait, as affording a line of demarcation between instinct and reason:—"that animals acting by instinct are rarely, if ever, competent to make the most trifling changes in their modes of action, no matter what contingencies may occur to require such changes."

We must probably rest satisfied at present with respect to the amount of our knowledge in many things; and, in reference to instinct, we are totally unable to fathom what that is which is implanted in the lower grades, we should say, of all kinds in the creation, according to his kind; it is a something, we perceive, which, ordered by Almighty fiat, can, and does, continually impel to actions, either through the medium of a cerebro-spinal system, as in the mammalia, birds, reptiles, and fishes; or through what is termed the ganglionic or splanchnic system, as in insects, spiders, crustacea, worms, and mollusca; or, finally, we find it impelling to action where no nervous system of any description can, apparently, for so far, be made visible to our eyes, even upon the field of the most powerful microscope, aided by the most minute dissection—as in the zoophytes.

How else could we explain the actions of those last-mentioned existences, whose organs and functions are so simple, so few, and so uncomplicated;

consequently termed the lowest in the gradation of animal organization?

Or how should we be competent to explain the state of desperation exhibited by the fly at the sight of a spider? or the manifest delight of a geometric net-working spider when engaged whirling with rapidity round a butterfly, encircling and fettering its wings by the coils of the gossamer-like thread thrown around them?

Or, to take an instance in a higher range, how could the greatest philosopher ever attempt to give a rationale for the apparent joy, as exhibited by a young turkey, just escaped that instant from its shell, seeing a fly, and no sooner seeing than darting at and swallowing it? Than this there could scarcely be adduced a more trite and positive specimen of pure instinct; and many such, if necessary, might be added, where the actions could not possibly result from previous impressions dwelling in the memory.

I find I have not yet exhausted all my notes on the subject, but shall, for the present, come to a conclusion: it is difficult bringing such matters down to a state fitted for the general reader, yet I hope, to a certain extent, I have succeeded, as I have excluded some profound and debateable physiological points altogether, which it were scarcely possible to render intelligible, excepting to those initiated in the observational sciences.

I hope it will now be seen and granted that insects, &c., although not possessed of distinct nerves, yet may have the faculty of touch, and, having that, must, reasoning consequentially, be liable to pain. We have also shewn that they most unequivocally do exhibit a variety of feelings or emotions, or whatever metaphysicians may define them; and what reason is there why these should not be implanted in them, when we find that in many there are the most truly wonderful indications of what might be termed intellectual instincts, as in ants, bees, wasps, spiders, &c., and why should we deny to them emotional instincts?—I am, sir,

Your obedient servant,
GEORGE HILL ADAMS, M.D.E.

Chichester Street, Belfast,
Nov. 10, 1840.

ON STAMMERING,

AND THE METHODS PROPOSED FOR ITS
REMOVAL.

BY EDWIN LEE, ESQ.

[*For the Medical Gazette.*]

PREVIOUS to entering upon the consideration of stammering, I shall briefly notice some points connected with the functions of the voice and speech.

The first condition necessary for the production of the voice is that the air contained in the lungs should be carried, by the action of the muscles of expiration, against the vocal chords, during its passage through the larynx; but as this takes place in breathing without sound being occasioned, the action of the small muscles which enter into their composition, under the influence of the will, is a condition no less essential than the former. The quantity of the air, the greater or less rapidity with which it strikes the vocal chords, and the varying degrees of tension or contraction of these latter, are the circumstances which cause the innumerable modulations of sound in speaking, singing, and other exercises of the voice.

This function may then be considered as intimately connected with, and as superadded to, that of respiration; and its performance depends entirely upon the lower vocal chords, as, if the recurrent or inferior laryngeal nerve, which supplies them with nervous energy, be divided or paralysed, the voice is lost, which is not the case when the upper vocal chords are deprived of power by the lesion of the superior laryngeal nerves, which, as well as the recurrent, are branches of the eighth pair. The voice may also be impaired or lost by lesions of the brain affecting the origin of the nerves, or by moral impressions and other causes which interfere with the exercise of volition, of which the nerves are merely the agents or conductors, as far as muscular action is concerned; and it is liable, as is well known, to a variety of alterations, according to the state of excitement or depression of the cerebral faculties, as illustrated by the various passions and emotions—joy, anger, fear, sorrow, &c.

Of the cerebral nerves, some are exclusively agents for the transmission of special sensation, as the first and second pairs, and a portion of the

seventh (which, however, is essentially a separate nerve) for smell, sight, and hearing. Others serve solely for the transmission of volitions, and the performance of muscular action, as the third, fourth, sixth, a part of the seventh, and the ninth pairs; while others again, as the fifth and eighth, are agents both of sensation and muscular motion.

It is scarcely necessary to repeat the obvious truism, that speech cannot be produced without the voice; but the voice may exist, and indeed does exist in the greater number of animals, without speech, which is peculiar to man, being bestowed upon him for the expression of his thoughts and ideas. Idiots, who have no ideas to express, are generally dumb, or can merely articulate a few words or phrases without meaning, or which they know will cause their ordinary wants to be supplied, but they are incapable of conversing. Speech, however, is not a natural gift, but is acquired by education; hence those born deaf are also dumb: and instances are on record of individuals who have been found in solitudes far from the haunts of men, who, though not deaf, were dumb, and had acquired the voice of animals with whom they had associated, but who were afterwards taught to speak. Dumbness, therefore, does not consist in the absence of voice, but of speech and the deficiency of ideas, as most dumb persons can utter a variety of inarticulate sounds; but it by no means follows that a facility in speaking necessarily exists in proportion to the quantity of the ideas or the power of the mind. Many men of genius and great imaginative powers are but little talkers, and some not unfrequently experience difficulty in expressing their thoughts on any particular subject; while empty-headed fellows are frequently very loquacious, and, like Gratiano, “speak an infinite deal of nothing;” for—

“Talking is not always to converse;
Not more distant from harmony divine,
The constant creaking of a country sign.”

And, as has been observed by a powerful writer, a great fluency of speech is frequently owing to a scarcity of matter and a scarcity of words, “for whoever is a master of language and has a mind full of ideas, will be apt in speaking to hesitate upon the choice of both; whereas common speakers have only

one set of ideas, and one set of words to clothe them in, and these are always ready at the mouth: so people come faster out of a church when it is almost empty, than when a crowd is at the door*." On the other hand, many highly intellectual men are great talkers, and not unfrequently jest or talk nonsense by way of relaxation; so that silence and gravity of demeanour are not always a proof of superior wisdom, though they are frequently considered so by the multitude, and are in consequence sometimes assumed by persons who either have nothing to say, or who wish to impress others with a sense of their superiority†.

"Since silence seems to carry wisdom's power,
Th' affected rogues, like clocks, speak once an hour‡."

Such individuals are likewise noticed by Shakspeare, as the sort of men

"Whose visages
Do cream and mantle like a standing pond,
And do a wilful stillness entertain,
With purpose to be dressed in an opinion
Of wisdom, gravity, profound conceit."

The immortal bard adds—

"I do know of these
That therefore only are reputed wise
For saying nothing."

A complicated mechanism is required for the correct articulation of words or speech, in the production of which various parts are concerned, which should be in a perfect state, and act in harmony. If a portion of the upper or lower jaw, or if the bony palate, be destroyed, if many of the teeth be lost, the speech will be more or less affected. A similar effect will ensue if the soft palate be perforated by ulceration; if the passage through which the air has to pass be narrowed, or its sensibility increased by inflammation or swelled tonsils; if there be paralysis or other serious lesion of the muscles of the cheek, lips, or tongue, though this organ may be in great part extirpated without speech being lost§. Now, the

* Swift.

† "Reserve is no more essentially connected with understanding, than a church organ with devotion, or wine with good nature."—*Shenstone's Essays*.

‡ Dryden.

§ A case is recorded by Tulpus (*Observationes Medicæ*), which is quoted in the *Dictionnaire de Médecine*, of loss of speech after extirpation of one-half of the tongue, but in which speech was restored after an interval of three years, in consequence of the fright caused by a storm. Here the moral impression from the operation doubtless occasioned aphonia.

pharynx, fauces, the soft palate, the uvula, the tonsils, derive their nervous influence from the eighth pair (which we have seen supplies also the laryngeal muscles), by means of the glosso-pharyngeal branch (which, however, is considered by some anatomists, Gall and Bichat, &c. as a distinct nerve), by means of which their muscles contract on the application of their appropriate stimuli—food for deglutition, the air for the formation of speech, &c.—being but partially under the control of the will as far as other actions are concerned. This nerve likewise endows these parts with their peculiar sensibility, and also supplies the muscles of the posterior part of the tongue, being finally distributed to the papillæ on its surface; and is in all probability not only the nerve of taste, but is likewise concerned in some of the movements of the base of the tongue, the associated action of which is necessary in speaking, in deglutition, &c.* It forms inosculation with the respiratory nerves, and with the sympathetic, participating in their healthy and disordered conditions. The tongue is likewise supplied by the fifth pair for common sensation, and by the ninth, which, being purely a nerve of voluntary motion distributed to the muscles, brings it more directly under the control of the will than those parts which are supplied by the glosso-pharyngeal; as may be seen on comparing the flexibility, rapidity, and great variety of the movements of this organ, with the limited action of the fauces†. The same remark applies to the muscles supplied by the portio dura, which is also exclusively a motor nerve.

In the article *Bégaiement*, contained in the "*Dictionnaire de Médecine et Chirurgie pratique*," (which I read two or three days ago), I am happy to find an opinion which I expressed on the cause of stammering at the Westminster Medical Society, corroborated by the high authority of M. Magendie, who says, in the above-mentioned article—

* Galvanism of the trunk of this nerve, after death, produces contraction of the muscles of the fauces, &c. which it supplies.

† The size of the lingual nerve has been found to vary in a direct ratio to the quantity and rapidity of the movements of the tongue, either for mastication, the prehension of food and drink, as for the exercise of the voice. It is consequently larger in dogs and cats than in the ruminating animals and in the rodentia. — *Sarlandière, Traité du Système Nerveux*.

"Of the muscles which serve for articulation, some are subject to the will, as those of the lips and cheeks; those which carry the point of the tongue upwards or forwards, to protrude it out of the mouth; and those which draw it back. But the other muscles of the tongue, viz. those which carry its root upwards, downwards, or backwards; the muscles of the velum palati, those of the pharynx and of the larynx, are only incompletely under the influence of volition,—thus, what a great difficulty most persons experience when requested to show the physician their throat or fauces. We may well tell them to depress the tongue, because it hides the tonsils; they make many efforts, and it is more by chance than by a true influence of the will that the desired action is obtained. If they be required to raise the velum palati, the will is most frequently powerless: and it would fail entirely in the attempts to contract separately the muscles of the pharynx or larynx. The muscular organs only act in a complete manner to attain a certain end—deglutition. In order for deglutition to be performed there must be something to swallow, even if it be only a little water or air. Thus, in one point of view, the muscles of deglutition are not subject to the will.

What has been said of deglutition is applicable to other phenomena which take place in the mouth. Nothing is so simple as to gargarise, to spit, &c.; but in these instances, as in swallowing, we attain the end by means of organs which serve us, without knowing exactly the part which each takes in the action. It is the same with respect to the production of sounds in the larynx; and in speaking we form the voice, we articulate, without knowing exactly what movements take place either in the larynx or in the mouth. There are numerous vocal phenomena of which the mechanism is yet far from being known to the physiologist. We will the end, and we attain it; that is all."

In other parts of the body, the movements of which are completely under the influence of the will, as the trunk and extremities, a certain degree of harmony, or unity of action, must exist not only between certain sets of muscles, but also between muscles and their antagonists. When the flexors, for

example, are employed in the performance of an action, their antagonists are not altogether relaxed, but serve to modify the muscular contraction, in the appreciation of the degree of tension and resistance, in the preservation of the equilibrium of the body, &c. as seen in the instance of rope-dancing and other feats of agility, in which this harmony is carried by education and habit to the highest perfection; whereas the deficiency or want of it is illustrated by awkwardness, and in cases of drunkenness or chorea. Let two boys attempt for the first time to slide upon the ice, or to skate; one will preserve his equilibrium tolerably well, while the other will probably fall down every two or three minutes. Or, let them be taught to dance or fence; one will acquire easy and graceful movements without difficulty, while the other will probably retain much of his awkwardness, notwithstanding the instructions of the master. This greater or less aptitude to acquire perfection in muscular exercises, as well as the difference which is seen in the gait of individuals, depends, then, upon the greater or less degree in which this harmony exists between different parts of the muscular system, which derive their nervous power from the brain.

[To be continued.]

STATISTICAL ACCOUNT
OF THE
AMPUTATIONS PERFORMED AT
THE NORTHERN HOSPITAL,
LIVERPOOL,

From March 1834 to March 1841.

To the Editor of the Medical Gazette.

SIR,
SHOULD the following statistical account of the amputations performed at the Northern Hospital, Liverpool, from March 1834 to March 1841, be deemed of sufficient interest to merit a place in your valuable and highly useful journal, I shall feel obliged by its insertion. I regret that the records of the hospital do not enable me to give more ample details, with the post-mortem appearances of the unsuccessful cases; but as it is, it may perhaps be not unacceptable to some of your readers. Though containing comparatively few cases, it may yet,

if taken with others of a similar description, lead to some useful results, more especially the formation of an accurate estimate of the mortality in amputations of the extremities; and it is only in the hope that it may assist in doing so, that I am induced to lay it before the profession. I may here briefly mention that the circular operation was performed in nearly all the cases, and an attempt made to procure union by the first intention, which very frequently succeeded, if not completely, at least in great measure. The dressings generally consisted of slips of adhesive plaster, by which the lips of the wound were kept in apposition, over these lint spread with simple cerate, and the whole supported by a bandage: occasionally, instead of the lint and ointment, water dressing was applied. Ligatures of common stay silk were used, one end being cut away. They generally became detached from the tenth to the fourteenth day; but in one remarkable instance one of them remained attached seven weeks, causing no other inconvenience than a slight suppuration along its track.

Statistical account of seventy-five cases of amputation, performed on seventy-three patients at the Northern Hospital, Liver-

20 were under 20 years of age, of which number 18 were cured, and 2 died.									
15	were between	20 and 30	-	-	-	12	-	-	3
13	-	30 and 40	-	-	-	8	-	-	5
10	-	40 and 50	-	-	-	7	-	-	3
12	-	50 and 60	-	-	-	5	-	-	7
1	-	60 and 70	-	-	-	1	-	-	0
71					51			20	

Of two of the patients the age is not mentioned.

From a consideration of the foregoing tables, I think the following conclusions may be drawn:—

- 1st. That amputation is a more dangerous operation than is generally supposed, the proportion of fatal cases being 1 in $3\frac{1}{2}$.
- 2d. That it is more successful when performed for diseases than for accidents.
- 3d. That secondary is very much more fatal than primary amputation.
- 4th. That amputation of the lower is more fatal than of the upper extremity.
- 5th. That the danger increases with the age of the patient.

On comparing the results afforded by the preceding tables, with those of the Massachusetts and Pennsylvania Hospitals (to which, as noticed in the British

pool, from March 1834 to March 1841.—
See pp. 271, 272.

Total number of amputations 75 on 73 patients: of these 53 were cured, 20 died; being 1 in $3\frac{1}{2}$.

49 were accidents, of which 14 died, being 1 in $3\frac{1}{2}$; 24 were diseases, of which 6 died, being 1 in 4.

Of the accidents, 40 were primary (performed within 24 hours), of which 8 died, being 1 in 5; 9 were secondary, of which 6 died, being 2 in 3.

Of the primary amputations, 29 were on the lower extremity, of which 6 died, being 1 in $4\frac{8}{10}$; 11 were on the upper extremity, of which 2 died, being 1 in $5\frac{1}{2}$.

Of the secondary amputations, 7 were on the lower extremity, of which 4 died; 2 were on the upper, both of which died.

Of the amputations for diseases, 21 were of the lower extremity, of which 5 died, being 1 in $4\frac{1}{3}$; 3 were of the upper extremity, of which 1 died.

Of the whole number of amputations, 57 were on the lower extremity, of which 15 died, being 1 in $3\frac{8}{10}$; 16 were on the upper extremity, of which 5 died, being 1 in $3\frac{1}{3}$.

Below knee.—28 for accidents, of which 8 died; 9 for diseases, of which 3 died.

Above knee.—8 for accidents, of which 2 died; 12 for diseases, of which 2 died.

Below elbow.—9 for accidents, of which 3 died.

Above elbow.—4 for accidents, of which 1 died; 3 for diseases, of which 1 died.

and Foreign Medical Review, I am indebted for much valuable assistance), it will be immediately seen how strikingly they agree in leading to the same conclusions, though there are one or two minor particulars, in which some difference exists. In the cases here mentioned amputation above seems to have been more successful than below the knee, and operations on the upper extremity more fatal than in the reports alluded to; but the total number of such cases given here is so small, that it cannot in any degree be said to invalidate the common, and what I conceive to be the correct opinion.

I am, sir,
Your obedient servant,
EDWARD PARKER, M.R.C.S.E.
Assistant House-Surgeon to the Hospital.
Liverpool, April 19, 1841.

Sex.	Age.	Disease or Accident.	Primary.	Secondary.	Result.	Time of Death after Operation.
F.	20	Disease of the ankle - - - -	—	—	Cured.	—
M.	50	Disease of the knee - - - -	—	—	Cured.	—
M.	57	Compound fractured tibia and fibula - - -	P.	—	Died.	10 weeks.
M.	50	Compound dislocation of ankle - - -	—	S.	Died.	Same day ^a .
F.	40	Disease of the knee - - - -	—	—	Cured.	—
M.	32	Injury to the ankle - - - -	—	S.	Died.	Next day ^b .
M.	19	Inflammation of leg, and gangrene - - -	—	—	Died.	^c
M.	38	Compound fractured tibia into ankle - - -	P.	—	Cured.	—
M.	15	Lacerated forearm, and fractured radius - -	P.	—	Cured.	—
M.	40	Compound fractured leg - - - -	P.	—	Died.	3 months.
M.	21	Disease of the knee - - - -	—	—	Died.	^d
M.	14	Disease of the foot - - - -	—	—	Cured.	—
M.	21	Disease of the knee - - - -	—	—	Cured.	—
M.	53	Incised wrist - - - -	—	S.	Died.	3 weeks ^e .
M.	55	Compound fractured leg, with laceration of posterior tibial artery - - - -	P.	—	Cured.	—
M.	25	Compound fractured ankle - - - -	P.	—	Cured.	—
M.	41	Diseased leg - - - -	—	—	Cured.	—
M.	50	Compound fracture of leg - - - -	—	S.	Died.	17 days ^f .
M.	54	Diseased ankle - - - -	—	—	Died.	6 days.
M.	—	Compound fracture of leg - - - -	P.	—	Cured.	—
M.	9	Diseased knee - - - -	—	—	Cured.	—
M.	21	Fractured humerus, with wound of the brachial artery - - - -	P.	—	Cured.	—
M.	30	Compound fracture of leg, with wound of anterior tibial artery - - - -	P.	—	Cured.	—
M.	15	Compound fracture of the femur, with dislocation of the knee, and rupture of the popliteal artery - - -	P.	—	Cured.	^g
M.	53	Compound fracture of both forearms, and extensive laceration of the thigh - - -	P.	—	Died.	2 days ^h .
M.	48	Compound fracture of the carpus - - -	P.	—	Cured.	—
M.	23	Disease of the knee-joint - - - -	—	—	Cured.	—
M.	17	Disease of the knee-joint - - - -	—	—	Cured.	—
F.	50	Compound dislocation of the ankle, with fracture of the tibia - - - -	P.	—	Cured.	—
M.	42	Compound fracture of the leg - - - -	—	S.	Cured.	ⁱ
M.	15	Compound fracture of the thigh, with extensive laceration of the soft parts - - -	P.	—	Cured.	^j
M.	17	Disease of the knee-joint - - - -	—	—	Cured.	—
M.	32	Compound dislocation of the elbow, with laceration of the brachial artery - - -	—	S.	Died.	30 days ^k .
M.	12	Compound fracture of the arm - - - -	P.	—	Cured.	—
M.	37	Gangrene, supervening on fracture of the tibia and fibula - - - -	—	S.	Cured.	^l
M.	27	Caries of the tibia - - - -	—	—	Cured.	—
M.	49	Disease of the ankle-joint - - - -	—	—	Cured.	—
M.	25	Foot torn off by a chain cable - - -	P.	—	Cured.	^m

REMARKS.

^a The operation was not performed until three weeks after the accident.^b The operation was performed two days after the accident.^c Cause of the disease, and time of death after operation, not mentioned.^d Date of death not mentioned.^e Forearm amputated two weeks after the accident, in consequence of gangrene supervening. In the first instance the radial artery was wounded, and a very considerable quantity of blood lost, before he came to the hospital. After the amputation, though much exhausted, he went on very well for some time, when a large abscess formed under the pectoral muscle, under which he sunk.^f Amputated twelve days after.^g In this case the thigh was amputated at its upper third.^h Both forearms were amputated at the same time. The accident was caused by the unexpected discharge of a cannon, and both hands were blown completely off.ⁱ The limb was removed three weeks after the accident.^j The thigh was amputated at its upper third.^k The arm was removed four days after the accident.^l Amputation was required, and performed by the flap operation, ten days after the injury.^m Flap operation.

Sex.	Age.	Disease or Accident.	Primary.	Secondary.	Result.	Time of Death after Operation.
M.	34	Compound fracture of tibia and fibula - - -	P.	—	Cured.	—
M.	36	Compound fracture of the leg - - -	P.	—	Cured.	—
M.	52	Compound dislocation of the astragalus - - -	P.	—	Cured.	—
F.	18	Compound fracture of the tibia and fibula - - -	P.	—	Cured.	—
M.	23	Compound dislocation of the ankle, with fracture of the tibia and fibula - - -	P.	—	Cured.	—
M.	20	Compound fracture of the forearm - - -	P.	—	Cured.	—
M.	19	Hand torn off - - -	P.	—	Cured.	n
M.	6	Disease of the tarsus - - -	—	—	Cured.	—
M.	45	Compound fracture of femur, tibia, and fibula - - -	P.	—	Cured.	—
M.	38	Compound fracture of the leg - - -	P.	—	Died.	3 days.
M.	35	Compound fracture of the leg - - -	P.	—	Cured.	—
M.	11	Hand torn off - - -	P.	—	Cured.	—
M.	14	Burn and disease of the knee-joint - - -	—	—	Died.	5 weeks.
F.	18	Disease of the knee-joint - - -	—	—	Cured.	—
M.	15	Lacerated hand - - -	P.	—	Cured.	o
M.	29	Gangrene of foot, supervening on ligature of femoral artery for popliteal aneurism - - -	—	—	Died.	p
F.	46	Caries of the elbow-joint - - -	—	—	Cured.	—
M.	61	Compound fracture of femur into knee - - -	P.	—	Cured.	q
M.	40	Compound dislocation of lower extremity of the ulna, and comminuted fracture of the radius - - -	P.	—	Died.	7 days ^r .
M.	54	Lacerated foot, with fractured cuboid bone, followed by gangrene - - -	—	S.	Cured.	s
M.	15	Compound fracture of leg - - -	P.	—	Cured.	—
M.	6	Compound fracture of arm - - -	P.	—	Cured.	—
M.	32	Fractured tibia, extending into the knee, with rupture of blood-vessel - - -	P.	—	Cured.	—
M.	41	Compound dislocation of both ankles - - -	P.	—	Died.	3 days ^t .
M.	22	Compound fracture of leg and foot - - -	P.	—	Cured.	—
M.	58	Fractured tibia and fibula, with extravasation of blood - - -	—	S.	Died.	2 days ^u .
M.	28	Comminuted fracture of left foot - - -	P.	—	Cured.	v
M.	38	Fracture of tibia into the knee - - -	P.	—	Died.	9 days ^w .
M.	19	Disease of knee-joint, supervening on erysipelas of the leg - - -	—	—	Cured.	—
M.	—	Extensive sloughing ulcer of arm and forearm - - -	—	—	Cured.	x
M.	32	Compound dislocation of the ankle, with fracture of the tarsal bones - - -	P.	—	Cured.	—
M.	24	Compound dislocation of head of fibula, fracture of tibia and fibula, with laceration of the muscles - - -	P.	—	Died.	7 weeks.
M.	13	Fracture of the tibia, with effusion of blood into the knee-joint - - -	P.	—	Cured.	—
M.	29	Disease of the ankle-joint - - -	—	—	Cured.	—
M.	30	Disease of the elbow-joint - - -	—	—	Died.	26 days.

REMARKS.

n An attack of phlegmonous erysipelas supervened, but he was discharged cured at the end of two months from admission.

o The hand was removed at the wrist-joint. It was a very considerable time in healing.

p Amputation was performed below the knee. The stump had nearly healed, when, two months after the operation, he died suddenly of hæmoptysis, from pulmonary apoplexy.

q In this case one of the ligatures remained for seven weeks, causing only very slight suppuration along its track.

r In this case there was also a dislocation of the hip, and fracture of the fibula. He went on well for three or four days, when hiccup and vomiting commenced, which increasing, soon produced an alarming state of prostration, terminating in death. On dissection, the viscera of abdomen were perfectly healthy.

s Amputation was performed nineteen days after the injury.

t One was amputated the same day, the other next morning.

u Amputated three days after the accident.

v There was also a compound fracture of the right leg, which was cured.

w On dissection, the psoas and iliacus muscles of the same side were softened and decomposed, evidently from previous disease.

x In this case the arm was amputated at the shoulder.

"TRANCE"—PSORIASIS.

To the Editor of the Medical Gazette.

SIR,

I BEG to transmit for insertion in your journal the following account of a remarkable case of trance, which lately came under my observation. I have also subjoined a notice of a peculiar form of psoriasis, which I believe to be a novel species of that disease.

I have the honour to be, sir,

Your obedient servant,

J. M. WINN, M.D.

Truro, April 23, 1841.

On the 17th December, 1840, I was requested to see, in consultation with Mr. Pearce, of St. Austell, a young lady of that town, aged 18, who had been ailing for eighteen months with anemia, disordered bowels, and irregular menstruation. I found her suffering from amenorrhœa of two months' duration, increased action of the heart, with bellows murmur, and a congested state of the left lung; her face was pallid, and her legs slightly œdematous. I was informed that she had been affected with chorea when 13 years of age, but that the complaint subsided on the first appearance of the catamenia. I was also informed that her spirits had always been remarkably good, and that she had never suffered from hysteria. She had been under Dr. Hope's care when in London during the summer, and had taken steel, by his direction, with some benefit; she had also lately resumed the use of iron by Mr. Pearce's advice. We mutually advised the application of a blister to the side, and some draughts, composed of digitalis and a saline aperient. The steel had been discontinued previous to my arrival.

Dec. 10th.—I was informed that she was considerably better on the 9th, but that to-day she was seized, immediately after vomiting some undigested food, with profound coma. She is now lying in a comatose state. When roused she appears to be partly conscious, though speechless, and quickly relapses into an apparently sound sleep. The pupils are sluggish, and the heart beats strongly. With a view of rousing the nervous system, and of remov-

ing effusion, should any exist, we deemed it right to order turpentine injections, and blisters to the legs.

13th.—There has been little change in the symptoms. The bowels and kidneys have acted freely. The heart's action is less strong, and the bellows murmur is considerably diminished. The lungs appear to be entirely free from congestion. In consequence of the debility, ammonia and small quantities of wine were ordered.

15th.—She rallied after taking the stimulus, and opened her eyes and smiled, but speedily relapsed into a lethargic state. She takes small quantities of beef-tea and jelly at frequent intervals. Her deglutition remains unimpaired; she will, however, frequently reject the food from her mouth.

18th.—Remains in a trance-like state. She frequently opens her eyes, and smiles vacantly, but never speaks. The same remedies were continued.

24th.—In consequence of the symptoms remaining unaltered, large doses of musk, and ammoniated tincture of valerian, were ordered, in addition to the turpentine enemata.

26th.—Appeared to gain strength immediately after taking the musk, and smiled when her mother spoke.

30th.—Is evidently worse. The body is greatly emaciated; quite unconscious even when roused; moans frequently, as if in pain, and has a distressing cough; refuses at times to swallow.

Jan. 2d.—Seized at 9 A.M. with a tremor of the whole body, resembling paralysis agitans: the shaking lasted ten minutes.

To take thirty drops of the Sesquichloride of Iron twice daily, and repeat the Turpentine injections.

8th.—To-day the catamenia, of a natural colour, reappeared, and she seems to be stronger. At one period of the day she laughed loudly; she has also taken more nourishment. These circumstances have raised the hopes of all her friends. In consequence of a letter which I received from Dr. Hope, with whom I had recently corresponded respecting the case, we ordered, by his suggestion, that the turpentine injection should be doubled in strength, and that the steel should be continued.

19th.—Since the 8th she has been again declining. The breathing is

hurried, the cough distressing, and the pulse at times scarcely perceptible.

21st.—The point of the nose has become purple from extravasated blood; there is a similar effusion under the cuticle of the nates. Breathing excessively fast; is evidently sinking.

22d.—Died at three P.M.

Throughout the course of this singular case it was considered expedient to have an attendant constantly by the bed-side, to administer nourishment in small quantities; and there can be no doubt but that life was prolonged by so doing. From the commencement of the attack to the day of her death, a period of six weeks, she never spoke; and although she occasionally smiled and opened her eyes, it is probable that she never recovered, even for a moment, complete power of consciousness and volition. It is remarkable, that although the complaint was so clearly a disease of innervation, yet the digestive organs performed their functions tolerably well during the period that the trance-like condition lasted.

In Dr. Elliotson's Practice of Medicine, two cases of a similar kind are mentioned, both of which terminated fatally. These cases will teach the necessity of giving a guarded prognosis in severe forms of hysteria.

Three weeks since, Mr. Paull, of this town, obligingly requested a patient of his to call upon me, who had been affected for five months with a peculiar complaint of the skin. The patient, a girl ætat. 14, was a field labourer, and appeared to be in good health. The catamenia had not yet appeared, and she complained of headache. The eruption was confined to the right fore-arm, the left cheek, and the skin around the orbits. The patches of eruption on the arm had a singularly symmetrical appearance, being composed of concentric rings. The lines forming the different rings were even, rather elevated, of a red colour, slightly fissured, and covered with thin scales. She stated that the patches commenced with a small red spot, which soon became surrounded with a ring; that another quickly ensued, and so on in regular rotation until the patches had acquired their present dimensions. The largest patch is about four inches in diameter, and perfectly circular. She says the eruption has never been

attended with smarting, itching, or discharge. We prescribed a purgative plan of treatment, with calomel ointment; and at the end of a fortnight, when the girl called on me again, the eruption was nearly healed. I was almost sorry to find the remedies had taken effect so speedily, as it was my intention to have procured a drawing of the disease at her next visit, that I might have had the pleasure of sending a sketch of the complaint to the MEDICAL GAZETTE. Neither Mr. Paull nor myself had ever seen or read of a similar disease; we, however, both consider it to be a novel form of psoriasis.

DESCRIPTION OF MR. LUKE'S ELEVATING BEDSTEAD.

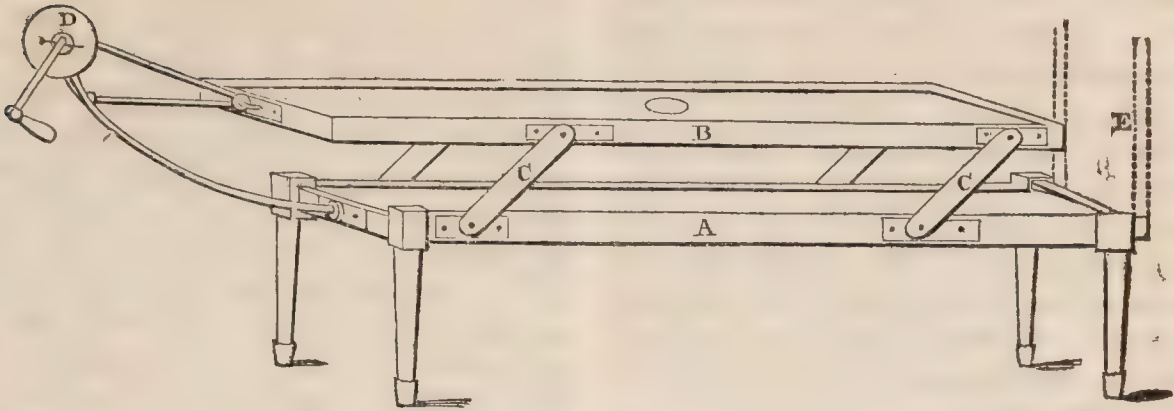
To the Editor of the Medical Gazette.

SIR,

EVERY surgeon in extensive practice has probably at some time or other experienced a wish to secure to his patient a ready means of elevation in bed without disturbance, not only from the convenience which results from such elevation, but also from the more speedy recovery and diminution of danger which is oftentimes thereby promoted. To effect these desirable objects, various contrivances have been suggested; which, however, by reason of the mechanism employed, have been rendered so expensive, that their application to actual practice has not been so frequent as the indications of treatment dictate.

With the view to diminish as far as possible the pecuniary objection, yet still preserving the efficacy of the means, I was induced to have constructed, for the use of the patients of the London Hospital, an "elevating bedstead," in which are combined the advantages of simplicity and cheapness, and which experience has shewn to be both effective and easy in use, and capable of being put into operation by a single nurse of the most ordinary intelligence and strength. I have been told that, as it admits of extensive practical application, it would be useful to make its arrangements known to the public. Hoping that it will so prove, I beg to send to you a description, with a diagram annexed, and to request that

ELEVATING BED.



- A. The bed on which the mattress, &c. lies.
- B. The centre on which the strong sheet is stretched.
- C. The parallel bars.
- D. The windlass, &c.
- E. Supports for curtain rods, if required.

you will kindly afford it a place in the journal over which you preside.

The first part to be noticed is the bedstead properly so called, constructed after the common mode, with the exception that it has not any foot-piece raised upon it. Its dimensions are six feet four inches in length, and two feet nine inches in breadth, and is provided with a sacking stretched upon it in the usual way, for the support of a hair mattress, blankets, &c.

The second part is a frame corresponding to the size of the bedstead. To this frame, around all its sides, is nailed a strong narrow fillet, in which holes, at equal distances apart, are made for the purpose of lacing and stretching a sheet, similarly provided with holes at its edge at similar distances. The sheet itself is also provided with a central oval aperture, for the convenience of the occupant during evacuations.

The frame is connected with the bedstead beneath by four strong flat iron bars. Each bar is fifteen inches in length, and pierced at each end by a hole, through which it is riveted to a plate fastened by four screws at one end of the bar to the side of the bedstead, and at the other to the side of the frame. These bars are arranged two on each side of the bed, one being placed near to its head, the other near to its foot, after the manner of the bars of a parallel ruler, which, in operation, they imitate.

The force which is necessary to raise the frame is applied by means of an iron arm fitted into a square socket at the foot of the bedstead; upon which arm, at its more distant extremity, is

placed a small windlass acting upon a moveable pulley, attached by a hook to a ring on the foot of the frame, in the manner represented in the diagram.

To enable the nurse to perform various offices about the occupant during the elevation, it is necessary that provision should be made for maintaining the elevation. This is easily accomplished by holes drilled through the spindle of the windlass and collar in which the spindle revolves. By means of a pin inserted into either of these holes, the revolution of the windlass is stopped at any desired point, and a maintained elevation is the result.

It is also necessary to guard against the danger of the parallel bars being drawn beyond a line perpendicular to the bedstead during the elevation. A projecting pin should therefore be inserted into the plate above the centre upon which the bar revolves.

When occupied and not elevated, the whole has the appearance of a common bed, to which curtains, if desired, may be attached. The windlass, &c. at this time is removed, and the parallel bars are covered by the bed-clothes. The occupant lies upon the sheet stretched by the frame over the subjacent mattress, &c. When the elevation is desired, the arm of the windlass is placed in its socket, and the hook of the pulley is connected with the ring at the foot of the frame, and the elevation is effected by turning the handle of the windlass, and maintained by the insertion of a pin into the hole of the spindle.

When a change of sheet is necessary, it may be accomplished without

disturbance to the occupant, by placing the clean sheet, during the elevation, upon the mattress, &c. taking care that the central aperture be in the right situation, so as to correspond to the aperture of the soiled sheet when it is allowed to descend upon it. After this descent, the soiled sheet may be detached from the frame, and the clean sheet attached in its place. The former may now be carefully withdrawn from under the occupant with little or no disturbance to him.

A bed chair, if desired, may be easily added to the head of the frame.

I am, sir,

Your obedient servant,

J. LUKE.

39, Broad Street Buildings,
April 28, 1841.

ANALYSES AND NOTICE OF BOOKS.

“L'Auteur se tue à alonger ce que le lecteur se tue à abréger.”—D'ALEMBERT.

The Principles and Practice of Obstetric Medicine and Surgery, in reference to the Process of Parturition. With 100 Illustrations on Steel and Wood. By F. H. RAMSBOTHAM, M.D. &c. &c. London, Churchill, 1841. 8vo. pp. 672.

THE notice which we have already taken of the first part of this work, when it was in the progress of publication, is much too brief to do justice either to the author or his very spirited publisher. It is intended expressly for students and junior practitioners in midwifery. It is, therefore, as it ought to be, elementary, and will not, consequently, admit of an elaborate and extended review. Our chief object now is to state our decided opinion, that this work is by far the best that has appeared in this country, for those who seek practical information upon midwifery, conveyed in a clear and concise style. The value of the work, too, is strongly enhanced by the numerous and beautiful drawings, by Bagg, which are in the first style of excellence. Every point of practical importance is illustrated, that requires the aid of the engraver to fix it upon the mind, and to render it clear to the comprehension of the student. We shall offer a few comments upon some points of prac-

tical importance that have attracted our attention in perusing the work.

Dr. Ramsbotham observes, upon the subject of lingering labour, from ovarian tumors in the pelvis, that “we may establish the principle, that if the swelling possess the least degree of fluctuation evident, a puncture should be made into its substance, by means of a trochar introduced through the vagina or rectum.” Mr. Ingleby* has published several interesting cases which bear upon this point, and which would induce us to puncture an ovarian tumor, which impeded the passage of the child through the pelvis, even if there were *not* “the least degree of fluctuation” to be detected. For, if the head of the fœtus is powerfully pressed upon a tumor that does contain fluid, and which might be evacuated by a moderate-sized puncture, the tumor might still feel very hard and resisting, and offer to the touch no evidence of fluctuation. Mr. Ingleby, with praiseworthy candour, relates cases in which he was thus at first deceived. In one instance the tumor reduced the diameter of the pelvis to one inch and a half. The head of the child was not within reach. The tumor felt hard, like cartilage. It was not softer in one part than another. There was no fluctuation evident in it. It was not harder during the pains of labour, than in their absence. Here the difficulties were great. The tumor could not be pushed above the brim of the pelvis. Embryotomy was impracticable, and the Cæsarean section seemed the only resource. Before, however, this desperate expedient was thought of, Mr. Ingleby, as an experiment, thrust a trochar into the tumor, and drew off seven ounces of clear fluid. In another case he drew off twelve ounces of fluid from a tumor thought to be solid. And he alludes to two other cases where prolapsed ovaria, filled with pus, felt so solid, that puncturing was not thought of, and both cases terminated fatally. In short, we perfectly agree with Burns†, that, “as it is impossible to decide with certainty on the nature or contents of many of these tumors, we ought, in all cases, where we cannot push them up, to try

* Cases in Midwifery (?). We quote this title from memory, but we made a note of the cases referred to when we read them.—*Rev.*

† Midwifery, p. 36, 8th edition.

the effect of puncturing from the vagina with a trochar."

At page 347, Dr. R. refers to Dr. D. Davis's "Osteotomist," an instrument which "is intended to break up the bones of the child's head, particularly at the base of the skull, so as to enable the operator to extract the foetus through a very narrow pelvis, and to prevent, in many instances, the necessity of having recourse to the Cæsarean section." We should wish to know whether this instrument has ever been used in practice; and we would just observe, that it may be very easy to "make large breaches in strong ribs of beef"* by the osteotomist, but neither easy nor safe to employ it on a living woman. Our own experience as to the alleged power of the ergot of rye, in inducing uterine action, would lead us to agree with Mme. La Chapelle, "*que son innocuité est sa plus grande vertu*†." We willingly quote the better, because affirmative, evidence of Dr. Ramsbotham. He states that, in cases in which he wished to induce premature labour, he "made some experiments with the ergot, and that he found expulsive action soon follow its exhibition in all instances, with very few exceptions, when he administered it."

Notwithstanding the complete failure of the ergot in about forty cases in which we have given it, we would still have recourse to it under appropriate indications, from the confidence with which many respectable authorities speak of its power.

Transverse presentations.—We do not perceive that Dr. R. adverts to the occasional possibility, when the hand or shoulder presents, of avoiding the operation of turning, by adopting the practice mentioned by Gooch and others, and which, in three or four instances, we have had recourse to with success. Gooch says‡, "Sometimes, when the hand or shoulder presents, the head rests on the edge of the brim of the pelvis, and if you return the presenting part, the uterus is so stimulated to a vigorous action, by the introduction of the hand, that the head is thrown off the brim of the pelvis, and descends as in a natural presentation. I have succeeded in this way in many cases; and in some, to which I was called for the

express purpose of turning, while apparently only making a common examination, I have returned the presenting part, and then waited for a pain, which has brought down the head." This is a very different mode of practice from the old and now abandoned attempt of grasping the head, after pushing back the shoulder, so as to bring the vertex over the centre of the superior aperture.

Uterine hæmorrhage.—Dr. R., we apprehend, is much more fearful of blood collecting in the gravid uterus, so as to endanger or even destroy life, when its external escape is prevented by plugging the vagina, than the records of obstetrical experience will justify. As far as we know, this apprehension has been more frequently fancied than proved; for cases in point are rare, and even the possibility of such an occurrence has been doubted by many high practical authorities, some of whom are quoted by Dr. R. To the names he mentions we would add, the always valuable evidence of Madame La Chapelle, whose long experience confirmed her both in the safety and efficacy of the plug*. This subject is of importance. Our means of arresting uterine hæmorrhage, in cases where we cannot deliver the patient with safety, are few. Plugging the vagina, we, in common with many of the best practical authorities, regard as one of the most valuable in certain cases, and we may fairly require very good evidence, before we allow our confidence in the practice to be diminished. We are, of course, referring to the use of the plug in the latter months of pregnancy. Its safety, and certainty, in the early months, is not disputed. Dr. R. tells us, in a footnote, at page 504, that "more than one instance has come within his knowledge" of internal hæmorrhage occurring in the gravid uterus. Now granting this fact, and we do so willingly upon the assertion of the author, we would reply, that plugging the vagina is not to be slightly thought of in appropriate cases, because in the course of extensive practice "more than one instance" has proved its occasional failure, any more than we should be justified in abandoning the

* Davis's Obstetric Medicine, vol. ii. p. 1164.

† Accouchemens, t. i. p. 52.

‡ Midwifery, by Skinner, p. 236.

* Practique des Accouchemens, par Dugès, t. ii. 352.

practice of rupturing the membranes, for the purpose of arresting uterine hæmorrhage, because it also is not invariably successful.

We are not at all inclined to deny the truth of Dr. R.'s assertion, that, in cases of placental presentation, "in the great majority of instances, the patient will die if relief be not afforded; and that, under entire placental presentations, delivery should be effected by art as early as is practicable, without incurring the risk of injury." We merely touch upon this subject for the purpose of stating briefly the practice recommended by Wigand*. As far as we know, the opinions of this celebrated writer upon this important practical point have not before been given to English readers. In placental presentations, with slight hæmorrhage, he keeps the patient as quiet as possible both in mind and body. During the intervals of the hæmorrhage he allows her to move about gently, but carefully to avoid fatigue. The bowels are kept open once daily. Excepting occasional aperients, no other internal medicines are given, except in particular cases. Upon the first occurrence of true labour-pains, or rather free hæmorrhage at about the period of labour, he plugs the vagina with soft linen, steeped in thick gruel; the upper part of the plug being moistened with a solution of gum Arabic and powdered resin. He introduces the plug high enough to enter the os uteri, and to lay firmly against the separated edge of the placenta. And, in order that the plug may remain firmly and immovably in this position, he further fills the vagina, in every direction, as completely as the patient can bear without pain, with linen or pieces of sponge. He then lays a thick dry towel externally and firmly upon the os externum. He takes care, at the same time, to remove any morbid condition of the uterus that may happen to exist, "as rheumatism, spasm, or inflammation." The disposition to bear down, which will sooner or later arise from the artificial distension of the vagina, must be supported as long as possible by the patient. If she succeeds in conquering this disposition for about half an hour, the vagina becomes accustomed to the irritation, and remains quiet. If, in a few hours,

the pains becoming more rapid and stronger, the bearing down sensation is accompanied by a burning feel in the vagina, the original plug is replaced by a smaller one. Before this smaller plug is applied, an examination is made per vaginam, and the following circumstances are especially attended to. Whether the os uteri is much expanded, and whether the placenta has already begun to separate from one or the other side. Whether the head, or the feet, or what part of the child presents. Whether the bladder of membranes is distended. And whether it can be ascertained that both the presenting part of the child, as well as the separated edge of the placenta, are equally and simultaneously pressed down during the pain. If any other part than the head, feet, or breech, presents, Wigand turns as soon as he can; but if the presentation is natural, the pains powerful and frequent, the passage of the child quick, and if the patient's pulse is full and slow, he leaves the delivery entirely to nature. In the course of some time unusually quick and powerful pains come on, and expel the child "alive and merry" (*lebendigen und ganz munteren kinde*), together with the tampon. The placenta is expelled spontaneously; and soon after the child, when this practice is adopted. Wigand assures us that for many years he has carried this practice into effect in cases of placenta prævia, and that he has not lost a single woman or child; and that the recovery of the patient has always been favourable. He admits that there are cases in which this plan would not be applicable. For example, in women with such irritable vaginæ that the plug could not be borne; or if the hæmorrhage had greatly exhausted the patient before the practitioner was consulted.

Dr. Ramsbotham comments very properly upon the hazardous remarks of some writers, whose statements may lead young practitioners to the belief that, in cases of retained placenta, from morbid adhesion to the uterus, it not unfrequently happens that it is inevitably necessary to leave a portion of the placenta in the uterus. "It would be my wish, on the contrary, to inculcate the idea that the *whole* can very generally, and ought, even at the expense of some trouble, to be removed; and that we should never feel satisfied

* Geburt des Menschen, p. 132. Berlin, 1839.

that we have done our duty, unless we have used our best endeavours to effect its entire abstraction." Our "best endeavours" must of course be compatible with the safety of the patient, and how far we may endeavour to remove the whole of a morbidly adherent placenta must be left to the judgment of the practitioner in each individual case. "If," says Burns*, "the adhesion of part of the placenta be very intimate, we must not, in order to destroy it, scrape and irritate the surface of the uterus; but we ought, rather, to remove all that does not adhere intimately, leaving the rest to be separated by nature." In several instances that have occurred to us we have been compelled to leave a large portion of the placenta in the uterus, as a lesser evil than that of using great force to separate its close adhesions. We perfectly agree with Dr. R. that such an alternative is a fearful one; but we are sometimes compelled to choose it; and in the cases we refer to we have done so with safety, for the retained portions have been expelled without severe hæmorrhage or serious symptoms afterwards.

Inversion of the uterus.—Dr. R. states that unless the uterus be replaced "within a short time when the accident took place, he should presume either that the uterus could not be reduced at all, or that the hæmorrhage must be excessive."

We would just observe that spontaneous reduction has been known to take place when the inversion has been of long-standing†. Gardien‡, again, an excellent practical authority, says, "Les auteurs n'ont tant insisté sur la nécessité de réduire sur-le-champ la matrice, que parcequ'ils étaient dans l'opinion que si on laissait échapper ce moment, on rencontrerait par la suite plus de difficulté, qu'elle deviendrait même impossible. L'expérience apprend, au contraire, que la réduction est quelquefois plus facile au bout de plusieurs jours qu'après quelques heures de retard seulement." We have recorded a case of Dr. Belcombe's§, in which the inverted uterus was replaced

twelve weeks after its displacement; and in twenty days the patient was well enough to travel. The hæmorrhage had been severe during the whole time the uterus was inverted. Dr. R. would prefer returning the inverted uterus and placenta together, and afterwards removing the placenta from the uterus; while others recommend that the placenta should be separated from its attachment before any attempt is made to replace the uterus. Barlow's observations upon this subject are worth noticing. He says*, "The mode of replacing the inversion, as usually recommended by authors when the placenta is adherent to the inverted uterus, is to return both organs at the same time, without first separating the one from the other. This counsel, though resting on high authority, should be received and acted upon with great caution, as the bulk of the two protruded organs, when surcharged with blood, will present such resistance to the exertions of the accoucheur in attempting reduction under these circumstances, that it cannot be effected without using great violence, and exposing the woman to imminent and unnecessary danger: and supposing it to be accomplished in this way, the placenta will have to be disparted in the uterus, and afterwards extracted, and great risk incurred of a second inversion, which presents a material objection to this mode of proceeding. The more rational and less injurious method appears to be in all cases, whether the placenta be attached to the uterus, or otherwise, to compress the whole substance between the hands like a sponge, by which the blood will escape, and the uterus be diminished in size: and the placenta, if adherent at the same time, will be separated, so that its reduction may be safely and easily accomplished without further interference."

Hæmorrhage after the removal of the placenta.—Dr. Ramsbotham very wisely deprecates the advice that is given by many authorities, of *immediately* introducing the hand into the uterus, in cases of severe hæmorrhage after labour. He would first adopt the less harsh and safer means of external pressure, and the application of cold to

* Midwifery, p. 372. 8th edit.

† Osiander. Die Ursachen und Hülfsanzeigen der unregelmässigen und schweren Geburten. Zweite Auflage. 145.

‡ Accouchemens, t. 3, 313.

§ MED. GAZ. vol. vii. 783.

* Essays on Surgery and Midwifery, 209. Foot-note.

the uterus. If these means fail, which we know they will not in the majority of cases, the hand must then be introduced into the uterus to remove any coagula it may contain, and to ensure efficient contraction. We very cordially agree too with Dr. R. upon the much disputed point of the use of opium in severe cases of uterine hæmorrhage, although his opinions are opposed by the high names of Burns, Stewart, &c. "I have stated that opium, in large doses, is very much extolled in cases of flooding, especially by Professor Burns and Dr. Stewart, but that I consider it a medicine inadmissible, unless the uterus have entirely contracted, and when the danger of fresh bleeding has gone by. Opium certainly acts as a cordial, lulling the irritability of the patient, and producing sleep, or, at any rate, a composing stupor; but it also takes off both muscular and uterine action: it disables the uterus, therefore, from contracting, even when it is so disposed: and if the proposition be true, that on the contraction of the uterus alone we are to rely for the patient's ultimate safety, it cannot but appear contradictory to resort to a medicine whose very action tends to prevent the effect desired. It has been objected, that although opium, in small quantities, takes away uterine action, yet, in large doses, it produces the very opposite result, and excites contraction. This proposition is at variance with common sense, with all analogy of the actions of other drugs, and at least with my experience. I have often seen, at the commencement of labour, uterine action suspended by what would be considered a large dose of opium: and if the same quantity will take away action at the beginning of the process, is it reasonable to suppose it will excite it at the termination? But it may be said the experience of practitioners of eminence proves the value of opium in the case under consideration, and that there is no reasoning against experience. I by no means deny that many patients have done well after the administration of large doses of opium; but that circumstance does not prove that the drug was the agent of their preservation. I should be inclined to attribute the recovery to other causes, independently of the exhibition of the medicine. I strongly recommend opium in large doses, and the solid form in those

cases of irritability produced by a loss of blood which had previously taken place; but that only when the uterus is contracted, and the danger from flooding is past," p. 551.

Once more we strongly recommend this work to the student who wishes to become acquainted with all the leading doctrines and facts of obstetrical science, as a sure and safe introduction to the perusal of more elaborate works.

Researches on Operative Midwifery, &c. With Plates. By FLEETWOOD CHURCHILL, M.D. &c. &c. Dublin and London, 1841. 8vo. pp. 360.

THE greater part of this work has already appeared in different periodicals in the form of detached papers; and, with every disposition to applaud the industry of the author, we cannot refrain from saying, that no additions are made to the volume before us of sufficient importance to justify their republication. The "table of contents" comprises accounts of the artificial induction of premature labour; of version, or turning; of the vectis, or lever; of the forceps; of the perforator and crotchet; of the Cæsarean section; of symphyseotomy; of the umbilical cord; a report of the Western Lying-in Hospital; and lastly, a chronological catalogue of midwifery authors. It has always been our opinion, that the merit of a good practical compilation, and the requisite ability on the part of the compiler, are generally very much underrated; and we are not at all inclined to doubt, that Dr. Churchill has enough of literary knowledge, and quite sufficient practical judgment and experience, to enable him to give a good and very acceptable abstract of the opinions of foreign and English writers, as well as of the standard practice of the day, on obstetrical subjects. But neither of these objects appear to us to be satisfactorily effected by these "Researches." They consist chiefly of a chronological history of obstetrical operations, to which follows, for the most part, a very meagre account of the practice now generally adopted. Some of the statistical records and tables are valuable. The essay on the funis umbilicalis, and the report of the Western Lying-in Hospital of Dublin, give the volume a respectable appearance as to its bulk,

but any great merit these additions contain has escaped us; although, in justice both to our author and our readers, we perused them with becoming attention.

MEDICAL GAZETTE.

Friday, May 7, 1841.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

'PRACTICAL REMARKS' ON REFORM.

AFTER many nibbles, you may expect a bite, say fishermen; and after the several crude schemes, and bills not to be passed, which medical reform has inflicted on us, it is probable that something efficient will really be done, although we doubt whether it will now be accomplished during the present session. If we were to hazard a conjecture concerning the nature of the alterations likely to take place, we should predict that they would be of two kinds. In the first place, several medical corporations will be compelled (for better or for worse) to throw down the wall which separates their esoteric from their exoteric members, and thus admit all alike into the interior of the temple. Secondly, some changes will be made in the system of medical education. On both these subjects the greatest difference of opinion naturally prevails; and it is scarcely possible that the details of any new regulations should be generally acceptable, even in theory; far less in their practical working. Meantime, the continual discussions of delegates, the declarations of councils, and the ceaseless torrent of articles and pamphlets, show that the profession expects its deep-seated evils to be speedily taken into consideration by Parliament. Men of name, and men of no name, have alike

spent their money in paper and print, and, disdaining to give a silent vote, have set forth at length the reasons which induce them either to acquiesce in the present system, or else to hope for better things from a regenerated profession. Among the throng comes a general practitioner of no very decided opinions*, but who probably represents the doubts and hesitations of a considerable class. He desires, he says, "to tread in the straightforward path of truth and honesty, and to rank with those who form an exception to the general rule; but who, fearlessly as they give their opinions, forget not that low and vulgar satire is not reason, and that falsehood and abuse are not substantial and conclusive arguments."

Quid dignum tanto feret hic promissor hiatu?

On the first of the two kinds of reform which we alluded to above, the General Practitioner says but little. He merely suggests that some of the licentiates of the Apothecaries' Company might be elected into the Court of Examiners—a distinction now confined to Members. On the other point, or medical education, he says more, but nothing which is very striking. In examinations he would "cautiously steer between the two extremes, believing the middle path to be the safest." No doubt; so would we all. But the questions remain, where are the two extremes, and which is the middle path? For instance, to take a topic discussed by the author—should a competent knowledge of the classics be required from medical students, or not? At present, the quantum demanded at Apothecaries' Hall is too trifling to deserve mention; and though the number of candidates plucked in this sub-

* Practical Remarks on the measures proposed for reform in the medical profession. By a General Practitioner, &c. London, 1841.

ject is considerable, this is a proof rather of the inefficiency of our common schools, than of the severity of the examination.

The Quarterly Review, on whose opinions we commented some weeks ago, observed that the preliminary education of students was quite overlooked both by the College of Surgeons and the Society of Apothecaries, except that the latter required the candidates to construe some scraps of Latin. The General Practitioner cannot understand what is meant by "scraps of Latin," and seems somewhat scandalized at the phrase. If he lives in some *pays Latin*, where every one is up to the mark, has a scholarlike acquaintance with the language, and repudiates fragmentary knowledge, we would recommend him to visit the establishment of a London grinder, or examine a few pupils going up to Blackfriars; and if that does not teach him what "scraps of Latin" means, his case is hopeless. He fears something, moreover, which is not likely to be proposed by any one. He dreads some romantic attempt to extend the usefulness of practitioners by "high classical requisitions." But let him dismiss his fears: no one proposes that the mass should do more than come up to a low standard in classics; a high one is quite out of the question. The "General Practitioner" makes a curious objection to the statement of the Quarterly Reviewer, when he thinks that the latter must have overlooked the recommendations of the Court of Examiners; for they assert that a familiar acquaintance with the Latin language is indispensable, and a knowledge of Greek scarcely less so. The difference between preaching and practice was never more clearly exemplified: the pamphleteer quotes the former, and the reviewer the latter. If all these recommendations of the Worshipful Society were

followed, the young practitioner, with his store of Greek and Latin, French, German, and mathematics, together with medicine and its ancillary sciences, would be a very remarkable person; one whose attainments must be the subject of universal envy, and whose education the most zealous reformer would scarcely venture to meddle with.

But since the facts are otherwise, let us consider why some candid persons wish for a reform in this particular, and desire the practice to be brought up to the preaching. The "General Practitioner" observes that members of our profession may be extensively useful with a comparatively scanty measure of classical knowledge. This is true; they may be useful, not only with a comparatively scanty measure, but with no measure at all: a practitioner may treat fever very sensibly, though he could not read what Celsus says on the subject, were it to save his own life, instead of the patient's. Nay, we may go much farther; a man may obtain a large practice, though ignorant of the elements of literature; and many a surgeon may cure a broken head with great skill, though in the constant habit of breaking Priscian's. Ancient and modern languages, the exact sciences, chemistry and botany, nay anatomy itself, have sometimes been thrown overboard, and the vessel has seemed to sail the better for losing its cargo. But this spoliation of the stores of the mind is a dangerous experiment. Martinus Scriblerus endeavoured to give his son an abstract idea of a Lord Mayor, without a great horse, without a gold chain, without custard; and thus some theorists, with the hope of retaining the essential, and the essential alone, would reject the greater part of what have always been considered the ornamental necessities of a professional man.

But while the partisans of a minimum of education may quote some strong examples in favour of their arguments, those who recommend a larger acquisition of knowledge do not lack topics to urge on their side of the question. They allege that although instances are not wanting of practitioners who have attained great success, with an imperfect education, and uncultivated mind, yet that such examples are rarer than is sometimes asserted; that, on the whole, he who has most tickets has the best chance in the lottery of life; and that the social superiority of a professional man to the most ingenious artisan, depends, in a great measure, on his general information. They add, that the position of a whole profession in society is to be estimated not by the reception given to a few of its leaders, but by the place vouchsafed to a mere average member; and that if we adopt this reasonable criterion, much remains to be done to elevate the practitioner of physic. In conclusion they observe that we may apply to the practitioner of physic what was said of lawyers by a great authority, that every kind of knowledge comes into play in the practice of his profession. Hence we may learn, in answer to the General Practitioner, why so sound a critic as the Quarterly Reviewer desired the standard of classical knowledge to be raised among students; not merely that they might know the names of the tools they use, and read some great writers in the original languages; but that they might enlarge their minds by the conquest of difficulties, and contend on equal terms with the other learned professions.

The same considerations, *mutatis mutandis*, may be applied to the other branches of study recommended by the Worshipful Society.

Still the education of a numerous body cannot be radically amended in a moment. In order that these preliminary studies may be pursued with advantage, our common schools must be improved; or parents must be warned against those ill-managed academies, where nothing is tolerable but the prospectus. However, just as we urged in our late observations on Dr. Peacock's proposed reforms, that the improvements made at Cambridge will necessarily benefit the schools which prepare students for the University; so it is in the power of the Society of Apothecaries to effect improvements in many schools by demanding something in the shape of a preliminary education.

Such an examination would, of course, be sufficiently easy to allow students, even from our common schools, to pass without much difficulty; for any barrier suddenly checking admission into our ranks, and excluding a host of applicants, would be considered, and with justice, a practical grievance.

The same may be said of examinations undergone by the pupil after the completion of his professional studies, but with an additional circumstance. If any thing is added to the length or severity of the present studies, it should be in purely practical matters; and it might be well to make room for the essential, either by dispensing entirely with some of the minor subjects, or by confining them within narrow limits. The medical colleges, which we suggested in our article of April 2d, would be admirable adjuncts to any improvements in professional education, giving life and existence to regulations which, for want of them, are too often but a dead letter. Mr. Green, in his late pamphlet, "The Touchstone of Medical

Reform," suggests that candidates for general practice should be examined by the Colleges of Physicians and Surgeons; as an examining Board, consisting of general practitioners, would be likely to alienate them from those bodies, the character of which tends to give them rank and estimation. The author of the pamphlet, on the other hand, thinks that the rise of general practitioners in the profession is chiefly attributable to the regulations of the Society of Apothecaries; and he consequently wishes still to leave in their hands the office which they have discharged so well. The objection founded on the connection of general practitioners with trade, he thinks the offspring of mere ignorance.

"Very many of the general practitioners of London confine their demands upon their patients to medical or surgical attendance; and where this is not strictly the case, in the majority of instances it is owing to the public mind not being alive to the importance of such a procedure, and not a voluntary act on the part of the practitioner himself."

This statement is obviously an exaggeration; for as the middle classes insist on having physic as well as advice from their medical attendants, the man who endeavours to obtain practice among them must per force comply with their wishes.

What was the origin of the Apothecaries' Act in 1815? The General Practitioner says it was the result of an inquiry into the state of four districts in the North of England, where the regular doctors were far outnumbered by the unlicensed ones. According to Gray*, the immediate origin of the Act was a meeting convened in July 1812, to consider the high price of glass.

* Supplement to the Pharmacop. pref. p. xix.

LONDON UNIVERSITY EXAMINERS.

WE are quite at a loss to understand the principle on which the office-bearers of the London University are elected. Last year Dr. Tweedie was set aside, without any one knowing why, and Dr. Watson elected in his place. This year the former is replaced again, and the latter discharged, quite as unexpectedly as in the former case! The impression naturally produced under ordinary circumstances would be, that the party set aside had, in some way or other, been found unfit: but the chopping and changing which this season has produced, shews that some other and totally different considerations have been at work.

Among those elected examiners on the 8th of last month was Dr. Ferguson, of King's College; but he, staggered by the omission of Dr. Watson's name from the list, has tendered his resignation, as appears by the following letter.

"9, Queen Street, May Fair, 16th April, 1841.

MY LORD, — As President of the Senate of the University of London, permit me to tender to your Lordship my resignation of the office of Examiner in Midwifery, to which I had the honour of being appointed on the 8th instant.

The fact made apparent at the late election, that a faithful and efficient discharge of the duties of an office may nevertheless be followed by dismissal, has compelled me to withdraw myself from a position which, though honourable in itself, may entail on me also the implied censure of a rejection.

I need not assure your Lordship, that could I have foreseen the results to which I have alluded, I should have at once declined the offer of those friends who did me the honour of requesting me to serve as an Examiner. And I have only now delayed tendering my resignation a few days that I might avoid the imputation of a hasty decision in so important a matter, or the slightest appearance of disrespect towards that body of gentlemen over whom your Lordship presides.—I am, my Lord,

Your Lordship's obedient servant,
ROBERT FERGUSON, M.D."

These occurrences will render most persons—at least those who are fit for the appointments in question—rather chary about accepting them.

ROYAL MEDICAL & CHIRURGICAL
SOCIETY.

April 23, 1841.

DR. WILLIAMS in the Chair.

*Observations on the Anatomy of the Lungs.*By THOMAS ADDISON, M.D., Physician
to Guy's Hospital.

THE author begins by expressing an opinion, that notwithstanding the attention which has been devoted of late to morbid anatomy, it is by no means apparent that those researches have been made sufficiently available to explain the function and structure of obscure, intricate, and complicated parts of the body in a state of health. Proceeding on this principle of rendering healthy and morbid anatomy mutually subservient to the elucidation of each other, the author hopes that he has succeeded in illustrating, if not in demonstrating, certain points of great interest with reference to the lungs, some of which have been already pretty generally admitted, whilst others are perhaps altogether novel. The results of his investigations on this subject seem to him to prove almost beyond dispute—That the aerial cellular tissue of the lungs is made up of well-defined, rounded, or oval lobules, united to each other by interlobular cellular membranes, each lobule constituting a sort of distinct lung in miniature, having its own separate artery and vein; that those lobules do not communicate directly with each other; that they do not, as Reissessen has supposed, consist of the globular extremities of as many bronchial tubes, but, on the contrary, as Dr. Hodgkin has suggested, are made up of a collection of cells, in which, by a common opening, a minute filiform bronchial tube abruptly terminates; that the pulmonary artery accompanies the bronchi, branch for branch, to the minutest divisions of the latter; that pneumonia consists essentially of inflammation of the aerial cells; that pneumonia and inflammatory tubercle are identical; that acute pneumonia in healthy constitutions scarcely ever leads to the formation of an abscess, unless deposit previously existed, but that when it occurs in cachectic or broken-down constitutions, or supervenes in the progress of organic diseases, it causes one or more distinct lobules to soften down into an ill-conditioned abscess; that ordinary tubercles present the same varieties in the lungs that they do in serous membranes; that emphysema of the lungs consists chiefly of mere dilatation of the cells, but in parts also of more or less extensive laceration of them; and, lastly, that the circumscribed gangrene of Laennec is commonly, if not uniformly,

a mere effect or advanced stage of pulmonary apoplexy.

The author's object, however, in the present communication, is not to solicit attention to any of these matters, but merely to point out a mode of distribution of the pulmonary veins which has not, as far as he knows, been noticed by any preceding anatomist. After having cited passages from the works of Cloquet, Meckel, Adelon, Ollivier, and Bichat, touching the situation and course of the minute pulmonary veins, the author proceeds to shew the mode of their distribution which his own dissections appear to demonstrate. It would exceed the limits of an abstract to give the author's description at length; suffice it to say, that the lung is made up essentially of a vast expanse of membrane, the interior of which is unceasingly exposed to the influence of atmospheric air, and upon the surface, or in the substance, of which are spread out the capillary ramifications of the pulmonary artery; these arterial capillaries passing from thence to the exterior of the membrane to form the pulmonary vein, which, throughout its whole course, is found to be situated on the exterior of the aerial cellular structure of the organs. The author indulges a hope that, with a knowledge of this striking distribution of the pulmonary vein, we shall, in future, be more successful in our investigations into the pathology of phthisis; and especially that it will set at rest the long-agitated questions respecting the origin and seat of pulmonary apoplexy.

The president remarked that the paper just read appeared to him to possess peculiar interest, not merely in regard to anatomy and physiology, but still more in its relation to pathology. If he had understood its import aright, it implied that henceforward many of the diseases of the lung would be much more under control, and even that we might see no more of phthisis. He should be very glad to hear from the author somewhat more of the application of his discovery to pathology.

Mr. Bransby Cooper said he feared that the president had, like himself, been somewhat disturbed in his attention to the paper by the interruption which had accidentally arisen, and by the difficulty which the secretary had experienced in deciphering Dr. Addison's hand-writing. Mr. Cooper, then detailing what he thought he had understood to be the objects of the paper, said that he believed it would be found to have peculiar interest, especially by the relation which the lungs were now found to hold with certain other organs of the body, in regard to the distribution of their vessels, and the separated course of their arteries and veins. In the thymus and thyroid glands, for example,

and in the capsulæ renales, there was a similar separation of the arteries and veins, only in these the arteries were on the exterior, the veins in the interior of the organs; while in the lungs Dr. Addison had shewn the arteries in the interior, the veins in the exterior of the organ; for of course the exterior of the lobules on which these veins were traced might be spoken of as the exterior of the organ. He thought it likely that this difference of relative position might have something to do with the very different offices of the veins in the two cases; which were, in the one, to carry purified blood from the capillaries to the left side of the heart; in the other, to carry deteriorated blood into the general venous system.

Dr. Addison said he did not think there was any expression in his paper that could justly lead to the idea that he contemplated, as a result of his discovery of the course of the pulmonary veins, the extinction of phthisis. At the same time he believed that it might prove of some importance in the determination of the true seat and pathology of certain pulmonary diseases. For example, it is still a question whether, in what was called œdema pulmonum, the serous fluid with which the lungs were found loaded was situated in the air-cells of the lungs, or in the cellular tissue investing them and the lobules; and so also in pulmonary apoplexy, it was still disputed what was the seat of the effusion of blood. Now these and many similar questions he thought might be explained by following out the mode of investigation he had adopted. If, for example, on separating the lung into its parts, lobule by lobule, as he had done, it was found that the fluid effused was not removed or diminished in quantity, then there could be little doubt that the interior and not the exterior of these parts of the organ were its true seat. And so on for several other questions; as, for example, the seat of tubercle, of inflammatory tubercle, of pneumonia, &c.

Mr. James Blake said, that he could not understand from Dr. Addison's description what the means of communication between the ultimate branches of the pulmonary arteries and veins were. He had spoken of the arteries as passing to the interior of the lobules, and of the veins as arising from their exterior; but had not mentioned the means by which they were connected. It was surely not probable that they formed an exception to the general law now universally received, of arteries and veins being connected by an intermediate system of capillaries arranged in a network.

Dr. Addison said, that he imagined the pulmonary vessels did also so communicate, but still the preparations showed that the arteries passed to the interior, and the veins

came from the exterior of the lobules. In answer to a question by Mr. B. Cooper, Dr. Addison said that he supposed it was not possible to tell whether minute vessels of the lung ran on the surface or in the substance of the membrane of the aerial cells. He also detailed some observations which he had made on the distribution of the vessels in the lung of the frog, in which there was a similar separation of the main arteries and veins, only that here the pulmonary vein ran in the interior of the saccular lung, as if to guard it from pressure during any great exertion of the animal, by placing it in a medium of elastic air.

Mr. Bowman said that the microscopic examination which he had made of the structure of the lungs had shewn him that the air-cells were composed of a perfectly homogeneous tissue, and that they were all lined by a very fine epithelium, thickly set with ciliæ, which were during life continually vibrating. This ciliary epithelium was the continuation of that which lined the whole of the bronchial tubes. He had no doubt that the capillaries of the lungs might be said to be arranged in the substance of the membrane of the air-cells; that is, like the capillary vessels of all similar membranes, they are placed beneath the epithelium. In a lung he had recently injected he had observed the same isolation of the pulmonary veins from the arteries and bronchial tubes as Dr. Addison had described, but at the time had thought it of no practical importance.

Mr. Paget said that he thought the misunderstanding respecting the arrangement of the arteries on the interior, and the veins on the exterior, of the structures, had arisen from its being supposed that Dr. Addison had described the vessels, not of the pulmonary lobules, but of the air-cells. In the air-cells it was quite certain there were nothing but capillaries, nor could it be possibly determined whether on them the arteries or veins were separated; for the vessels there were in fact not distinguishable as arteries and veins, but were merely an intermedium between the two. What Dr. A. had described were the coarser branches of the arteries and veins, of which it appeared that the former ran into the interior of the lobules or collections of air-cells, while the latter were arranged on their exterior. He would take the opportunity of mentioning, what might have some interest in its relation to Dr. Addison's discovery, that in the last three cases of pulmonary apoplexy that he had examined, he had found the pulmonary veins coming from the seat of the effusion completely blocked up by very firm fibrinous coagula, similar to those formed in phlebitis, and evidently formed long before death.

Dr. Addison said that his preparations

would show that he had injected not merely the larger, but the smallest branches of the pulmonary vessels; and the veins might be seen in them ramifying like the diverging fingers of the hands or claws on the exterior of the lobules. He then entered, with some detail, into the pathological deductions which he believed might be made from the fact which he had discovered; and after some observations from Dr. Mayo the Society adjourned.

THE MEDICAL SCHOOLS.

To the Editor of the Medical Gazette.

SIR,

As there are now in this metropolis so many *soi-disant* schools of medicine, and so various in the means of instruction they afford to their pupils, it is much to be lamented that some strict *surveillance* is not by law established over them. Young men raw from the country are led to enter certain schools by various motives; and as all are recognized, they feel sure they cannot err in their choice. Too late do some find they have taken a step which they will silently lament all their days. Silently, I say, because, were they to complain, such complaint would involve their own reputation. They may have *passed*, it is true, thanks to the grinders, but their conscience is not satisfied. Their time is irrevocably lost—their money is gone—they are not half educated. It would be generous in the old respectable schools to open their doors to the duped, and to allow them to finish their education at a less charge than what is demanded from the fresh pupils, which they, in many cases, could afford; but to pay all over again is more than many in our poor profession can find the means to do. Surely the College of Surgeons, who have the surgical, and of course the anatomical departments of medical instruction under their charge, should deem it a moral duty to inspect the schools, and see if the facilities for acquiring a proper knowledge of these subjects be afforded to the pupils. In many cases of rejection the censure should fall on the teacher, and not on the candidate for the diploma. It is to be feared some schools are not established for teaching, but for the purpose of getting the fees.

It may be said, these things will find their own level: no such thing. If the pupils were to pay for each course, they would find their own level; but, in such establishments, he who takes his tickets for all the prescribed courses at once, gets them at a reduced price. Once caught in the trap there is no retreat, and silence is discretion.—I am sir,

Your obedient servant,

AN OBSERVER.

London, May 1, 1841.

GLASGOW COLLEGE.

10th March, 1841.

MEDICAL REFORM.

A MEETING of senate being duly summoned and convened, the committee appointed by the senate to consider the Bill for the better Government of the Medical Profession, which has been introduced into the House of Commons by Mr. Warburton and Mr. Hawes, beg leave to report—

1. That, in the opinion of the committee, evils have arisen from the possession by particular corporations of local privileges, which render their licentiates alone legally qualified to act as medical practitioners in particular districts of the country, to the exclusion of all other persons, however highly qualified.

2. That it is desirable that those local privileges should be abolished, and a uniform system adopted, whereby the graduates of all universities, and the licentiates of all legally constituted medical corporations, shall be placed on an equal footing, in respect to the right of practising; so that those who have received certificates of their fitness to exercise the medical profession from any one of the established corporations, shall be entitled to practise in any part of the United Kingdom, without requiring to submit to a second examination, or pay for a second license.

3. That the most essential part of such uniform system must consist in the general adoption of a certain course of education, as the *minimum*, entitling any one to become a candidate for a medical license.

4. That the measures proposed in the bill recently introduced into parliament are inadequate to the accomplishment of the objects in view, and could not be carried into effect, without the most serious inconveniences.

5. That neither the proposed compulsory registration of licensed and qualified medical practitioners, nor any other measure, can be effectual in preventing unlicensed and unqualified persons from practising medicine, so long as there exists a demand for the services of the latter on the part of the public.

6. That the establishment of a new and permanent board, having power to control the universities and medical corporations, is highly objectionable and uncalled for; all the real benefits contemplated by its establishment being attainable by merely enforcing uniformity as to a minimum course of study, to be determined on, after due communication with all the established licensing corporations.

7. That the proposed election of a representative body by the entire medical profes-

sion in each of the three kingdoms, could not fail to produce a regular recurrence of dissension and agitation among its members, which would interfere materially with the ordinary avocations; while those best qualified to perform the duties of a representative would be least likely to use the means necessary to secure their election, and if elected would be least able to repair, from time to time, to a distance from their homes, for the execution of those duties; whence the appointments would inevitably fall into the hands of persons in whom neither the public nor the profession would have confidence.

8. That the provisions of the bill now under consideration would, if carried into effect, gradually undermine and ultimately supersede all the existing British Medical Schools and licensing bodies, with the exception of those seated in the three capitals of the United Kingdom, by confining the power of granting licenses to practise the medical profession to those three cities exclusively, or perhaps to London alone.

9. That considering the large amount of services gratuitously rendered to the public by the medical profession, the inadequate compensation received by many of its members for their long and expensive education, and for their laborious professional exertions; considering also that each degree of Doctor in Medicine is already taxed to the extent of ten pounds; and considering finally that it is the public, much more than the medical profession, which is interested in being able to distinguish easily betwixt qualified and unqualified practitioners; the Committee is very decidedly of opinion that the expenses incurred by an improved system of medical legislation should be entirely defrayed out of the public purse, and not by tax imposed in any shape upon the profession.

The senate unanimously approved of this report, and directed that the same should be printed and circulated as extensively as possible.

Glasgow College, March 10th, 1841.

ROYAL COLLEGE OF SURGEONS IN LONDON.

THE council of the College, desirous of furnishing to the public a correct list of their members, request that each member will be pleased to transmit to the secretary, between the 1st of June and 1st of July in every year by letter, a statement containing his name at full length, address, and date of diploma, in his own handwriting, in order that it may be compared with the chronological list.

The council will be further obliged by the member stating it in a similar manner

when he has a degree in medicine, or the license of the Society of Apothecaries.

The council will be glad to receive corresponding statements from the members of the Edinburgh or Dublin College of Surgeons, practising in England or Wales.

EDMUND BELFOUR,
Secretary.

April 8, 1841.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Monday, April 26, 1841.

Lewis Rudge.—Joseph Savory Tylor.—Egerton James Pratt.—Thomas Spencer Wells.—William Pritchit Hodgson.—Robert Lawle Downall.—Samuel Parker.—Richard Prior.—Isaac John Gillam.—James Lewis Winchester.—Thomas Edward Vernon.—John Lodge.

Friday, April 30, 1841.

W. Copeland.—G. Guillemard.—J. Morgan.—F. J. Corbould.—T. Tardrew.—A. J. Burmester.—G. J. Hilbers.—W. Weld.—R. G. Coombe.

Monday, May 3, 1841.

T. Baynton.—E. Berney.—J. Young.—R. Gilling.—J. P. Harris.—J. Whaley.—J. Duncan.—R. B. Sullock.—C. P. Bates.—T. J. Tuffnell.—J. Clayton.

APOTHECARIES HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, April 29, 1841.

C. H. Buncombe.—F. S. Cornish, Kingsbridge, Devon.—E. Colchester, Ipswich.—J. Cresswell, Loose, near Maidstone.—R. T. Frere, Diss, Norfolk.—J. V. Norman, Videliscombe, Somerset.—J. Rogers, Watford, Herts.—H. C. Goodlake, Cheltenham.—J. H. Partridge, Colchester, Essex.—R. L. Baker.—E. L. I. Gaine.—C. P. Daniell, Norwich, Norfolk.—J. Fidler, Dunmow, Essex.—S. Hare.—G. T. Smeatham, Great Missenden, Bucks.—F. J. F. Payne, Weymouth.—J. Lithgow, Weymouth.—H. Lambert, Drews Leighton, Devon.—J. Jackson, London.

METEOROLOGICAL JOURNAL.

*Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.*

	April.	THERMOMETER.		BAROMETER.	
Wednesday	28	from	48 to 73	30·08 to	30·06
Thursday	29		42 69	30·06	30·09
Friday	30		40 68	30·11	30·07
	May.				
Saturday	1		38 69	29·97	29·84
Sunday	2		40 69	29·66	29·63
Monday	3		44 52	29·68	29·76
Tuesday	4		41 64	29·61	29·53

Winds, S.W. and N.E.

On 28th ult. and two following days, generally clear; rain fell on the 28th and 29th. The 1st inst. clear. The 2nd, morning clear, otherwise cloudy, raining very heavily during the afternoon, accompanied with distant thunder in the north. The 3d, overcast, raining frequently during the day. The 4th, morning overcast, otherwise clear.

Rain fallen, ·425 of an inch.

CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, MAY 14, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXXIV.

Hydrophobia, concluded. Various questions considered respecting the disease as it appears in the Human Subject, and respecting Rabies in the Dog. Pathology of the disorder. Treatment. Preventive measures.

AFTER giving you some account of the phenomena of *hydrophobia*, or *rabies canina*, I began to notice, in the last lecture, the chief of the interesting questions which naturally present themselves to the minds of most men, and especially of medical men, in respect to that shocking disorder.

In the first place, there is such a disorder. It appears, too, secondly, from statements made upon credible authority, that the same group and succession of symptoms as characterize the disease, when it is produced by the bite of a rabid animal, have been observed to occur in persons who were never known to have been bitten. My own opinion is, that it is more probable that these persons had been exposed to the virus without being aware of it, than that the disease was spontaneously engendered in their bodies. I would make the same remark in regard to an instance which is said to have happened of hydrophobia in a lad who had been bitten five weeks before by a *healthy* dog: the dog remaining well at the time of his seizure and death. Mr. Youatt holds, indeed, that however the disease originated, it never occurs now, not even in the dog, except as a consequence of the application of the specific

contagion. It is certain, in the third place, that (besides the dog) the wolf, the fox, the jackal, and the cat, have communicated the disorder to the human animal. Mr. Youatt affirms, that the saliva of the badger, of the horse, and of the human being, has caused rabies; and I mentioned, on his authority, a case in which a groom contracted the disease from a scratch which he received while administering a ball to a rabid horse. But I feel much less certain about these latter animals. Respecting the dog, the fox, the wolf, the jackal, the cat, there can be no question. The result of certain experiments made at the Veterinary School, at Alfort, is opposed to Mr. Youatt's statement. Professor Dupuy made wounds in cows and sheep, and rubbed upon these wounds sponges which had been chewed by rabid animals of the same species; but he never succeeded in communicating the disorder in this way: but when he used a sponge that had been mumbled by a mad dog, then the disease occurred in the sheep and cows.

It is still more interesting to enquire, whether the saliva of a human being, labouring under hydrophobia, is capable of inoculating another human being with the same complaint? Mr. Youatt says yes: that the disease has undoubtedly been so produced. If this be so, the fact will teach us—not to desert or neglect these unhappy patients, still less to murder them by smothering—but to minister to their wants with certain precautions; so as not to suffer their saliva to come in contact with any sore or abraded surface; nor, if it can be avoided, with any mucous surface. On the other hand, all carefulness of that kind will be unnecessary, if the disease cannot be propagated by the human saliva. Certainly many experimenters have tried in vain to inoculate dogs with the spittle of a hydrophobic man: but there is one authentic experiment on record, which makes it too probable, that the disease, though it may not be communi-

cated often, or easily, is yet communicable. The experiment is said to have been made by MM. Magendie and Breschet, at the Hôtel-Dieu, and to have been witnessed by a great number of medical men and students. Two healthy dogs were inoculated, on the 19th of June, 1813, with the saliva of a patient, named Surlu, who died of hydrophobia the same day, in that hospital. One of these dogs became mad on the 27th of the following month. They caused this dog to bite others, which, in their turn, became rabid also; and in this way they propagated the malady, among dogs, during the whole summer. Now this is a very striking fact, yet it ought not to be considered conclusive: for it is possible that the dog might have gone mad at that time, whether he had been so inoculated, or not. It may have been a mere coincidence. We want repetitions of such experiments to settle the point: nevertheless we have enough in this one experiment to make us use all necessary caution when engaged in attending upon a hydrophobic patient.

I just touched upon the question, whether the saliva of a rabid dog could produce the disease if it fell upon the *sound skin*? The first of the two cases which I related as having been witnessed by myself, would appear to give an affirmative answer to this question. Mr. Youatt thinks the disease would *not* follow such an application of the virus; but that it cannot be received upon even the unbroken surface of a mucous membrane without the greatest danger. Horses are said to have died mad after eating straw upon which rabid pigs had died. Portal was assured that two dogs, which had licked the mouth of another dog that was rabid, were attacked with rabies seven or eight days afterwards. Mr. Gillman, of Highgate, in a little pamphlet on Hydrophobia, quotes an instance from Dr. Perceval, in which a rabid dog licked the face of a sleeping man, near his mouth, and the man died of hydrophobia, although the strictest search failed to discover the smallest scratch or abrasion on any part of his skin.

At the very close of the lecture I observed, that even should it be clearly proved that hydrophobia has ever resulted from the scratch of a rabid animal's *claws*—the claws of a cat, for example—we are not to set it down as a sure thing that the disease can be introduced into the system independantly of the saliva of the diseased animal. As we know that dogs and cats are in the habit of putting their paws to their mouths when they feel uneasy there, we may readily understand how the poisonous saliva may be introduced by a mere scratch with the creature's nails. Mr. Youatt believes that the saliva *only* is capable of conveying the disease.

4. Supposing the virus to have been in-

serted in the part bitten, what becomes of it? Is it immediately taken into the system, and does it, like the poison of small-pox, in some mysterious way, multiply and diffuse itself in the body, until the disease explodes? Or does it remain imprisoned in the wound, or in the cicatrix, for a time? This is an important practical question. For if the poison lurks for some weeks in the place where it was originally deposited, we might successfully remove it at any time between the infliction of the bite and the period of recrudescence. Now the facts that at this period of recrudescence the wound or scar is re-inflamed often, and almost always becomes the seat of some fresh morbid phenomena, pain, swelling, numbness, and the like, spreading towards the trunk—and that, *soon after this*, the peculiar paroxysmal symptoms begin—these facts are strong in favour of the belief that the poison does lie inert in the place of the original hurt, for some time. Dr. Bardsley states that the recrudescence pains seem always to follow the course of the nerves, and do certainly never inflame or irritate the lymphatic glands in the vicinity, though passing in a parallel course towards the trunk. He affirms the entire absence of any fact contrary to this observation in the works of the numerous authors who have written on the subject. I mention this statement, because it certainly is not correct. Mr. Mayo says, “in one case which I witnessed and examined after death, the inner part of the cicatrix was bloodshot; and a gland in the axilla had swelled at the coming on of the hydrophobic symptoms.” And I find, among my notes of Mr. Abernethy's lectures, another striking case, still more to the point. “A very intelligent boy had been bitten by a dog in the finger: he was brought into St. Bartholomew's Hospital. Caustic had been liberally used, affecting the sinewy parts, and producing a terrible sore: yet the boy was recovering himself, and the sore was healing. One day, as Mr. Abernethy was going round the hospital, he saw and spoke to the boy, who said he thought himself getting well, but that he had that day an odd sensation in his fingers, stretching upwards into his hand and arm. Going up the arm, Mr. Abernethy saw two red lines, like inflamed absorbents: they doubtless were so. He affected to make light of the matter, ordered a poultice, and recommended the boy to take some medicine. Early the next morning Mr. Abernethy visited the ward, pretending he had some other patient there, whom he wished particularly to see: and when going out again, he asked the boy, carelessly, how he was. He said that he had lost the pain, but that he was very unwell, and had not slept all night. Mr. Abernethy felt his pulse, told

him he was a little feverish, as might be expected, and asked him if he were not thirsty, and would like some toast and water. The boy said he *was* thirsty, and that he *should* like some drink: when, however, the cup was brought, he pushed it from him; he could not drink. In forty-eight hours he was dead.

Facts such as these would lead to the conclusion that, in cases in which excision had not been performed in the first instance, the scar, or the sore, might be cut out with propriety at any time before the period of recrudescence: and if the case happened to be my own, I would have this done even at that period, the moment any new sensation manifested itself in the seat of the injury. Mr. Mayo, on the same grounds, advocates the removal of the cicatrix, even although the hydrophobic symptoms may have appeared. I do not mean to say that the facts, now referred to, show with any certainty that the poison remains in the place where it was first deposited until the phænomena of recrudescence take place; but they afford some presumption in favour of that notion: and, in such a disease as hydrophobia, we are bound to act upon the very lowest presumption that affords a chance for our patient's life. The poison may be absorbed into the general system at the period of recrudescence, although no affection of the absorbing vessels or glands should be manifest: through the veins, namely.

Considering the matter philosophically, we might be inclined to suppose that the poison was silently maturing its force in the general system during the period of incubation, just as the poison of small-pox and measles are presumed to do. But looking at it practically, I should recommend, under the circumstances already stated, the excision of the cicatrix.

5. Another important question is this. Is a man who has been bitten by a mad dog, and in whose case no precautions have been taken, a doomed man? will he be sure to have the disease, and therefore to die of it? By no means. But few, upon the whole, of those who are bitten, become affected with hydrophobia.

It is curious that different species of animals appear to be susceptible of hydrophobia in different degrees. Thus, according to Mr. Youatt, two dogs out of three, bitten by one that is rabid, become rabid. The majority of horses inoculated with the virus, perish. Cattle have a better chance: perhaps because in them the skin is looser and less easily penetrated. A full half (he thinks) of those that were seized by a mad dog, would escape. With sheep the bite is still less dangerous. He reckons that not more than one in three would be affected. The tooth, perhaps, has been wiped clean in its passage

through the wool. The human being is least of all in danger. John Hunter states that he knew an instance in which, of twenty-one persons bitten, one alone became affected with hydrophobia. Dr. Hamilton estimates the proportion to be one in twenty-five. But I fear these computations are much too low. In 1780, a mad dog, in the neighbourhood of Senlis, took his course within a small circle, and bit fifteen persons before he was killed: three of these died of hydrophobia. The slaver of a rabid wolf would seem to be highly virulent and effective. These beasts fly always, I believe, at a naked part. Hence, probably, the fatality of their bites. The following statement applies exclusively to the wolf. In December 1774, twenty persons were bitten in the neighbourhood of Troyes; nine of them died. Of seventeen persons similarly bitten in 1784, near Brive, ten died rabid. In May 1817, twenty-three persons were bitten, and fourteen perished. Four died out of eleven that were bitten near Dijon: and eighteen of twenty-four bitten near Rochelle. At Barsur-Ornain, nineteen were bitten, of whom twelve died of hydrophobia within two months. Here we have one hundred and fourteen persons bitten by rabid wolves, and among them no less than sixty-seven victims; considerably more than one-half. There is no doubt, however, that a majority of persons who are bitten by a mad dog escape the disease. This may partly be owing to an inherent inaptitude for accepting it. We see some persons who, though often in the way of it, do not contract syphilis; there are others upon whom the contagion of small-pox has no influence. This difference exists, apparently, even among dogs. There was one dog, at Charenton, that did not become rabid after being bitten by a rabid dog; and it was so managed that, at different times, he was bitten by thirty different mad dogs; but he outlived it all. Much will depend also upon the circumstances and manner in which the bite is inflicted; if it be made through clothes, and especially through thick woollen garments, or through leather, the saliva may be wiped clean away from the tooth before it reaches the flesh. In the fifth volume of the *Edinburgh Medical and Surgical Journal*, there is a case described by Mr. Oldknow, of Nottingham, in which a man was bitten in three different places by the same dog; viz. in the scrotum, the thigh, and the left hand; the bite on the hand was the last. Now it seems not improbable that but for this last bite, on a naked part, he might have escaped. At least it was a remarkable circumstance that the phænomena of recrudescence occurred only in the hand and arm. The dog is supposed to have closed his mouth after inflicting the first two bites; and thus to

have charged his teeth afresh with the poisonous saliva.

It is this frequent immunity from the disease in persons who have been bitten, that has tended to confer reputation upon so many vaunted methods of prevention. Ignorant persons, and knavish persons, have not failed to take advantage of this. They announce that they are in possession of some secret remedy which will prevent the virus from operating: they persuade the friends of those who die that the remedy was not rightly employed, or not resorted to sufficiently early: and they persuade those who escape that they escape by virtue of the preventive remedy. If the plunder they reap from the foolish and the frightened was all, it would be of less consequence; but unfortunately the hope of security without undergoing a painful operation leads many to neglect the only sure mode of obtaining safety.

Mr. Youatt is of opinion that the power of the virus ceases with the life of the animal. He states, that in many dissections of the dog, the saliva, in spite of all care, must have come in abundant contact with his hands, and they were not always sound. I should strongly recommend you not to act upon this opinion: but to use the same precautions, in dissecting a rabid animal, as you would use if you were persuaded that the disease might be communicated with equal certainty before and after the death of the animal.

There are some considerations respecting this disease, which relate both to the biter and to the bitten; the canine and the human being. And there are some which relate exclusively to the dog, yet concerning which we, as medical philosophers, ought not to be ignorant. I shall advert to a few of these.

One question I have already glanced at; viz. whether the disease may be produced by a healthy, though angry dog or cat. I referred to one instance in which this was supposed to have been the case; and I repeat that I should be more inclined to think, unless we had other examples of the same kind, that the person had been inoculated in some way that he was not aware of. But I have heard Mr. Youatt describe cases in which there had been no symptoms of rabies observed in the dog at the time the injury was inflicted, though soon afterwards the animal became decidedly rabid. It is much to be regretted that the dog is so often destroyed. When a person has been bitten by a dog or cat suspected to be rabid, the beast ought to be secured, and kept under surveillance, and suffered, if it shall so happen, to die of his disease. If he does not die, in other words if he be really not rabid, that will soon appear; and the mind of the patient will then be relieved from a very

painful state of suspense and uncertainty, which might otherwise have haunted him for months or years. If the dog dies mad, the injured person will be no worse off than if the animal had been killed in the first instance: nay in one respect he will be better off, inasmuch as certainty of evil is preferable to perpetual and uneasy doubt. "Give a dog a bad name, (says the proverb) and hang him:" and it is literally so with the imputation of madness. A poor wretch of a dog is perhaps ill, or weary, or cross, or he may have been worried already by mischievous boys: the cry of mad dog is raised; and then he can expect no mercy. There are gross errors prevalent with regard to the signs of madness in the dog. If a dog be seen in a fit in the street, some person charitably offers a conjecture that perhaps he may be mad; the next person has no doubt about it; and then, woe to that dog! But Mr. Youatt assures us that the rabid dog never has fits: that the existence of epilepsy is a clear proof that there is no rabies. Again, it is a very common belief that a rabid dog, like a hydrophobic man, will shun water; and if he takes to a river, that is thought to be conclusive evidence that he is not mad. But the truth is, that the disease, in the quadruped, cannot be called *hydrophobia*: there is no dread of water, but an unquenchable thirst; no spasm attending the effort to swallow, but sometimes in dogs an inability to swallow, from paralysis of the muscles about the jaws and throat. They will stand lap, lapping, without getting any of the liquid down. They fly eagerly to the water, and Mr. Youatt states that all other quadrupeds, with perhaps an occasional exception in the horse, drink with ease, and with increased avidity. This erroneous impression is not confined to the vulgar. In the case which I have more than once alluded to, and which is mentioned in Hufeland's Journal, of a lad who died of hydrophobia after having been bitten by a dog that had not been and was not mad, one circumstance stated in evidence of the animal's freedom from rabies is, that he drank without difficulty a large quantity of water.

There is another superstitious notion not at all uncommon, viz. that healthy dogs recognize one that is mad, and fear him, and run away from his presence, in consequence of some mysterious and wonderful instinct warning them of danger. This is quite unfounded. Equally mistaken are the notions that the mad dog exhales a peculiar and offensive smell, and that he may be known by his running with his tail between his legs; except, as Mr. Youatt says, when, weary and exhausted, he is seeking his home.

It will not be out of place to state what are the symptoms of rabies as observed in the dog, and as described by Mr. Youatt.

The earliest symptoms of madness in the dog, (he says) are sullenness, fidgetiness, continual shifting of posture, a steadfast gaze, expressive of suspicion, an earnest licking of some part, on which a scar may generally be found. If the ear be the affected part, the dog is incessantly and violently scratching it. If it be the foot, he gnaws it till the integuments are destroyed.

Occasional vomiting and a depraved appetite are very early noticeable. The dog will pick up and swallow bits of thread or silk from the carpet, hair, straw, even dung; and frequently he will lap his own urine, and devour his own excrement. Then the animal becomes irritable; flies fiercely at strangers; is impatient of correction; seizes the stick or whip; quarrels with his own companions; eagerly hunts and worries the cats; demolishes his bed; and if chained up makes violent efforts to escape, tearing his kennel to pieces with his teeth. If he be at large he usually attacks only those dogs that come in his way; but if he be naturally ferocious he will diligently and perseveringly seek his enemy. According to Mr. Youatt, the disease is principally propagated by the fighting dog in towns; and by the cur or lurcher in the country: by those dogs, that is, that minister to the vices of the lower classes in town and country respectively. He maintains that if a well-enforced quarantine could be established, and every dog in the kingdom confined separately for seven months, the disease might be extirpated. This opinion is founded of course in the belief that rabies never originates at present, any more than small-pox does, *spontaneously*; but is always propagated by the specific virus. And it is corroborated by the fact that rabies and hydrophobia are unknown in some countries: I fancy that South America is, or was, a stranger to it. It appears to have been imported into Jamaica, after that island had enjoyed an immunity from the disease for at least fifty years previously; and Dr. Heineken states that curs of the most wretched description abound in the island of Madeira; that they are afflicted with almost every disease, tormented by flies, and heat, and thirst, and famine, yet no rabid dog was ever seen there. On the contrary 1666 deaths from hydrophobia, in the human subject, are stated to have occurred in Prussia in the space of ten years.

Very early in the disease, as it appears in the dog, the expression of countenance is remarkably changed; the eyes glisten, and there is slight strabismus. Twitchings of the face come on. About the second day a considerable discharge of saliva commences; but this does not continue more than ten or twelve hours, and is succeeded by insatiable thirst: the dog is incessantly drinking, or attempting to drink: he plunges his muzzle

into the water. When the flow of saliva has ceased he appears to be annoyed by some viscid matter in the fauces; and in the most eager and extraordinary manner he works with his paws at the corners of his mouth to get rid of it: and while thus employed he frequently loses his balance and rolls over.

A loss of power over the voluntary muscles is next observed. It begins with the lower jaw, which hangs down, and the mouth is partially open; but by a sudden effort the dog can sometimes close it, though occasionally the paralysis is complete. The tongue is affected in a less degree. The dog is able to use it in the act of lapping; but the mouth is not sufficiently closed to retain the water. Therefore, while he hangs over the fluid, eagerly lapping for several minutes, it is very little or not at all diminished. The paralysis often attacks the loins and extremities also. The animal staggers about, and frequently falls. Previously to this he is in almost incessant action. Mr. Youatt fancies that the dog is subject to what we call spectral illusions. He says he starts up and gazes eagerly at some real or imaginary object: he appears to be tracing the path of something floating around him, or he fixes his eye intently upon some spot in the wall, and suddenly plunges at it; then his eyes close, and his head droops.

Frequently, with his head erect, the dog utters a short and very peculiar howl: or if he barks, it is in a hoarse inward sound, altogether dissimilar from his usual tone, and generally terminating with this characteristic howl. Respiration is always affected: often the breathing is very laborious; and the *inspiration* is attended with a very singular grating, choaking noise. On the fourth, fifth, or sixth day of the disease, he dies: occasionally in slight convulsions; but oftener without a struggle.

Mr. Youatt gives a detailed account of the appearances met with after death in the carcasses of these rabid dogs. They are not very constant or distinctive. The most curious and uniform consist in the presence of unnatural ingesta in the stomach; straw, hay, hair, horse-dung, and earth. Sometimes the stomach is perfectly distended with these substances; and when it contains none of them, there is a fluid resembling the deepest chocolate mixed with olive; or still darker, like coffee: and when neither the unnatural ingesta nor the dark fluid appear, it will be found, Mr. Youatt says, upon careful enquiry, that the dog has vomited much hair, hay, straw, or the like.

In 1837, a few days after the case of hydrophobia occurred in the Middlesex Hospital, I saw the carcass of a dog, that had died rabid, examined by Mr. Ainslie at his and Mr. Youatt's Infirmary. The most remarkable morbid appearances were

in the stomach, which contained some bits of straw and stick, and a considerable quantity of a dark fluid like thin treacle. In various parts of the stomach there were spots, almost black, of a considerable size; apparently produced by dark blood partly extravasated beneath, and partly incorporated with, the mucous membrane.

I believe that Mr. Youatt's opinion, already mentioned, of the cause of rabies in dogs, and in all creatures—viz. that it always results from the introduction of a specific virus into the system—I believe this opinion is not commonly entertained. Most people think that the disease is generated, *de novo*, in the dog at least, and causes have been assigned for it which certainly are not the true or the sole causes. Thus hydrophobia in the dog has been ascribed to extreme heat of the weather; it is thought by many to be particularly likely to occur in the dog-days: and many cautions are annually put forth, about that period, for muzzling dogs, and so on: very good and proper advice, but it would be as appropriate, if those who have noted the statistics of the disease may be depended upon, at one period of the year as at another. Rabies occurs nearly as often in the spring, in the autumn, and even in winter, as it does in summer. M. Trollet, who has written an interesting essay on rabies, states that January, which is the coldest, and August, which is the hottest month in the year, are the very months that furnish the fewest examples of the disease. The disorder has often been ascribed to want of water in hot weather, and sometimes to want of food. But MM. Dupuytren, Breschet, and Magendie, have caused both dogs and cats to perish with hunger and thirst, without producing the smallest approach to a state of rabies. At the Veterinary School, at Alfort, three dogs were subjected to some very cruel but decisive experiments. It was during the heat of summer, and they were all chained in the full blaze of the sun. To one salted meat was given; to the second water only; and to the third neither food nor drink. They all died; but none of them became rabid. Nor does the supposition that the disorder has some connexion with the period of sexual heat in these animals appear to have any better foundation.

If you are desirous of knowing what my own opinion on this matter is, I must say that I think Mr. Youatt's doctrine by far the most probable one; that rabies never occurs except from inoculation of the specific virus. It has never been proved, and indeed it would scarcely be susceptible of proof, that the disease ever breaks out spontaneously; large tracts of country are totally free from it; and in 19 cases out of 20, perhaps, we trace the

bite or the fray in which the inoculation has been effected.

If I were asked to define the seat of this terrible disease, I should place it, without hesitation, in that division of the nervous system which comprises the excito-motory apparatus; the true spinal marrow, with its appendages of afferent and efferent nerves. Nay, I should go farther, and say that it is the upper part of this apparatus, of which the functions are primarily and chiefly deranged: that the poison acts mainly upon the nervous arcs which pertain to the throat, and with which the eighth pair of nerves in particular is connected. There is nothing singular in this localization of the influence of a specific poison. The ergot of rye affects principally those arcs which belong to the uterus; cantharides those which govern the muscular fibres of the bladder. It is true that the mental functions are remarkably modified, and that paralysis of the lower extremities occurs, in most instances of the disease. But neither of these phænomena are constant; and they simply illustrate, when they do happen, the facility with which any morbid state of the spinal cord may propagate its influence in either direction. Whether, in hydrophobia, the essential change be centric or eccentric, cannot be determined with any thing like certainty: but it seems to me to be most probable that the sensibility of the afferent nerves of the fauces, of the skin, and of the air-passages, is altered or morbidly exalted; whence, upon the application of the exciting stimulus, the peculiar sighing dyspnœa, and the strangling dysphagia, are produced by a reflected influence through the central axis upon the muscles concerned in these actions. But, as I said before, the pathology of the excito-motory apparatus is as yet in its new birth.

What can I say of the *treatment* in hydrophobia; or in rabies? There is no well-authenticated case on record, that I am aware of, in which a hydrophobic person has recovered. As it has been, so it is still, *ιατρος ιαται θανατος*. The physician that cures is death. There can be no ground therefore for the recommendation of any especial drug, or form of medicine, or even for any general plan of treatment, after the peculiar symptoms of the disease have once set in.

Of course those powerful remedial agencies that are in common use among medical men, have been fairly tried: copious blood-letting, mercury, opium, arsenic, sugar of lead, oil of turpentine, the cold affusion even: and not only those, but the strong poisons that are sometimes, but not so generally, employed for other diseases; belladonna, stramonium, prussic acid,

white hellebore, strychnia, cantharides, the nitrous oxide gas: and no end of less gigantic remedies; such as alkalies, and especially ammonia, carbonate of iron, electricity and galvanism, tobacco-juice, and the guaco (which was introduced into this country a few years ago with high encomiums for its power over the disease), the mineral acids, violent exercise: and if we take into account the substances administered to the brute also, we may increase this list by the alisma plantago, scutellaria, box, and rue, all of which, at one time or another, have been vaunted as successful remedies, veratrum sabadilla, and ticunas poison.

The difficulty of swallowing fluids, and in some cases of swallowing at all, is a serious obstacle to the fair trial of almost every form of internal remedy. It has been proposed to introduce powerful medicines into the rectum, in clysters: but to this also the patients have been found to make great resistance. The injection of medicines into the veins has been tried; Magendie hoped that he had discovered a cure, in first largely bleeding the patient, and then injecting his veins with a corresponding quantity of warm water: but it has always happened with this, and with other promising experiments, that just as the patient seemed to be about to recover, he has died. The nervous irritability has in one case or two been much calmed by the injection of a solution of the acetate of morphia into the veins.

Mr. Mayo has suggested bronchotomy: upon this ground, (to use his own words) "that the principal character of the disease, and the rapid exhaustion which attends it, appear to depend in great part upon the fits of spasm and closure of the glottis, brought on, not merely by the attempt, or the idea of drinking, but by any sudden impression upon the senses. Now it is clear, he adds, that as far as the distressing feelings in the throat consist in a sense of suffocation, they would be put an end to or relieved by the establishment of a free opening in the windpipe." Dr. Marshall Hall would use, in combination with tracheotomy, the hydrocyanic acid. Now I should be sorry to say any thing to damp your reasonable hope of benefit from any experiment; but I am bound to confess to you that I should not expect the smallest advantage from tracheotomy in this disease. The mode of death offers no encouragement to its use. There may be spasm of the glottis, but I doubt it; at any rate the patients do not die of suffocation; the death is not death by apnoea, but by asthenia. We see persons labouring greivously for their breath for hours together, who yet survive, and are presently themselves again; persons,

for instance, who are affected with severe spasmodic asthma. I have seen a man sitting up in bed a whole night long, inspiring with such difficulty that, if I had not been aware of his having scores of times been as bad before, I should have thought he could not exist five minutes longer. Now we have nothing of this dyspnoea in hydrophobia: and, as I said already, I am sorry, and diffident too, when I differ from great authorities on practical points, but I see no hope of cure, nor even of sufficient benefit, to counter-balance the inconvenience and hazard of the operation, from the performance of bronchotomy. The principle is that of suffering the parts gradually to recover themselves, and of allowing the patient in the meanwhile to breathe through another channel. The principle is excellent, (as I shall shew you by and by,) where there is a permanent obstacle to the admission of air to the lungs through the larynx; but in hydrophobia there is no such permanent obstacle to surmount. Though your patient, in laryngitis, should be at the point of death, yet open his wind-pipe, and he breathes again and is safe; but it is not at all uncommon for a hydrophobic patient to lose his spasms, to swallow well, and to breathe easily, yet he does not recover. This amendment is the prelude of death, the last flicker of the expiring lamp. Since I lectured upon this subject last year, Dr. Latham has told me the following circumstance respecting a patient whom he treated for hydrophobia, in the Middlesex Hospital. He went one day to the ward, fully expecting to hear that the patient was dead. But he found him sitting up in his bed quite calm, and free from spasm; and he had just drunk a large jug of porter. "Lawk, sir, (said a nurse who sat by) what a wonderful cure!" The man himself seemed surprised at the change. But *he had no pulse*; his surface was cold as marble. In half an hour he sunk back, and expired. Furthermore the experiment in question has been tried, and it has been tried by its proposer Mr. Mayo, upon the dog, without affording, as Mr. Youatt assures us, the slightest relief. In the matter of cure, surgery, I fear, is as impotent as physic.

Not so, however, in the matter of prevention: this is the most important part of the practice. The early and complete excision of the bitten part is the only measure in which we can put any confidence: and even here we are met with a source of fallacy. In the majority of cases, no hydrophobia would ensue, though nothing at all were done to the wound. How can we know, then, that the disease is ever prevented by its excision? No doubt many persons go

through the pain of the operation needlessly. But we can never be sure of this. They get at any rate relief from the most harassing suspense, with which they would be probably tortured for months. And if a large number of bitten persons, who had suffered the wound to heal as it would, could be compared with the same number who had had the bitten part cut out, hydrophobia would be found a frequent consequence of the bite in the first class—a very rare consequence of it in the second. Mr. Youatt, who trusts to caustics, and who has himself been bitten seven times, and is yet alive and well, tells us that he has operated, with the caustic, on more than four hundred persons, all bitten by dogs, respecting the nature of whose disease there could be no question; and that he has not lost a case. One man died of fright, but not one of hydrophobia; and he says also that a surgeon of St. George's Hospital told him that ten times that number had undergone the operation of excision there, after being bitten by dogs (all of which might not, however, have been rabid), and that it was not known that any one had been lost. Mr. Youatt, I say, trusts to caustic; and the caustic he uses is the nitrate of silver. But I advise you to trust to nothing but the knife, if the situation of the bite will allow you to employ it effectually. If the injury be so deep or extensive, or so situated, that you cannot remove the whole surface of the wound, cut away what you can; then wash the wound thoroughly, and for some hours together, by means of a stream of warm water, which may be poured from a tea-kettle; place a cupping-glass from time to time over the exposed wound; and finally apply to every point of it a pencil of lunar caustic. If you cannot get the solid caustic in contact with every part, you had better make use of some liquid escharotic; the nitric acid, for example. In my own case—and what I should choose for myself I should advise for another—if I had received a bite from a decidedly rabid animal upon my arm or leg, and the bite was of such a kind that the whole wound could not be excised, my reason would teach me to desire, and I hope I should have fortitude enough to bear, amputation of the limb, above the place of the injury.

But if the wound is of such a size, and in such a part, that it can be excised, what is the proper way of cutting it out? If I were to give you any opinion, as from myself, upon that point, you might think, perhaps, that I was stepping beyond my proper province. I shall, therefore, again retail to you the advice of my old master, Mr. Abernethy. "The cell (he says) into which a penetrating tooth has gone, must be cut out. Let a skewer be shaped, as nearly as may be, into the form of the tooth, and then be placed in

the cavity formed by the tooth; and then let the skewer, and the whole cell containing it, be removed together by an elliptical incision. We may examine the removed cell, to see if every portion with which the tooth might have come in contact has been taken away: the cell may even be filled with quicksilver, to see if a globule will escape. The efficient performance of the excision does not depend upon the extent, but upon the accuracy, of the operation." Mr. Abernethy was of opinion that when once the poison had been imbibed into the system, nothing ever had done good, and nothing, probably, ever would. I should be sorry to be so absolutely despairing in respect to a disorder in which dissection after death discloses no reason why the patient *might* not recover. He used to add, that as bleeding had been much extolled, had he hydrophobia he would allow a surgeon to bleed him, even to death. Like Seneca he would be willing to have his veins opened, though his disease might not permit him to indulge at the same time, like Seneca, in the luxury of a warm bath.

I say *early* excision is the only sure preventive; but let me repeat that it will, in all suspicious cases, be advisable, if, for any reason, the operation has been omitted in the first instance, to cut out the wound, or the cicatrix, at any time before the symptoms of recrudescence have appeared. One would do it, though with less hope, as soon as possible *after* they had appeared: but I do not expect to hear of excision being successful then in stopping the disease. Dr. Bright has recorded a case in which the arm was amputated upon the supervention of tingling, and other symptoms, in the hand, in which the patient had been bitten some time before; but the amputation did not save him.

It has been proposed to fill the wound with ink, and then to wash it until every trace of the ink is gone; in this way, it is conceived, the complete ablation of the poison also will be ensured. With a timid or an obstinate patient, who would not submit to the knife or the caustic, some such expedient ought to be diligently tried: but it would be better to try it *after* the excision, or after the application of the escharotic substance. It is impossible to take superfluous pains to obviate so fearful a disease as hydrophobia.

It has been recommended, after the wound has been excised or cauterized, that it should be prevented from healing, and made to discharge for a long time, by means of irritating applications. This may be advisable when thorough excision, or complete cauterization, cannot be effected; but I should think it quite useless as auxiliary to those expedients, and only likely to keep up, or to produce, a hurtful irritability of the system.

I should perhaps have mentioned before, a theory, and a plan of preventive treatment, which made a great figure in all the journals, foreign and domestic, a few years ago. It was pretended by a Russian physician, Dr. Marochetti, that some time between the third and the ninth day after a person had been inoculated with the hydrophobic poison, by the bite of a rabid dog, small pustules appear on or about the frænum of the tongue, containing a small quantity of sanious fluid, of a yellow or greenish colour. The same kind of pustules were declared to exist also under the tongues of the mad dogs themselves. Now Dr. Marochetti pretended farther, that if, from the very time of the bite, you gave the patient large doses of the decoction of broom tops, and looked out for the eruption of these pustules, which seldom lasted more than twenty-four hours, you might infallibly prevent the disease by opening and emptying the pustules, and then cauterizing them with a red-hot iron; and afterwards causing the patient to gargle his mouth with that same decoction of broom. He held that the poison was deposited there for a short time, and then reabsorbed into the system; and he proposed to prevent such reabsorption. This was a very pretty theory; and took mightily in the medical world. But it has turned out a sort of hoax. I do not mean a wilful hoax on the part of Dr. Marochetti; for I have no doubt that he contrived to hoax himself. These pustules have been looked for again and again; but they have never been discovered in Englishmen affected with hydrophobia; nor in English mad dogs. The truth seems to be that the mucous follicles of the mouth, generally, and those at the base of the tongue, and those beneath the tongue, in particular, are commonly enlarged and exaggerated in the dog, and in the human animal, labouring under the disease; and these enlarged and altered follicles were regarded by the Russian physician as a specific eruption, that furnished the virus and pabulum of the complaint.

As almost every drug that has ever been included in any Pharmacopœia has been administered with the hope of *checking* the disease, so a great number of medicines and measures have been praised as preventives. Some people have great faith in sea bathing; and they go to the coast to be ducked and half drowned every day for six weeks: and if they escape hydrophobia they conclude that the immersion in the salt water has saved them. Some of the specifics, as you may suppose, are great secrets; and they who possess them—whether they believe in them or not is another matter—sell them at no cheap rate to those who have been bitten by the dog, and are weak enough to be bitten again by the quack. The compo-

sition of several of them has transpired; and they are found to consist either of ingredients the most insignificant and worthless, or of poisons of which the inefficacy had already been ascertained. The celebrated *pulvis antilyssus*, which was introduced by no less a person than Dr. Mead, into the London Pharmacopœia, was a mixture of ash-coloured liverwort and black pepper. The *Ormskirk medicine*, long famous, and scarcely obsolete yet in the north of England, was made up of bole armeniac, alum, chalk, elecampane, and oil of aniseed. The *Tonquin medicine* was composed of cinnabar and musk: and the *Tanjore pills* were a combination of mercury and arsenic. Even now scarce a year elapses but some correspondent of the newspapers, whose philanthropy is more conspicuous than his judgment or his knowledge, recommends a new and infallible preventive. I confess to you that I have not the slightest faith in any one of them: but as I have a great respect for Mr. Youatt, and as he is not *quite* so sceptical as I am on this point, and as patients or their friends will insist upon the adoption of protective measures sometimes, when the local means of prevention have been omitted or imperfect, I will tell you what he (Mr. Youatt) has done in respect to those prophylactic drugs.

In the first place he never succeeded in curing the disease in the dog with any thing that he ever tried.

In the way of prophylaxis, he experimented with a great number of substances. He thought that the box-wood, which is the basis of some celebrated preventive drinks in Hertfordshire and Kent, had some effect. He tried the alisma plantago, the boasted efficacy of which had been strictly inquired into by the magistracy of Toula, and the receipt purchased by the Russian Government at an immense price. But he had no success with it. He then put the belladonna to the test, beginning with two grains, and increasing the dose to a scruple twice every day, and continuing this for six weeks: and he says he is confident that he saved several dogs; but he lost almost as many. They all became debilitated and most rapidly emaciated.

Then, in the year 1820, his attention was directed to the *scutellaria lateriflora*, which Dr. Spalding, an American physician, had found highly successful as a preventive of rabies: and upon trial of it, he soon was brought to regard it as really valuable: and (not to tire you with a detail of his proceedings in the interim) he at length combined it with belladonna; “and the result,” (I here quote his own language) has been a medicine which I cannot, dare not, call a specific; for it has failed: but the use of which, in the cases of doubt and fear to

which I have alluded, I would most earnestly recommend." He relates two experiments, which seem to have made a great impression upon his mind. They are as follows:—

"Three pieces of tape were thoroughly moistened with the saliva of a rabid dog, and inserted as rowells in the polls of three dogs. To two the scutellaria and belladonna were given: the third, a fox-hound bitch, was abandoned to her fate. On the 29th day after the inoculation she became rabid." The others, at the time this was written, *i. e.* some months afterwards, were living and well.

He afterwards took the same two dogs, and a third. He moistened two pieces of tape with the saliva of a rabid dog, and inserted them in the polls of one of the old dogs, and of the third dog. Another piece of tape dragged repeatedly through the mouth of the same rabid dog *twenty-four hours after its death*, was inserted in the poll of the second of the old dogs. This dog and the new one were suffered to take their chance. To the other old dog the medicine was given. In the fourth week the new dog died undeniably rabid. The other two survived.

I repeat that I have no faith in these preventives. But *sometimes* some of them must be tried; and I would prefer those which are thus sanctioned by Mr. Youatt's good opinion to any others.

And with respect to the established disease, I think that if I were the unhappy subject of it, I should wish to be put into a hot air bath, and thoroughly sweated, and to take opiates; not so much in the hope of recovering as with a view to the euthanasia. But, with all respect to those gentlemen who advocate that practice, no one, if I could help it, should make a hole in my wind-pipe.

CLINICAL LECTURES,

BY DR. CORRIGAN,

*Delivered at the Hardwicke Fever Hospital,
Dublin,*

During the Session, 1840-41.

LECTURE VII.—FEVER, No. 5.

Case of Gray; post-mortem examination of, shewing that death had occurred without any structural lesion. Follicular Enteritis; case of Mary Cope; case of Purcell; treatment of, by sponge suppositories; case of Graham; peculiar form of Pneumonia.

IN accordance with the analysis we have made of fever, we should come now to the derangement of the function of respiration; but, before going into general observations, I wish to direct your attention to three cases, which have occurred within the last fortnight, which

are connected with the pathology of fever, and one of them more particularly with the immediate subject before us.

The first of these cases is that of William Gray, a groom, *ætat.* 40. He was admitted on February 11th, then ill three days. He was suffering under very great prostration of strength; his face was congested, the depending portions of the body and extremities were of a livid red colour, and the surface was cold. The maculæ were few and pale, and badly developed; his pulse was very quick and weak; respiration was scarcely disturbed; there was no tympanites. He had not delirium. He slept, and he complained only of slight headache. On the previous Monday, in his passage over from England, he had been much exposed to cold and wet. Notwithstanding all endeavours to rouse him, by wine, blisters, bark, and ammonia, his pulse continued to grow feebler; but he slept, and he was not delirious, until a few hours before his death, when he became comatose, and died on the second day after his admission into the hospital, the fifth day of his illness.—This man was completely struck down by the severity of the fever. It was the worst, the most rapidly fatal case of typhus, we had had in the hospital for many months; yet what did we find on examination of the body of this patient? No organic disease any where. The brain was perfectly sound; the heart was remarkably firm and muscular; and the most careful examination could detect no trace of disease in the intestines. So little tendency, indeed, was there to any affection of the glands of the intestines, that they were not even sufficiently developed to enable us to detect them with the naked eye. In the lungs there was some amount of hypostatic congestion, but this was all the most careful examination could discover. Structural pathology, then, tells us nothing of the cause of death in this case: there was no local alteration of structure to which by possibility could even be referred the symptoms or death of this individual; but, if we apply to the case functional pathology—if we use in considering it the analysis we have already made of fever—we can understand its nature. From the very commencement of the case there was a sinking of one of the most important functions of the living body—the circulation. The capillaries had completely lost their tone; mere gravitation they were even unable to contend against; the depending portions of the body were everywhere of a livid red; and even in the face, where there might have been expected to be some action, there was so little, that, while the vessels were distended with blood, there was scarcely any development of animal heat; the surface was cold; the state of the function of circulation through the whole system, seemed, in truth, to be one of

complete atony. This state of so important a vital function, the distension of the capillaries, and the want of almost any development of animal heat, made me mark the case on its very admission as one of a hopeless character. There is no necessity for seeking for structural change to account for death in such a case. There is no difficulty in understanding that death, in such case, will follow from the sinking of so important a vital function as circulation; no more difficulty in supposing death to be the result of this functional derangement, than there is in admitting death to follow on the structural derangement which is the result of pneumonia or peritonitis, or any other local structural disease; indeed, less; for, in the case before us, the affection of so important a vital function is general through the whole body, while, in a local disease, the morbid alteration is confined to a particular and, perhaps, a small space.

This case is, I think, a valuable one for our information: we found no structural lesion in a case of typhus fever so aggravated as this. Had we found any, we might still have had before us the "*vexata questio*," as to whether the local disease was the effect or the cause of the typhus fever. Here we found none. It therefore follows, unequivocally and undeniably, that typhus, or maculated fever, as we now have it, may not only exist, but be fatal, without local organic disease: in other words, it proves incontestibly the truth of our analysis, that our present typhus, or maculated fever, is a disease of function; that it has no structural pathology. There are some very interesting points connected with the nature and alteration of the urinary secretion in this case; but we must reserve these for another time.

The next case, Mary Cope, shews to us the exact nature of a disease of the mucous membrane of the intestines, which is sometimes one of the most fatal of the sequelæ of our fever, viz. follicular enteritis. This woman, ætat. 22, previously in the enjoyment of the best health, was admitted into the hospital on the 23d January, ill of maculated fever. There was nothing unusual in her case. She was marked convalescent, when, about fourteen days before her death, she complained of debility and diarrhoea; her tongue became brown and dry in the centre, but not furred; there was no tenderness of the abdomen, nor tympanites, but there was gargouillement over the cœcum; the stools were gruel-like, but were not observed to be mucous or bloody; there was no tenesmus; her pulse became very quick and weak. She became so debilitated on the 18th February as to require wine in considerable quantity. All means to stop the diarrhoea were of no avail, and she died on the 19th February.

On dissection, the peritoneal coat of the

abdominal viscera was sound; but the small intestine, the ileum, and the large intestine, presented (as you saw) the finest specimen of follicular enteritis in every stage. In some places the follicles were just protruding under the mucous membrane, filling with a cheesy purulent-looking matter; in others, this matter had amounted to such a quantity as to make each follicle protrude the mucous membrane, as if a pea were lying under it, while it felt hard and firm to the finger passed over it; while, in others, still farther advanced, one or more had gone on to ulceration, destroying the mucous membrane, and leaving only a grey slough of cellular tissue in the place of the follicle. The whole circle of the ileo-cœcal valve was one circular jagged ulcer: there were several small ulcers in the wider extremity of the appendix vermiformis. This appendix was swollen and congested externally; and, on slitting it up, it was found distended with a tenacious, lymph, purulent fluid. In the ilium, a little above the ileo-cœcal valve, were several circular or oval ulcers, apparently resulting from the destructive process having attacked patches of the glandulæ agminatæ; while around, and still higher up, were a few of the glandulæ solitariæ similarly diseased (but less in degree) to those in the colon. The greatest intensity of disease was at the ileo-cœcal valve. Both above and below this, the diseased appearance gradually grew less marked. The state of the appendix vermiformis, in this case, explains, I believe, what I lately observed in a case in private practice, in which the patient, a lady, was attacked, after partial recovery from fever, by follicular enteritis, as in the instance of Cope. During the progress of the case, the nurse-tender called my attention to what she believed to be a worm passed per anum, but, on examination, I found it to be a cord of lymph, about as thick as a quill, and from two to three inches long. It was quite solid. A similar substance was passed twice, or oftener, at intervals of two or three days. It is very probable that it was secreted in the appendix vermiformis and expelled, and a similar one again re-formed.

We have had several cases like Cope's, but all recovered except this one. Purcell, to whose case I have already alluded, was a well-marked instance of the same disease, coming on as a sequela of the fever; but no one who has seen Cope's or Purcell's case could for a moment confound it with typhus fever, or suppose the disease to be the pathology of maculated fever. Follicular enteritis is a local disease marked by as distinct symptoms, and as different from typhus fever, as any local disease to which the living system is subject. I have wished you to notice this case, as it bears upon a point we have previously discussed;—the supposed dependence of typhus on follicular disease of the intes-

tines.—I may make a few short observations now on the treatment of this follicular disease of the intestines. There is one remarkable feature about it, and that is, the difficulty of salivating a patient labouring under it; and this you may generally take in most diseases as an indication that mercury is a remedy in such diseases of doubtful, if not of injurious, action. The treatment which we adopted in Purcell's case, which was one of very great severity, and in the others, and which proved successful in all but Cope's case, was the continued administration of small doses of opium or Dover's powder, with counter-irritation repeated over the abdomen and right iliac region particularly, and the mildest farinaceous diet. In some cases, all ordinary means, whether by medicines given by the mouth, or in the form of enemata, are of no avail to check the diarrhoea, and the patient is fast sinking under it, when it will be rapidly and efficiently averted by the introduction into the rectum of a piece of sponge, about the size of a small hazel-nut, soaked in a solution of watery extract of opium. This will at first require to be renewed perhaps three or four times a day, but most often only once after the second day.

The third case to which I have to direct your attention is one of considerable importance, both as regards itself, as to its own peculiar nature, and also in relation to the insight which it gives us into a state of the respiratory and circulating functions of frequent occurrence in our maculated fever.

Mary Graham, a servant, æt. 24 years, was admitted into the hospital, Jan. 1841, in an apparently dying state: she was in a state of great relapse: her limbs and hands were livid; the surface generally cold, and she was almost pulseless: she was suffering very much from cough and dyspnoea. With great difficulty, from her being scarcely able to articulate, she informed us that, about three weeks before, she had been attacked with pain in the left side and oppression of breathing, for which she had been bled, with temporary relief; but soon after, the oppression continuing to increase, she was removed to the hospital. On a physical examination, the chest appeared to be well formed: over the anterior portion of the left clavicular region, and to the lateral and posterior inferior region of the same side, there was dulness to the greatest degree. Over a great portion of the left lung there was some crepitating rattle, but not fine, nor heard plainly over the whole extent of the dull region, nor was there bronchial respiration audible in the dull sounding parts. The infra clavicular region, after a few days, cleared on percussion, muco-crepitating rattle was heard, and the expectoration became muddy and sanguineous, but she never rallied; she was kept alive only by wine and carbonate of

ammonia, and an attempt was made at the same time to get her under the influence of mercury. She died on the 31st of January. —The right lung was very nearly sound. The heart was greatly enlarged as to the size of its cavities, but there was no valvular disease. The superior portion of the upper lobe of the left lung was not much congested, but from this, downwards, constituting probably three-fourths of the lung, and extending through the middle into the inferior portion of the lower lobe, the substance of the lung was in a singular form of pneumonia. The pleura was quite free from any trace of disease. The substance of the lung, viewed through it, was of a dark blue or purple colour. Its substance was tough, not in the slightest degree brittle, not crepitating, and, when thrown into water, it sunk like a hepatized lung. Very little fluid exuded from it when cut into. In the neighbourhood of the diseased portion, and in some places mixed with it, were a few small nodules of pulmonary apoplexy. Now, this was not the first stage of ordinary pneumonia, for it was not crepitating; it did not pour out fluid in an abundance when incised; it was not of the same colour. It was not hepatization, either red or grey, and still less did it bear any resemblance to the third stage of pneumonia. The case was, it will be remembered, also of three weeks' standing, when, in the ordinary form of the disease, it should have gone on to hepatization, or even farther. If we couple the history of the case with the general symptoms, we can, I think, arrive at a knowledge of its nature. The patient, brought from one of the most wretched abodes in the city, the neighbourhood of Mary's Lane, had herself bled for a commencing attack of pneumonia three weeks previously, and after this continued suffering under cold, damp, and probably want, in the severest season of the year. From this state she never rallied, neither did the distended capillaries of the affected lung. They remained in their congested state; there was no attempt at any of the ordinary changes, or the usual progress of pneumonia; they passed at once into a state of hyperæmia, and they remained in this state; in fact, they were in a state similar to that of the capillaries in an extremity that is affected with atonic congestion of its capillaries—a state that we see, on a small scale, in the capillaries of the skin, in depending portions of the body, in a case of typhus fever.

The appearance of the lung on post-mortem inspection coincided exactly with this; it was exactly such an appearance as would be produced by the capillaries passing into a state of congestion, and obliterating the air-cells; not as they are obliterated or filled up in ordinary pneumonia, but obliterating them by pressing in, from their distension, on the air-cells. The lung thus became solid,

and dull as muscle, dark in colour, and remained at the same time tough. The closing together of the air-cells was probably also facilitated by the great debility of the patient; her strength being exhausted, she dilated only the healthy portions of the lung, and thus the passive distension of the capillaries was in no way counteracted by the respiratory efforts drawing in air to the air-cells in the diseased portions of the lung. This weakness of respiratory effort will also account for the absence of bronchial respiration in the solid portion of lung. This case is very interesting, both on its own account, and from the light which I think it will throw on a similar state of the lung in fever.

ON STAMMERING,

AND THE METHODS PROPOSED FOR ITS
REMOVAL.

BY EDWIN LEE, Esq.

[Continued from p. 269.]

[For the Medical Gazette.]

SOME cases of paralysis, which serve to illustrate the destruction or interruption of the harmony existing between sets of muscles which are naturally associated in their actions, are thus referred to by a modern writer on the nervous system:—

“C’est ainsi que certaines paralysies partielles, en détruisant l’harmonie fonctionnelle, dénaturent les directions et l’équilibre; j’ai vu souvent dans de telles paralysies les médecins croire, d’après M. Flourens, à une lésion du cervelet, parcequ’ils remarquaient que l’équilibre étoit rompu; ou que les mouvemens n’étoient pas convenablement *balancés*; ou auroit pu croire aussi dans ces cas, en voyant l’individu manœuvrer autrement qu’il ne vouloit, que l’organe des directions étoit lésé; mais en observant mieux, il ne reste aucune incertitude à ce sujet; on s’aperçoit que les directions sont appréciées voulues et indiquées, mais que l’indication n’est nullement suivie, non par ineptie, mais par défaut d’ensemble des instrumens d’exécution, dont les uns sont plus ou moins faibles, ou manquant totalement à leur mission, et dont les autres manquant d’antagonisme ne sont pas pondérées; de là ces mouvemens brusques saccadés, ou trop énergiques sans mesure ni précision, qui lancent les paralytiques incomplets dans des directions qu’ils n’ont pas résolues, par la desharmonie d’action

le non-concours régulier, la non-coordination entre les instrumens d’exécution, dont les uns répondent mal ou même ne répondent pas aux directions imprévues, et dont les autres n’étant pas pondérés par leurs antagonistes, agissent trop vivement; il y a donc bien là non-coordination, mais non pas à la manière dont l’entend M. Flourens, qui place à tort la faculté coordinatrice dans le cervelet, au lieu de la direction des mouvemens en avant*.”

We may thus see, from what has preceded, that speech is dependent upon a variety of movements; and that for its formation, not only is harmony required between muscles completely under the control of the will (as are those of the lips and tongue), but also between such muscles and others which are but partially so: a free respiration, a regulated contraction of the muscles of expiration, of those of the larynx, palate, tongue, cheeks, and lips, in obedience to volitions conveyed to several of them simultaneously, or in rapid succession, being necessary for perfect articulation. It will therefore excite no surprise, on considering these circumstances, that speech should be so frequently affected in various diseases, and by various conditions of the nervous system, which destroy or interrupt, for a longer or shorter period, the harmonious action of these several parts. Persons labouring under concussion of the brain, apoplectic attacks, in the debility caused by exhaustion, or which supervenes in the latter stage of acute disease, can very frequently put out their tongue, and move their lips, but are unable to perform the associated actions required for speech, which inability is likewise frequently a premonitory symptom of paralysis or cerebral disease. After active exercise, as running, a person will frequently be speechless for some time, not merely from the want of air, but also from the temporary disorder or exhaustion of the nervous energies. The same circumstance may occur from mental emotions, as fear, surprise, agitation, and may even last for a long period; as the effect does not always cease with the action of the causes which produce it. When these causes act in an aggravated degree, they may induce loss of power over the more strictly

* Sarlandière, op. cit. Paris, 1840.

voluntary muscles, as those of the extremities, and the individual will be unable to move*. I have entered more fully into the consideration of these points in my work on Nervous Disorders, and will terminate these remarks by quoting from it a passage or two bearing upon the present inquiry. "Those parts which, though necessary for the performance of ordinary actions, are not so directly controlled by the will as some others, and hence require more exertion of this faculty for the healthy performance of their functions, appear to be most liable to be affected by its debility. Thus it has seemed to me that the muscles of the larynx are more subject to atony than the muscles of the extremities, when moral causes have been instrumental in producing the affection, and that the inferior extremities are more frequently affected than the superior."

"Loss of voice, depending on a state of atony or paralysis of the small muscles of the larynx, from suspension of their nervous power, is of frequent occurrence. It may be total or partial, in which latter case the patient can make himself understood in a low whisper. The lips and tongue can be freely moved in this variety of aphonia. It may supervene upon mental emotions, convulsive or other nervous affections, irritation of the surface, or of viscera; or it may come on without any obvious cause. A case is mentioned, in the *Dictionnaire de Médecine*, of a lady who was deprived of her voice during several years from excessive joy succeeding a state of great anxiety, which resisted all the means employed for its relief, and disappeared, when least expected, after strong emotion. In another case, the patient, during fourteen years, could only speak, every day, between the hours of twelve and two or three. Some years ago I saw a young man who suddenly lost his voice completely, for which no cause could be assigned. I stated that, although he might not derive relief from medicine, that the voice was not unlikely to return at some future period. This has since

occurred, after an interval of twelve years from the attack. M. Ollivier mentions a case of intermitting aphonia which, he says, 'existed more than thirty years, and was only cured for the time by abstraction of blood.' He adds, 'what is curious, however, is, that the voice suddenly returned after one or two spoonfuls of blood had flowed.' In this case it can hardly be supposed that the quantity of blood lost could have had any influence in the restoration of the voice; and I am inclined to believe that it was in consequence of the impression on the patient's mind that the voice must necessarily return at the time of the bleeding. The long duration of the disorder may be ascribed to the habit of having it recur at stated periods. These cases also tend to corroborate the opinion of their nature which I had been led to entertain, and there is every reason to believe that, could the patients' minds have been diverted from the expectations of the attacks at the accustomed period, they would not have occurred."

I might adduce several other cases illustrative of the action of mental impressions in the production and removal of disorders of the voice and speech; but as such would be out of place on the present occasion, I will merely extract the following from another of my works, as serving to exhibit the powerful influence of the imagination over those disorders.

"*Aphonia of six weeks' duration cured in a few hours by starch pills given homœopathically.*—A girl, æt. 20, was admitted on the 14th of January, with complete loss of voice, which had existed since the middle of November. She had experienced a similar attack in the preceding year, but had recovered in fifteen days. The menstruation was regular. A few days rest, and the usual hospital regimen, producing no effect, she was consequently placed in the department where the homœopathic experiments were made, and was ordered two pills composed of starch, which she believed homœopathic remedies. The first to be taken in the presence of the physician, the other at the expiration of four hours. A few minutes after the first pill had been swallowed the following symptoms manifested themselves:—Anxiety, pain, and uneasiness in the region of the heart and thorax; perspiration, with heat and eruption on the

* The fascination of some birds by the aspect of certain serpents rendering them unable to escape, or even to keep their position upon the tree, is in consequence of volition being paralysed by fear. The same circumstance occurs, in a minor degree, when an individual becomes speechless from strong mental emotion.—*Treatise on some Nervous Disorders*, &c. 2d edition, 1838.

skin. The second pill appeared to aggravate these symptoms, with the addition of hiccough. She afterwards fell asleep, and on awaking she was astonished to find she could talk in a loud tone. The complaint did not recur, and she soon quitted the hospital*."

Similar cases to the above will not of course be confounded, by the discriminating practitioner, with the loss or alterations of the voice and speech arising from obvious local irritation reacting upon the spinal cord or nerves, as in the instances of traumatic tetanus, worms in the intestines, &c.

On Stammering.

This affection has been confounded with the hesitation, difficulty of expression, or repetition of words—*balbuties*, which so commonly occurs in children in their first attempts to talk, and which generally ceases as they grow up; though, in a few instances, stammering supervenes upon it, which leads parents, and those who have the charge of them, sometimes to say, that the stammer has existed from the time they began to speak, which is not the case. A degree of hesitation, difficulty, or stuttering, may arise from other causes—as bashfulness, stupidity, confusion of ideas, apprehension, surprise, &c. which may occasion considerable embarrassment in the articulation of words; and, as a foreign author has observed, "it frequently happens even in persons highly endowed, and who usually express themselves with facility, to be wanting, as it were, to themselves, and to stammer out with difficulty the most trifling excuse, the most simple compliment, or the most ordinary answer to a question, which requires on their part some little presence of mind†." The imperfect articulation in certain states of cerebral congestion, inebriety, or weakness from disease, must also be distinguished from stammering.

Stammering, then, as may be inferred from what has preceded, may be considered as a spasmodic affection in the great majority of cases; not, however, of that kind which is caused by irritation of the surface, or of internal parts, or by lesions of the nerves or

spinal cord, but of a cerebral origin, and depending upon a temporary disorder or a faulty action of the faculty of volition, as far as certain muscles employed in respiration and articulation are concerned, which induces a want of consent or harmony of action between these muscles and others, which are more directly under the influence of this faculty*. In the peculiarities which it presents it has great analogy with other nervous affections, especially chorea. It seldom occurs before the age at which children possess consciousness, and are able to pronounce words with facility, viz. about four or five years. Like chorea, it may be acquired by imitation; is almost always increased by agitation, or when it is remarked by others; and affects, for the most part, individuals who are endowed with a high degree of susceptibility. It may be frequently stopped by measured and regulated movements, as speaking slowly after a full inspiration, reciting or singing, in the same way as the irregular muscular action in chorea may often be suspended for a time by the patient's playing the piano, skipping, or performing other actions which require attention, and a regulated exertion of volition. Like other nervous affections, stammering is more common, and usually increases in intensity, about the period of puberty, and the succeeding years, when the mental sensibilities are so strongly called into play, but it diminishes towards the decline of life, and ceases altogether when the nervous sensibility is blunted by the approach of old age. When the stammerer is alone, or with an intimate friend or relation, he can usually speak very well, or at all events much better than when he is in company, or in the presence of strangers. This fact is not invalidated, but is rather corroborated, by the circumstances that some persons lose their stammer after a good dinner and wine, though in company; and that others, having begun a speech on a subject which interests them, in a public assembly, have been able to go on speaking for a considerable time without any impediment; as in point of fact they are then in much the same

* Animal Magnetism and Homœopathy, with Notes illustrative of the Influence of the Mind on the Body. 2d Edition, 1838.

† Dictionnaire de Médecine, art. Balbutiement.

* Stammerers, when not attempting to speak, can, like other people, move their lips and tongue freely in all directions, except in those cases where the tongue is larger, or more bound down, than natural.

condition (with respect to the stammer) as when alone, having confidence in their powers, or forgetting their infirmity for the moment. It has been stated by a French author, that stammerers can generally speak very well when disguised by wearing a mask; and M. Itard mentions the case of a boy, eleven years old, "who was a great stammerer whenever he spoke in the presence of persons who looked at him; but he did not stammer when he knew that he could not be seen, and could speak to people very well when in the dark. The attempt had been made to cure him when he was younger by bandaging his eyes*." These cases are obviously explicable upon the same principle. Most stammerers, again, are made much worse when put in a passion, though some, by being put in a passion, become extremely voluble, from their attention being altogether diverted to other matters. Stammering also resembles other nervous affections in presenting frequent intermissions at regular or irregular periods; some stammerers are affected by the state of the atmosphere, being frequently worse in wet than in dry, in warm than in cold weather. A few find their stammer better or worse according as the moon is in its first quarter or at the full. I have seen two persons in which this was the case. Some, again, lose their stammer for a few days, or even for a much longer period, after which it recurs without their being able to ascribe the recurrence to any particular cause; and as a further corroboration (if any be needed) that stammering is especially a nervous complaint, and of the view which I have expressed respecting its nature, I may allude to the fact, that the great majority of cases may be cured by systems of education which have for their object the regulation of respiration, and of the muscular powers concerned in speech.

Several persons, however, have attributed this complaint to various physical alterations of the tongue, fauces, tonsils, &c.; and, indeed, in a large proportion of stammerers some anormal or peculiar conditions may be perceived on examination of these parts; yet none of them are sufficiently constant to authorize the supposition, that the disorder is occasioned by them; though, no doubt,

when existing, they tend to aggravate it. In many individuals the tongue is thicker, its muscles being more developed than natural; in some it is more bound down to the floor of the mouth, either by the development of the genio-glossi muscles, or by the frenum being inserted too near the apex. In others, one side of the tongue (the right) is somewhat larger than the other, and the organ is drawn to one side on being protruded from the mouth. In several, again, the tonsils are swollen, and the arches of the soft palate are lower than natural; though, from what I have seen, I cannot concur in the opinion expressed by Mr. Yearsley, in his pamphlet, that, "in the great majority of stammerers, the tonsils and uvula are in a diseased state." On the contrary, I should say that, in many of the individuals who experience great difficulty in speaking, nothing anormal is perceptible in the appearance of the parts, and that the physical alteration most frequently met with is the increased size and more energetic contraction of the muscles of the tongue and beneath it, which sometimes prevent its being freely protruded from the mouth, or its tip from being turned over the lip towards the nose, notwithstanding the person's efforts, as would be the case with an individual whose tongue was swollen from the exhibition of mercury, or any other cause. This alteration I consider to be a consequence of the disordered nervous action, which, by inducing irregular and spasmodic contraction, causes a greater degree of development of the parts, as would be the case with any other muscular structure which is unduly exercised. We therefore see that there is more difficulty and spasmodic action induced by the attempts to articulate words which begin with some of the consonants than with the vowels, the former requiring more the concurrence of voluntary muscles than do the latter, the pronunciation of which is of a passive nature. We also see, in the majority of stammerers, that there is more difficulty in beginning a sentence or speech than in its continuation, as a more direct effort of volition is required to commence a muscular action than for its repetition; and many individuals can pronounce a difficult word with much greater ease if they are desired to repeat it after another person.

* Dictionnaire de Médecine, art. Bégaiement.

These physical alterations may, then, be considered as an accidental coincidence, or as an effect of the disorder. One or other of them are constantly met with in tolerably healthy persons, and others who do not stammer; and it must be obvious that if they were the cause, the effect must be permanent, and stammering would always be present, instead of presenting, as it does, such remarkable variations, which are occasioned by the state of the person's mind at different times, and by other circumstances to which I have alluded. The same may be said of the opinion which would refer the complaint to the the incident or reflex action on the spinal cord, to which of late years it has been the fashion to ascribe many diseases the origin of which is referrible to a higher source*.

I have, however, seen several individuals (though the number is small when compared with the others) in whom the physical alteration in the size of the tongue, and the energy of the contraction of the genio-glossi muscles, appeared to be the cause of the infirmity, from the manner in which the tongue is bound down, and the resistance which is afforded on attempting to introduce the fingers beneath it. This class of stammerers differs from the other, inasmuch as they do not stammer in general, but invariably upon the same words beginning with a consonant, as C, B, P, K, or T, or any combination of syllables resembling them; they are sometimes a minute or more before the difficult words can be articulated, after which they can go on pretty fluently, and they experience nearly as much

difficulty if the word be in the middle of a phrase as if it be at the commencement. They are not, like the generality of stammerers, affected by being in society, as they experience an equal difficulty when alone; neither are they influenced by change of weather, &c. The first individual with this kind of stammer, whom I noticed, was a man who presented himself at M. Amussat's, and who could not pronounce the word *courroyeur*, and two or three others which resemble it, but in other respects could articulate tolerably well. Immediately after the section of the genio-glossi muscles he could speak this word and the others without any difficulty; and when I saw him for the last time, three weeks after the operation, he had no further impediment in his speech. I have little doubt that several of those, whom a long treatment and education have failed to cure, would be found to belong to this class*. There is, again, another class of individuals in whom the nervous or general stammer (which is sometimes upon some words, and sometimes upon others, and to which the preceding remarks principally apply) is combined with the difficulty in articulating particular words; these persons, though not free from the stammer when alone, can yet speak much better, and are much less liable to gesticulations of the head, neck, or limbs, and to choreal or spasmodic action of the muscles of the face or lips, than when in company with strangers, or otherwise excited. In several of those whom I have seen, there has been nothing remarkable in the appearance of the tongue or mouth, though the majority of them cannot turn the tip of the tongue upwards over the lip, and the muscles of the organ contract forcibly on the introduction of the fingers beneath it. I am inclined to think that in these persons the difficulty in the articulation of particular words

* "The instant production of several nervous affections from moral impressions, the variability of the symptoms, their transient nature and periodicity in many cases, are incompatible with the existence of organic lesion as their cause; and, although there may be some unequal distribution in the capillary system of the brain or spinal cord in many instances, yet this is probably an effect. The immediate cause of the symptoms most likely consists in some modification of the nervous action, with which we are unacquainted."—*Treatise on some Nervous Disorders*, &c. p. 22.

"The frequent variation and the cessation and aggravation of the symptoms at regular or irregular periods, are features peculiar to disorder of the cerebral functions, serving, in many instances, to distinguish it from other diseases. These peculiarities may be accounted for by the constant succession of impressions to which the brain is exposed. The disorders of that organ do not consequently follow the definite course pursued by affections of other parts."—*Ibid.* p. 26.

* A well-marked case of this permanent or physical stammer occurred in a man who was a patient in the hospital Beaujon for a surgical disease. The tongue was bound down to the floor of the mouth by an extremely short frenum, which even turned its tip downwards. As soon as the membrane was divided, the patient, who previously could scarcely speak a few words in succession, spoke distinctly and without difficulty; but, as he did not take proper precautions against the part reuniting, he daily lost some of the advantages of the operation.—*Dict. de Méd. et Chir. pratiques*, art. Langue.

is superinduced upon the nervous stammer; and that, when in an aggravated form and of long duration, stammering would always be found to be connected with increased muscularity of the tongue.

[To be continued]

CASES OF PTOSIS.—OPERATION.

To the Editor of the Medical Gazette.

SIR,

As the subject of ptosis appears just now to attract a little attention, I take the liberty of furnishing the two following cases from my note book:—

Samuel Jepson, æt. 30, applied on Nov. 28th, 1840, with ptosis of the left eye. Four years ago, after unusual exposure to cold, on waking from sleep, he found that he had lost the power of elevating the upper lid of the left eye. At the present time, the space between the lids is in extent about a fourth of that on the opposite side; there is no voluntary power of raising the upper lid, which feels soft and flaccid, and is not hypertrophied. The irides act equally, vision is perfect, and there is no palsy of any of the muscles of the globe.

A large elliptical fold, consisting of integument and a portion of the orbicular muscle, was carefully dissected away with the scalpel, the upper incision at its centre being close to the eyebrow. Four sutures were employed.

Dec. 10th.—A longitudinal raised seam marks the site of the portion removed.

30th.—There is now nothing disagreeable in the appearance of the eye, the most striking peculiarity arising from the absence of the natural fold beneath the eyebrow. The patient can close the eye perfectly; but, when winking, the upper lid is moved less, and the lower lid is elevated higher, than is the case with the right eye.

March 3d, 1841.—Sarah M'Carthy, æt. 28, has ptosis of both eyes, which, from her own account, is congenital. She states that the lids could never be more separated by a voluntary effort than at present. The inter-palpebral aperture is about a fourth of the usual extent. To enable her to view surrounding objects, the head is thrown back,

and both corneæ so depressed as to produce, when the upper lids are raised by the fingers, the appearance of downward squint. Vision is perfect; irides active; and all the muscles of the eyeball can be thrown into action, although, from the constant accommodation of the corneæ to the aperture between the lids, the inferior recti seem to have acquired predominance. The appearance of the face is peculiar, from the total absence of palpebral folds in the upper lids, and the very marked corrugations of the forehead, and raising of the inner half of each eyebrow, which have resulted from the constant but vain attempt to open the eyes by the energetic action of the occipito-frontalis and corrugatores superciliarum. There is no hypertrophy of the lids.

A portion of the substance of the upper lid of the left eye, half an inch in width, and an inch and half in length, was included between two elliptical incisions, and dissected off from the subjacent conjunctiva. The middle of the upper incision, and its inner extremity, were close beneath the eyebrow. By means of four sutures and strips of adhesive silk, the cut edges were retained in perfect apposition.

March 10th.—As the wound had perfectly healed, and the patient no longer required the eye to be covered, a similar operation was performed on the right eye.

April 2nd.—The existence of a curvilinear seam, and the want of the natural fold in each upper lid, are all that would be noticed as peculiar. At the cornea, the vertical space between the palpebræ is about half an inch, and can be slightly increased at will. In this, as in the preceding case, during closure of the eyes, the lower lids are more raised to meet the upper than before the operation, or than is natural in other persons; and, instead of the lids quite meeting in the common act of winking, the cornea is revolved to a noticeable extent under the upper lid, and a portion of the white sclerotica remains visible*. The undue action of

* In his notes to Paley's Theology, Sir C. Bell ascribes the depression of the lower eyelid to the action of the levator palpebræ superioris, which, by slightly projecting the eyeball, causes it to push downwards the inferior palpebra. Independent of other arguments against this supposition, the greater degree of elevation, and con-

the inferior recti is almost lost, the corneæ being as nearly as possible in the centres. The throwing back of the head, previously necessary, is no longer marked.

In the MEDICAL GAZETTE of March 25th, Mr. T. B. Curling published some interesting cases, including one of ptosis from palsy of the third nerve, on which he operated on the same principle which guided me in the above cases, in January 1841.

From the original paper of Mr. Hunt, and Mackenzie's remarks, as well as from some conversation which I had with the former gentleman previously to my first case, it was my impression that the new method described by him (viz. that of substituting the action of the occipito-frontalis for that of the levator palpebræ) as succeeding in traumatic ptosis, was the best operation in every case of ptosis, from whatever cause, in which an operation was advisable at all. I was not aware at the time, that its performance had hitherto been restricted to the cases in which it had been first tried.

I am, sir,
Your obedient servant,
C. RADCLYFFE HALL.

Manchester, April 17th, 1841.

PLURALITY OF CHILDREN—THREE GIRLS AT A BIRTH.

(For the London Medical Gazette.)

CAROLINE CHURCH, æt. 35 years, the mother of eight children, came to London, from Hayes, to the burial of her brother-in-law, March 26th. On the Sunday afternoon, March 28th, she was taken unexpectedly in labour, when a midwife was sent for in great haste. She was delivered of a girl at half-past six o'clock, and of a second girl at a quarter after seven o'clock, without any apparent second rupture of membranes. As the placenta did not follow readily, Mrs. Moss, the midwife, passed up her hand to assist in its delivery. She felt another set of membranes whole, which she ruptured with some difficulty, as they were extremely tough, and deli-

vered a third girl, footling, at a quarter after nine o'clock, the children all living. The woman was extremely low and unconscious. A few minutes before eleven o'clock the placenta was withdrawn.

Monday, 29th.—At the request of the midwife I visited her.

The mother is a stout robust woman; her legs enormously swollen; abdomen large and tender.

The children moderate sized, healthy looking; the third, or last-born, the smallest.

The first presented the head.

The second, arm and head.

The third was delivered footling.

The placenta (which is in possession of Dr. Robert Lee) was in one mass, the uterine membranes being common to the whole. The first two children were apparently contained in the same bag or set of foetal membranes, that is, there was no apparent membranous septum between the two. The third had a very distinct, firm, membranous septum, with distinct chorion and amnion. One, the third or single child's placental umbilical cord was injected with wax, by which only a portion of the placental mass was filled, and without any communication with the remaining and largest portion: subsequently air was thrown in by a second umbilical cord, and a portion of the remaining mass only was inflated. Air was then thrown into the third umbilical cord with a similar result. Thus it would appear that, although the placenta was in one mass, there was no communication between the placental vessels of the three children; notwithstanding that two of them were contained in one bag, still each had its distinct portion of placenta.

Mrs. Church did very well for a fortnight, suckling the three children alternately, when, on Sunday, April 11, she had a severe attack of intestinal fever, from which, after a considerable struggle, she has perfectly recovered, but without the capability of again nursing her three children. They are now in average health, at 29, Harrow Road.

HENRY DAVIES.

18, Saville Row,
May 4, 1841.

sequently of subsequent depression, of the lower eyelid in these cases where the levator palpebræ is so inefficient, appear objections.

ANALYSES AND NOTICES OF BOOKS.

“L'Auteur se tue à alonger ce que le lecteur se tue à abréger.”—D'ALEMBERT.

Ἑλληνικὴ Φαρμακοποιία κατὰ Βασιλικὴν Διαταγὴν καὶ κατ' Ἐγκρισιν τοῦ Β. Ἱατρικοῦ Συμβουλίου ἐκδοθεῖσα, συνταχθεῖσα δὲ παρὰ Ἰωαννοῦ Βουρου Ἱατροῦ, Καθηγητοῦ τῆς εἰδικῆς παθολ. καὶ θεραπευτικῆς Β. Ἱατροσυμβούλου, Μέλους τῆς Β. Βαναρικῆς Ἀκαδημίας τῶν ἐπιστημῶν κτλ. Ξανθερίου Λανδερερου Β. Φαρμακοποιού, Καθηγητοῦ τῆς χημείας, Μέλους τοῦ Β. Ἱατροσυμβουλίου, Ἰππότου τοῦ ἀργυροῦ σταυροῦ τοῦ Σωτήρος κτλ. Ἰωσηφ Σαρτορίου Αὐλικοῦ Φαρμακοποιού κτλ. Ἐν Ἀθήναις, ἐκ τῆς Βασιλικῆς Τυπογραφίας, 1837.

Pharmacopœia Græca, Jussu Regio, et Approbatione Collegii Medici Edita. Auctoribus JOANNE BOURO, Med. et Chir. Doct., Patholog. et Therap. special. Prof. p. o., Colleg. Med. Membro, Acad. Reg. Monac. Socio, etc.; XAVERIO LANDERER, Pharmac. Reg., Chemiæ Prof., Colleg. Med. Membro, Equite Crucis arg. Salvatoris, etc.; JOSEPHO SARTORI, Pharmacopœio Aulico. Athenis, ex Typographia Regia, 1837.

THERE lies before us a *Pharmacopœia*, published at Athens, by Royal authority, and with the approbation of the College of Medicine. The work is interesting in various points of view, and, certainly, in none more than in calling us back, after the lapse of ages, to that classical land which produced Hippocrates, and other sages in the healing art. Another interesting circumstance is, that, in giving the modern Greek names of the various medicines, it assists us in making out their ancient history; for, although Sibthorp effected this with respect to various plants, yet, for all the medicines more peculiarly so called, we know of no work, but the one before us, from which any considerable assistance can be derived.

From the account given in the *Προλογος*, it would appear that the present

undertaking was greatly wanted, owing to the confusion which had arisen in modern Greece from the number of medical men, who, having received their education at different European schools, adopted correspondingly different forms and modes of prescribing, giving rise, the editors inform us, to great confusion.

The Greek *Pharmacopœia* is divided into two parts: the first comprising those medicines which are supplied by the merchant, the second those made by the pharmacopolist; each of which has a description of the mode of its preparation. The whole work is also given in two languages, namely, Latin and modern Greek,—in this respect resembling the American *Pharmacopœia*, which is given in Latin and the vernacular tongue; while the plan of giving short descriptions of the substances used seems to be taken from that of Prussia, which it exactly resembles.

The names selected profess to be those best known, except with regard to chemicals, the names of which, we are told, are those most accordant with modern science. But, in neither respect, is the selection always happy: as we have *China fusca* and *regia* (from the Italian *Pharmacopœia*) for our *Cinchona Cordifolia* and *Lancifolia*; while the Sulphate of Quinine is designated *Chininum Sulphuricum*. Again, we find some of the old, and, in Europe, nearly exploded chemical designations adopted, such as *Ferrum muriaticum*, *Kali muriaticum oxygenatum*, &c.

Opium is represented as the product of two species of poppy, the *Papaver officinale* and the *P. somniferum*; but of these no more is said than that they are “*plantæ annuæ in Oriente et Ægypto cultæ*.” Do they refer to the two kinds of poppy mentioned by Hippocrates, the black and the white, and which are described by Dr. Royle as varieties only?

The name *Calumba* has led to the mistaken notion (not, however, confined to the Greek *Pharmacopœia*) that the root in question is brought from Columbo, in Ceylon; although we are rather inconsistently told, immediately after, that it grows at Mozambique!

The *Hellebori nigri radix* has afforded the Athenians an opportunity of falling into error; and we may remark, that

they are not the only parties to whom this same plant has proved a stumbling block. It is well known that Sibthorp has satisfactorily proved the Ἑλλέβορος μέλας of Dioscorides to be the *Helleborus officinalis*. Now the London College seems to have imagined that this plant must therefore be the source of our black hellebore, although it is well known that our root is the *Helleborus niger*, and comes from Germany, not Greece. Accordingly, the authors of the Athenian Pharmacopœia, although they refer to the "officinalis," yet adopt the *Helleborus niger* of Switzerland and Germany as the medicinal plant; thus fully establishing the mistake of the London College.

Upon the whole, the work is one of interest, serving to shew the state of medical science in modern Greece; and, as such, we have thought it deserving of this short notice.

MEDICAL GAZETTE.

Friday, May 14, 1841.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso." CICERO.

THE MEDICAL SCHOOL EXAMINATIONS.

THE return of the time for the examinations in the several medical schools reminds us of the necessity of again urging that they should be made to serve those purposes in medical education for which they are so evidently well adapted. We do not deny that already they have done much good, and will continue to do it; that the energy shown by the students since their introduction has been greater than it was before, and has been shared by a much greater number; and that they have given rise, in all who have engaged in them, to an honourable feeling of emulation, which has led them to acquire a much wider and deeper knowledge of professional sci-

ences than otherwise they would have attained.

All this, and more, might be truly said of the benefits resulting from the school examinations, and yet they will not have accomplished nearly all the good of which they are capable till they are made compulsory, universal, and necessary as a step previous to admission to the examination for the diploma. For, at present, their advantages are usually limited to a comparatively small proportion of the students of each class. When we look over the lists of the successful candidates in this and in past years, we find that they include but few names, and that very evidently the greater number of the students of each school have not engaged in the competitions at all. We do not mean to imply by this, that the credit of those who have gained prizes is much less than it appears, or that they have not fairly beaten the whole of the classes of which they are severally members; on the contrary, the probability is, that in general they have been suspected throughout the season to be the best men, and that therefore only a few have ventured to compete with them. But still their honour would have been greater had they occupied the same places after a fair competition with the whole class; and the rest, if they had had the stimulus of necessity to make them compete, would not have been so far behind as now they are.

There are, we suspect, very few among the medical students who, if they were compelled to a competition, would not do their best, and strain every energy, to occupy a respectable place in the examinations. At present many think it better to be safe against having a low place than to take their chance of getting a high one: and they refrain altogether from the contest, content with an average reputation for industry and attainments; but if the

examinations were universal this would cease, and there could be no safety from a low and somewhat disreputable position, except by the steady pursuit of knowledge. It seems certain, therefore, that such a plan as we propose would have the immediate effect of increasing many-fold the amount of work done in the medical schools, and of raising the average standard of acquirement among the pupils.

But besides this (in itself sufficient to render the question most worthy of consideration), the school-examinations, if all the students were compelled to pass them, might be made to remedy a great number of the defects which now confessedly exist in the scheme of medical education. For what purpose, for example, is all the difficult and tedious system of registration adopted? registration of lectures entered to, and of lectures professedly attended, and of residence in London, and so forth. It is said, to insure that the student regularly and diligently pursues a proper course of studies. And all this would be very well if the system accomplished any fair proportion of what it pretends to; but it is notorious that all that the most scrupulous registration can determine is, that the student entered at a certain time upon the right to attend certain lectures, and that, at certain subsequent periods, he was residing in London; but whether he attended his lectures regularly or not is always a matter of doubt; and whether he learnt any thing from them—whether he slept through them, or listened attentively and studied them at home—is never enquired into, although it is obvious that this is the only question the answer to which would be really important. Now the whole system of registration might be safely dispensed with, if at fixed times during the course of their studies all the medical pupils were

made to pass examinations in the several subjects on which they had attended lectures; and if, when the results of such examinations were not satisfactory, they were stayed in their course and held back to learn the necessary elements of medicine before they proceeded (as they now often do, unprepared and unfitly) to the study of its higher branches.

The greatest and hitherto the most insuperable evil which has resisted the benefits of every scheme of medical education is, that some of the students, after spending the greatest part of their time for their own amusement, and learning therein absolutely nothing of their profession, have yet been able to pass the ordeal of their examinations by a few months' artificial and comparatively useless study. The system of *grinding* has undoubtedly attained in these times a degree of perfection, which, as far as we know, in no other studies than those of medicine, it ever even aspired to; and it now almost defies examinations to detect the superficiality and inutility of the knowledge which it gives for the day of trial, but which vanishes in the following month, and leaves the intellect as dull and empty as it was before the system was commenced. We do not say that the necessity of passing school-examinations would altogether put an end to this baneful system; but we are certain that it would be to it both a heavy blow and a great discouragement. For the school-examinations the students would seek the assistance, not of grinders, but of their legitimate teachers, who could best and most cheaply instruct them in their own lectures—the subjects on which exclusively they would be examined. At present the evil is, that a pupil, having misspent a large portion of his time, cannot go to his legitimate teacher to ask him to repair the loss

by extra and daily instruction; and, therefore, putting off to a continually later period the time of study, at the last hour, and only when it is absolutely necessary to do something, does he adopt an illegitimate and useless mode of study. With repeated (say annual) school-examinations, this must in great measure cease; or even if they did not altogether abolish such a system, they would still be greatly beneficial; for, at the least, they would compel the student to be capable of passing examinations several times instead of once in the course of his study, and would give him by so many times a better chance of learning something of his profession, even in spite of himself.

Another evil that would be remedied by this measure is that to which we lately alluded as rendering desirable a system of more intimate *personal* instruction of the pupils; namely, the difficulty which they find in the earlier part, and sometimes even through the whole course of their education, in determining how and to what they shall give their chief attention. We are convinced that on this the loss of a great amount of most valuable time depends; and that the defective state of knowledge in which many students at last find themselves, depends on their not having studied aright, either in mode or order, much more than on any incapacity to learn what ought to be definitely set before them. At present, a pupil coming to London is given to understand, in few words, that in three years' time he must attend so many lectures on so many subjects, and so many days hospital practice, both medical and surgical; and that at the end of that time he must pass two examinations, which will include a great number of subjects which have no very evident connection with each other, but which extend from Botany to Practical Surgery. Well! with an in-

definite idea that the things would not be given to him to do, if they were not possible to be done, he sets to work, and takes the subjects that the Worshipful Society of Apothecaries have declared shall be learned in the first seven months; namely, anatomy, chemistry, materia medica, and (as added by the College of Surgeons) surgery. The result of the endeavour to study all these is always bad; but what form of mischievousness it will assume, depends much on the particular disposition of the student. If he be a young man of humility, and distrustful of his powers, he soon finds himself utterly lost; instead of learning all, he can learn not one of his subjects: he has neither time nor talent for the study of one-half of what is put before him; and believing (what is very true) that it would require the intellect of a well-instructed old philosopher to do any thing creditable in four such subjects within the brief period of seven months, he either gives up the whole matter in despair, and takes to his amusements, or (if he be possessed of more than ordinary discernment) he suspects that the curriculum is only intended to make an appearance of learning—he drops half his subjects—and, giving all his attention to those he likes best, or thinks most important, he leaves the remainder till he wants a knowledge of them for his examinations. And this last is a very wise course of proceeding, and the one which we very generally recommend students to adopt. A much more mischievous one is that pursued by most of the students, who, instead of this humility, possess a good opinion of their own powers of acquiring knowledge. Such an one is sure to find reason for believing that he makes progress, and satisfactory progress too, in all his studies, and he passes without ruffling himself through all the intricacies and difficulties of the curricu-

lum; and if his progress were all real and sure, he would indeed be a very perfect example of what medical education-systems seem to imagine medical students to be: but there is too often a melancholy end to all this. The examination at last comes, and with it, for the first time, dissatisfaction: the knowledge is found to be utterly superficial and useless, and the condition of him who had studied every thing is inferior to that of him who had left half his subjects uncared for. Now, in this respect, for all classes of pupils, but for this last more especially, who constitute a very considerable number, the system of frequent examination would be most beneficial.

Against such a system we are not aware that any good reason could be urged. It may be said to be troublesome; but the time is gone by when the trouble could be made an excuse for not undertaking what would be beneficial.

There is indeed another objection, but it is only apparently one, against this system. It is that it is absurd to expect the average of students to learn even a little of the subjects set before them in each, and especially in the first, season. This is quite true—it is absurd: not one student in a hundred learns a moderate quantity of each of his subjects; but this is not because of examinations, nor would the evil be increased by them. It is the result of the absurd mode in which the curriculum of education is arranged, and to which we shall take an early opportunity of alluding.

VACCINATION AMENDMENT BILL.

In this bill it is proposed to enact—

1. That it shall be and be deemed to have been lawful for the Guardians of every Parish or Union in England by whom the contracts for vaccination may respectively be or have been made

under the provisions of the act 3 and 4 Victoria, c. 29, to defray the expenses incidental to the execution of the said act out of any rates or monies which may come or may have come into their hands respectively for the relief of the poor.

2. That the vaccination, or surgical or medical assistance incident to the vaccination, of any person resident in any Parish or Union, or of any of his family under the said act, shall not be considered to be parochial relief, alms, or charitable allowance to such persons; and that no such persons shall by reason of such vaccination or assistance be deprived of any right or privilege, or be subject to any disability or disqualification whatever.

MEDICAL REFORM.

To the Editor of the Medical Gazette.

SIR,

ONE of the greatest difficulties connected with the arrangement of medical reform, is the constituting an order of practitioners for the service of the middle classes of society. It was proved, before the parliamentary committee, that so far from it being possible to effect this object by lowering the qualifications of such practitioners, they are precisely the persons in whom professional skill, liberal education, and moral principle, are most indispensable; since the opportunities of abusing their trust are numerous, and those who avail themselves of their assistance have not the means of obtaining consultations. The rich have those means; and the "poor" are without pride, and procure, on charitable grounds, due professional aid*. The proverb "*medio tutissimus ibis*" is quite inverted, when we compare rank, with the opportunity of obtaining adequate medical assistance; for practitioners, qualified as above mentioned, will certainly not devote themselves to hard work, and low remuneration, without the opportunity of either acquiring character, or the satisfaction of having performed a charitable act.

* It is quite a mistake to suppose that the class termed "the poor," are really the most deserving of commiseration. Take, for instance, the case of a widow, formerly well to pass in the world, but now in limited circumstances, requiring good (the best) medical assistance for herself, or one of her family: or that of a half-pay officer, whose wife suffers from a preternatural accouchement.

There is certainly no ground, established either on reason, common sense, political economy, or Christian feeling, why the middle classes should be thus treated. They are our countrymen, fellow tax-payers, persons of political influence, and brother-Christians. Their health and lives are surely as valuable as the "liberties" of the West India negroes; and their preservation of as much importance to Great Britain as the emancipation of our sable dependents, procured at the cost of twenty millions of money. Why, then, should not the interest of such a sum, if necessary, be devoted for the former purpose?

To be brief, I propose that the United Kingdom shall be divided into districts, and placed under the medical superintendence of a staff composed of competent physicians, surgeons, and pharmaceutical chemists, so as to afford a degree of professional aid to the middle classes of society, and take them out of the hands of quacks, nostrum venders, ignoramuses, and dealers in medicine by quantity instead of quality. The staff ought of course to be constituted, as in the army, of various grades; the juniors having the most work and least pay, and seniors promoted, on merit to the more responsible stations. Each officer ought to receive a salary, on a scale perhaps like that in the army, and be strictly superintended in the performance of his duty.

It is certainly not an easy matter to provide against the abuse of measures calculated for the public good; still, by proper surveillance, under the guidance of clerical and other parochial officers, means might readily be adopted to prevent any but the really deserving obtaining relief, according to the system above mentioned. Every claimant ought to be required to contribute, according to his or her ascertained means, to the aggregate fund, which must, of course, be aided by a large parliamentary grant.

Some of your readers may, perhaps, object to the plan, on the grounds that it would not be consented to by the authorities. Where, however, the principle is in itself right—where the only objection is an increase of taxation, to be borne chiefly by those about to derive benefit from the measure, and where this party is possessed of great political influence—I do not anticipate much difficulty as to the plan being carried into execution, if properly urged.

I am, sir,

Your obedient servant,

A PROFESSOR IN THE SCHOOL OF
PHYSIC IN IRELAND.

THE SHOP AND APPRENTICESHIP SYSTEM.

To the Editor of the Medical Gazette.

SIR,

I SHOULD feel infinitely obliged by your admitting into your valuable pages a few observations upon a point of medical reform, which seems, in the majority of cases, to be overlooked, or only slightly glanced at. I allude to the conjunction of pharmacy with medical practice, and the truly ridiculous and degrading custom of regulating charges by the amount of medicines supplied. These are the two great and flagrant evils laboured under by general practitioners, and involve others of scarcely less importance. In most schemes of medical reform, the great demand is, a change in our political condition, and a greater amount of professional equality; that this would be a source of happiness to some few I readily admit, but believe that the great majority would be far more content with that change in their condition which should tend to elevate them in the public eye, and generate within their bosoms that pride in their profession and brethren which is one of the most certain sources of self-esteem.

But to a detail of the evils. In all other departments of civilized life, the division of labour has kept pace with increasing civilization and refinement, but it was reserved for the nineteenth century and enlightened England to continue, or rather introduce the system, repudiated by the rest of Europe, of imposing upon the same individual the highly intellectual task of discriminating and treating disease on the one hand, and on the other, of watching boiling pots, and rubbing up pills, or exercising their taste in the selection of variously coloured papers with which to cap their bottles.

The worst consequences of this unhappy conjunction are the shop system, the apprenticeship system, and the useless occupation of time.

The shop system is pregnant with evil to the individual, to the profession, and the public.

The first effect upon the mind of the individual engaged in retail, is "decrease of self-estimation." The man who practises his profession in this manner will soon be convinced, in spite of any feelings with which he may have commenced life, that he is but a tradesman, and regarded in such light by the petty shopkeepers around.

For some time he may with proper spirit struggle against this humiliating position, and endeavour to maintain an air and manner of superiority; but what is the con-

sequence? the good people, finding that he shuns familiarity, or declines the gossip in which they are disposed to indulge when patronizing his house for a pennyworth of salts, at once proclaim him as a proud man, and infest their richer neighbours with the same notion, who, naturally enough, shun intercourse with one who sets himself above his station. Such, unfortunately, is generally our pecuniary condition, that, finding this independent and strictly correct conduct causes the loss of practice, we are compelled to bow the knee, and fall into the habits and customs which are most agreeable to those around; use shortly begets a second nature, and in due time the educated and scientific surgeon may be seen standing at his shop door, with hands behind his apron, cracking jokes with the milkmaid, or hand in glove with some neighbouring Boniface. What is the deplorable result? In a short time he sinks down to the mere vegetative being—eats, drinks, and is happy. But this degradation is not confined to himself; in his fall, he drags down with him the whole profession. It is this unfortunate abuse which makes us rank beneath every other profession, and induces young men of spirit and gentlemanly feeling to enter the army, navy, and East India Company's service, where, although the remuneration is limited, they can possess the feeling of pride in their occupation and associates.

These, sir, are plain and painful truths. The Queen's commissioner, the clerical and legal gowns, are universal passports to society. But, for ourselves, our profession is rather detrimental; we only obtain admission through tolerance or individual respect. This is not visionary. I would quote a recent circumstance of medical exclusion at Naples from your own pages, and a similar affront which was put some time ago (unintentionally it was said) upon the medical officers of one of the regiments of Foot Guards within my own knowledge. An English physician applied for introduction at one of the small German courts, and received for reply, that he would be willingly permitted to show himself, on condition that he would drop the title "Doctor," and merely appear as a private gentleman. I shall be told in reply, "This is puerile, and unworthy of thought by philosophic minds;" but such are few in number, and those even that exist, are rarely above courting the good opinion and respect of their fellow-creatures. An important question, however, occurs here: what is to become, in case of any peremptory legislative enactment, of those who now only continue to attain a scanty sustenance by means of retail? This is, indeed, of consequence, and brings to our view the evils of an over-stocked medical market.

This apparent comparative facility of gaining an existence is a great and fatal encouragement to parents, when debating upon the choice of a profession, to select our own. Oh! say they, why should we make our son a tradesman? when, by a very little additional outlay in premium, we can make a gentleman of him, being comparatively ignorant of the subsequent demands upon their purse. He is thus cast upon the world without being able to wait, in the regular course of practice, for an income; in self-defence takes to retail; and thus from day to day is the evil perpetrated. It is an invidious proposal that intellect should be kept down by pecuniary barriers; but surely that is a less evil than that a whole profession should be lowered in the social scale, and driven to all kinds of internal meanness and dissension by their rivalries. What is the cause of those pitiful endeavours to supplant one another, so often displayed? Superfluity. Why is it that men not unfrequently sacrifice honour, conscience, and their patient's welfare, to their own aggrandizement? Why, from our superfluity. A man, whom it was once my misfortune to know, used to inculcate such maxims as these: "the times are scrambling ones, if you ever get a good patient, strike it in [meaning the medicine] as much as you can, and make the most of him." There, sir, was a specimen of a medical philosopher.

Again, our mutual ill-will, and malevolent and depreciating observations, are proverbial. And is not the public injured by such proceedings? Undoubtedly; the man, whose mind is all day dwelling upon drugs, is surely not in a condition to be an improving practitioner; his time is too much occupied by considerations of profit and loss, the price-current of drugs, the appearance of his shop window, and the brightness of his bottles, to give much thought to the science he professes. Anxious as I am, to see every member of our profession possess a voice in the election, and the chance of a seat in the councils of its governing bodies, still I would by no means consent to such a change until its lowest members, the present retailers, had given up their trade, and could appear with proper dignity in such situations. Until such be the case, I am well content with those who stand forward as our leaders; men, whose birth, education, and connection, enable them to move in the first ranks of society. The class of physicians, as now constituted, is invaluable in giving tone and respectability to the profession. The enemies of the College say, "it is too exclusive; it denies its highest honours to almost all but University graduates." Well, be it so; I maintain that any one complying with its regulations, by giving himself a first-rate

education, more fully entitles himself to an easy access to its high places, than any one who devotes himself exclusively to medical pursuits; indeed, I have pretty universally found a man's professional information bear a direct ratio to his general acquirements.

It is, indeed, laughable to hear the occasional self-sufficient observation of some medical *parvenus* in asserting, that the general practitioner is a much better informed man than the physician, because he divides his attention amongst a greater variety of subjects, whilst the former concentrates his upon one. Another evil, the very name of which smells of the trading source whence it is derived, is the system of the apprenticeship; and this, too, is the unhappy consequence of blending pharmacy with practice. In the most desirable cases, the unfortunate youth spends two or three years more than necessity requires in pure dispensing, which can for all practical purposes, be acquired as well in twelve months as twelve years. Instead of being truly a pupil, and benefiting by his master's knowledge, he is too often taken only to save the expense of an assistant, and made a domestic drudge. Is it wonderful, that such a course of discipline should stifle all just and gentlemanly feelings?

The last circumstance to which I shall direct your attention, is the custom of regulating charges by the amount of medicine supplied. Of all professional absurdities it is the worst. It leads in every case to what has been happily termed the "drenching system," giving the unprincipled practitioner an opportunity of increasing his gains at the expense of the patient's health—certainly of his comfort, and depriving the honourable one of just remuneration. It is, besides, a fiction of so truly ludicrous a character to suppose or impress upon the public mind the fact, that we claim recompense only for drugs given, and not for the exertion of mind or manual skill, and which has led to the vulgar aphorism that we get eleven pence halfpenny out of a shilling. It leads also to another intolerable annoyance; I mean occasional requests that something should be deducted from the sum total of a bill, as they think the charges much too high, and wonder how they could have wanted so many draughts per day; and this request is, I regret to say, not unfrequently complied with, in order that the practitioner may retain his patients, thus tacitly acknowledging overcharge. For all these great and constantly recurring annoyances, I believe the separation of pharmacy from medicine would afford a certain remedy; and I cannot imagine a single valid argument in favour of its continuance. It would at once abolish shops and apprenticeship, and compel us all to regulate our at-

tendance by a certain known scale of fees, the amount of which might be determined by the age and professional standing of the practitioner.

The first five years of practice might entitle him to half a crown a visit; the second five, to five shillings; and all subsequent times to seven. These should be the maximum; but, doubtless, in many cases, charitable feelings would induce him to diminish the amount. Although I would most willingly see these paid at the time, yet I believe such would be an extreme inconvenience to the middle classes, when salaries are received at certain periods of the year; yet, I would have it distinctly understood, that the account should be sent in upon the termination of the illness, not waiting until Christmas, and then following in the wake of the bootmaker or tailor. But it is not sufficient to wait until we ourselves or the public adopt these measures spontaneously; they must be made the subject of legislative interference, otherwise they will never take place, for any one attempting innovation is regarded with a very jealous eye. These are the reforms which will be most efficient in brightening the prospects of the profession, and rendering the labour one of greater sweetness. I have not dwelt here upon the imprudent pretensions of the chemist; for I feel satisfied that, did we give up dispensing, he would readily abandon so dangerous an occupation as playing with the edge-tools of medical practice. With many apologies for my prolixity,

I am sir,

Yours respectfully,

J. R. B.

ON CONGENITAL OPACITY OF THE CORNEA.

To the Editor of the Medical Gazette.

SIR,

IN your journal for April 30, Mr. John Christie, of Glasgow, has obliged your readers with an interesting communication "on congenital opacity of the cornea." In the course of that communication the following statement occurs:—"Mr. Middlemore, of Birmingham, it would appear, doubts the occurrence of congenital opacity of the cornea; but the cases now on record, although few in number, leave no question as to the possibility of such an event; and the veracity of Messrs. Crompton, Walker, and Barton, cannot be called in question, however it may run counter to the preconceived notions of Mr. Middlemore." As my opinions, on this occasion, are greatly misrepresented, I can only suppose that the author, not having

seen my treatise, has relied, for his information, on a quotation from it, given by Mr. Crompton, of Manchester, in a former number of the Gazette, which is quite too partial and incomplete to convey a correct notion of my views. In truth, I have never doubted the existence of congenital opacity of the cornea (in fact, I have seen it in several instances), either as a consequence of hydrophthalmia, of imperfect development of the cornea, or as a result of inflammatory intra-uterine disease. I have never attempted to throw any doubt on the "veracity" of Messrs. Barton, Walker, and Crompton, for I have not replied, in any public form, to their statements; much less offensively, or with a view to impeach their veracity. I had no "preconceived notions" on the subject, but have merely arrived at a conclusion after a candid and attentive investigation of evidence.

Some years ago, when engaged in arranging for publication my treatise on Diseases of the Eye, I met with a statement in one of the old medical journals which much surprised me, and conceiving that the conclusions of the writer (which went to the establishment of a somewhat novel and highly important fact in ophthalmic pathology) were not borne out by the cases on which such conclusions were founded, I ventured to give an opinion to that effect. I have yet to learn that, in adopting this proceeding, I have deviated from the ordinary mode—the perfectly justifiable method—of examining the opinions of preceding and contemporary writers. In order that my professional brethren may judge how far I have merited the bland censures pronounced on me by Messrs. Crompton and Christie, I will connect with this brief communication the statement of Mr. Farar, and also my published opinions thereon, which have given so much offence to those gallant protectors of his fame. "About nine years since I was desired to see a child who was about a month old, and apparently blind, having the cornea of both eyes opaque, so that not the least of the iris was to be seen. My opinion was, that nothing could be done in this case, and that the child would for ever be blind. About three years after another child was born of the same parents, with exactly the same appearance. The manner in which the cornea acquired the transparency was, in these cases, remarkably curious. The external edge growing thin, soon after became clear and transparent; and after this the whole surface of the cornea brightened up, the centre being the last spot that recovered its transparency*." In reference to these cases

and the opinions and conclusions connected with them, I have said, "The occurrence of extensive opacity of the cornea, as an effect of purulent ophthalmia of newly-born infants, appears to have led Mr. Farar into a very curious blunder. In a paper read before the Society for promoting Medical Knowledge, he has very singularly pointed out as a congenital disease what I conceive to have been a mere effect of inflammation*." Because, sir, I have thus noticed the opinions (after having fairly quoted them) of Mr. Farar, on the subject of congenital opacity of the cornea, Mr. Christie has affirmed that I entertained preconceived notions on the subject of that disease, and that I have absolutely denied its existence! If the gentlemen who have taken the trouble to associate my name with opinions I never entertained will be so good as to wait a short time, they will find, in the new edition of my book, now in course of preparation, that I have ventured to speak more positively and definitely than I had hitherto done, in regard to the cases related by Mr. Farar, and their competence to sustain the conclusions since founded on them, although I have not neglected to give every attention to the cases and arguments adduced in their support by those whose knowledge has been assisted and guided by the published experience of many who have preceded them in the course of inquiry.

I am, sir,

Your obedient servant,

R. MIDDLEMORE,
Surgeon to the Birmingham Eye Infirmary.

COMPOUND CATHETER.

To the Editor of the Medical Gazette.

SIR,

I RECOLLECT Dr. Andrew Buchanan, of Glasgow, showing me, along with some other students, in 1832, his compound catheter, which I have seen him employ subsequently with considerable success.

When I read, in the part of your journal for last January, Mr. Foulkes' account of his instrument, respect for an old teacher caused me to think of claiming priority of invention for Dr. B. His letter of the 15th of February, however, superseded the necessity of any notice of the matter on my part. But on reading to-day Mr. Foulkes' second communication, which appears in this monthly part of the GAZETTE, I can neither refrain from publicly adding, along with "Suum

old, possessing an opaque cornea, would "for ever be blind." Neither would he have stated that the fact of an opacity of the cornea becoming thin at its circumference, before it disappeared at its centre, was "remarkably curious."

* See my Treatise on Diseases of the Eye, vol. i. p. 156.

* See Medical Communications, vol. ii. p. 463. The readers of the MED. GAZETTE will not fail to remark that a surgeon, possessing much experience in ophthalmic diseases, would scarcely have felt himself at liberty to say that a child, a month

Cuique," my testimony to the originality of Dr. B.'s invention, nor from expressing my sincere regret at the manner in which Mr. John Croxton Foulkes has penned his last letter. It is a very great pity that such a spirit as he evinces should have any existence among the members of our liberal profession. The injury which such an animus does to the advancement of medicine is incalculable, not to speak of its otherwise censurable character.

If Mr. J. C. Foulkes really did not get a notion of the instrument from the itinerant "catheter-maker," I am sure the readers of the *MEDICAL GAZETTE* will agree with me in thinking that he would be vastly benefited from *imitating* the very gentlemanly and courteous style of Dr. Buchanan. Besides ascribing, in a very unjustifiable manner, *motives* to Dr. B., Mr. F. charges him with selfishness in not making his invention known to the profession. The liberal and scientific character of the Doctor is too well known and appreciated to require my pen to defend it from such invidious attacks. His silence will be attributed by every one acquainted with him to the proper cause—his peculiar diffidence and unwillingness to appear before the public. In conclusion I would remark, that the "compound catheter" has been well known not only in Glasgow for the last ten years, but throughout Scotia, whose sons, I trust, will always feel proud of their illustrious "fore-runners" in medicine, in whose steps so many are still treading, and pushing onwards in the science and practice of their profession; and whose labours and attainments can suffer no tarnish when placed in comparison with that of their contemporaries of whatever country or clime.

Soliciting an early insertion of the above remarks,—I remain, sir,

Your obedient servant,

JOSEPH BELL, Surgeon.

Barrhead, May 5, 1841.

SELECT SURGICAL CASES.

Reported by GORDON BUCK, M.D.,

Surgeon of the New York Hospital.

[Concluded from page 158.]

Immobility of the lower jaw; division of the masseter muscle of the right side.

JOHN BISHOP, seaman, aged 19, born in Georgetown, D.C. About eighteen months before his admission to the New York Hospital, he had an attack of fever at the south, during which he was profusely salivated, and sloughing of the right cheek had occurred, with loss of considerable portions of the soft parts from the inside of the mouth. On re-

covery, he was unable to open his mouth, a band having formed, by which the jaws were kept firmly applied to each other. Unsuccessful attempts had been made to relieve this condition by dividing the constricting band, but without using means, at the same time, to force the jaws apart. Two portions of bone had been discharged.

On the 19th of August, 1839, when an operation was performed for the purpose of restoring the functions of the lower jaw, the parts presented the following appearances:

The right cheek was full and swollen, the skin and subcutaneous cellular tissue supple and moveable upon the masseter muscle, which could be felt contracting under the hand whenever he put it in action. In a state of rest this muscle felt hard and tense. On the inside of the cheek, a firm callous band extended from above the interval between the first and second upper molar teeth on the outside, to below the first molar tooth of the lower jaw, with a sharp unyielding edge, that would not permit the end of the finger to be insinuated between it and the outer surface of the teeth. The jaw was susceptible of a sliding motion, showing that the right tempora-maxillary articulation was moveable. The upper dental arch stood a little in advance of the lower, barely allowing the blade of a table knife to be introduced between them. His voice was very little affected. He was able to take solid food by cutting it very fine, and insinuating it between the upper and lower teeth. His general health was pretty good, and he had observed no change in the condition of his jaw for a year past.

Operation.—A bandage of two fingers' breadth, and sixteen inches in length, was insinuated between the upper and lower teeth, and the ends tied so as to form a loop below the chin. One assistant held the head firmly, while another acted on the lower jaw, by means of the bandage, bearing down so as to put the band to be divided as much on the stretch as possible. The fore-finger of the left hand was then introduced under the cheek, and the operation proceeded with, care being taken to avoid cutting near the middle of the masseter muscle, in order not to involve the parotid duct. Successive incisions from within outwards, and advancing from before backwards, guided by the sense of touch alone, were made on a level with the lower molar teeth, until the finger arrived at the last tooth; resisting bands were felt still farther back and were divided. By repeated attempts with instruments that acted as levers in prying the teeth apart, sufficient space was obtained to introduce the speculum oris, which acted with great effect in rupturing the fibres which still bound the jaws together. Proceeding cautiously, in this manner, alternately prying and dividing the

resisting bands, the jaws were separated so as to allow two fingers to be placed edgewise between the incisor teeth. The whole width of the masseter muscle was involved in the incision, and in some parts its whole thickness; the knife grating as if cutting through cartilage; the hæmorrhage was moderate, and ceased spontaneously. After the operation, the patient could himself open his mouth to the extent to which the teeth had been separated. A denuded bony surface was felt on the outside, a little behind the last molar tooth, and could be traced upwards to a rough pointed extremity, which was somewhat moveable. A wooden wedge was introduced between the molar teeth of the opposite side, and required to be kept in as much of the time as it was possible for him to bear it. Considerable swelling and inflammation succeeded the following day, and continued for some time. The use of the wedge was persevered in.

September 9th.—He could chew all kinds of food, and separate his teeth nearly an inch apart; with the wedge they could be separated still farther. The wound on the inside of the cheek is nearly healed; there is evidently a tendency to a reproduction of the same condition. A band is felt at the edge of the masseter when the parts are put on the stretch, resisting the farther separation of the jaws. He is himself sensible of their contracting. Introduced the speculum, and stretched the parts, and then substituted a thicker wedge, to be kept in as much as possible.

October 1st.—For a few days past the patient has used the speculum several times daily, for the purpose of stretching the parts which have still a great tendency to contract, and it produced a very good effect. By way of experiment, the jaws were left to themselves for twenty-four hours, when it was found that the thumb could scarcely be introduced edgewise between the teeth.

November 6th.—Since the last report the same treatment has been continued. Some of the newly formed bands have been divided. The patient has himself removed a portion of bone, which proved to be the extremity of the coracoid process, about three-fourths of an inch in length; he is now employed as nurse, and is left to use the speculum at discretion.

March 31st, 1840.—For several weeks past he has entirely discontinued the use of the speculum, and the parts have been left to themselves; his condition is as follows. The right cheek is much less swollen than it was before the operation, and is soft and supple; the masseter muscle feels hard. The forefinger can be introduced edgewise between the incisor teeth, and within these limits he has free use of the jaw, and perceives no tendency to farther contraction. The callous

band on the inside of the cheek exists very much in the same condition as before the operation, excepting that it does not advance as far forward.

May 30th.—He continues to enjoy the same use of the jaw as at the last report. A loose portion of bone is felt opposite to the upper molar teeth, and will require to be removed.

Enlargement of the bursa in front of the patella.—Cured by rupturing it.

Thomas Cranmer, of New Jersey, a seaman, aged 26, was admitted April 11th, with an enlargement of the bursa in front of the patella, caused by an injury about ten days previous. The tumor was of a globular form, and nearly half as large as the patient's fist.

It was seized firmly between the fingers, and pressed with great force, until it burst and discharged its contents into the cellular tissue beneath the skin. A roller was then applied, and the limb kept at rest for a few days. The patient was discharged cured on the 16th of April.

REMARKS.—Tumors of this sort are occasionally difficult to manage, and patients will not generally submit to have them broken by a direct blow. I treated a case precisely similar to the present, about a year previous, by tapping and injecting it with a solution of sulphate of zinc, as in the operation for hydrocele. In a few days after this operation, the tumor was again distended with serum as large as before, and continued so, until I finally treated it, as above described, by pressure until it burst, and with instant and permanent success. The enlarged bursæ occurring about the wrist, and occasionally under the chin, are more readily managed in this way than by any other means.

The other cases that called for operations, all terminated successfully: they present no points of special interest.

Opium mixed with superacetate of lead, in large dose, swallowed with impunity.

A seaman under treatment for a carious ulcer, near the lower part of the thigh, was seized with slight erysipelatous inflammation around the surface of the sore, for which the house-surgeon prescribed an opiate lotion, to be made from thirty grains of opium, and sixteen grains of sugar of lead. The patient, mistaking the directions, swallowed these substances in their dry state, about 8 o'clock in the evening of Jan. 26th. About midnight he began to feel unwell, and thinks he threw up a part of the powder; but he did not rest well during the night; no symptoms of narcotism followed the accident: on the following morning he was as comfortable as usual, and unaware that anything had gone amiss.

Is it possible that the superacetate of lead, which is known to precipitate the extractive matter of vegetable infusions, could in this case have had any effect as an antidote? The opium was good, and the patient had not been in the habit of using this substance.

An instance somewhat similar happened in the New York Hospital in the summer of 1837. A patient swallowed, by mistake, a drachm of opium mixed with a scruple of the superacetate of lead. The accident was discovered in about half an hour: an emetic was administered, and no serious consequences ensued.

ANCIENT MEDICAL STUDIES AT CAMBRIDGE.

THE ancient statute (*Stat. Ant.* 119.) prescribing the course of medical studies and exercises in this university, presents a very singular picture of the state of medical knowledge in those days. It made it generally necessary for a student to have graduated and *read* in arts, and to have subsequently, during a period of five years, heard *once* in the schools of his faculty, the book of Johannicius Philaretus *de pulsibus*, Theophilus *de urinis*, the Antidotarium of Nicholaus, with some one of the books of Isaac (Israelita), whether *de urinis*, *de febribus*, *de dietis particularibus*, or *de viatico*; he was required also to have heard *twice* the works of Galen, with their glosses or comments (known only by their Latin translations from the Arabic, as the works of Tegnus Galienus), including his book of *prognostics*, of *aphorisms*, and *de regimine acutorum*; it was further required that he himself should read *cursorie*, within not less than three years of his *readings* in arts, (*Stat. Ant.* 90) at least one book on the theory and another on the practice of medicine; that he should have both opposed and responded in the schools of his faculty, and should have practised at least during one year: he was then admitted to his degree after the usual deposition and forms.

It is added, as an additional provision to the same statute, that no one shall be admitted *ad incipiendum in medicinâ*, unless he had practised medicine at least two years, a condition apparently inconsistent with that which is contained in the body of the statute, though it is very possible one might refer that to the degree of bachelor, and the other to that of doctor in medicine. If a person had not exercised the functions of a regent in arts, he was required to have attended the schools of arts and philosophy, at least during seven years, and afterwards during five years in the schools of medicine.

The books which formed the subjects of medical study were chiefly translations from the Arabic compilations from Galen and

Hippocrates (for their works were confounded together) composed by Arabic physicians, (such as the ten books of practical, and ten books of theoretical medicine of Isaac Israelita), which had been introduced into the University of Salernum towards the end of the 11th century, chiefly by Constantine the African. Theophilus was a Greek physician in the reign of the Emperor Heraclius, by whom likewise the treatise ascribed to Philaretus was written; and the work of Nicholaus was a production of the school of Salernum towards the close of the 12th century, which long enjoyed a distinguished reputation.—*Dr. Peacock's Observations on the Statutes of the University of Cambridge.*

ANATOMICAL MODELS.

THE Society of Arts, at their meeting on Wednesday evening last, voted their Gold Isis Medal to Mr. Simpson, surgeon to the Westminster General Dispensary, for the application of papier mâché to the making of anatomical figures, and models of morbid anatomy. Mr. Simpson, some years ago, turned his attention to the constructing of anatomical models, in consequence of the difficulty and expense at that time attendant on procuring subjects for dissection. The materials in general use for anatomical models were wax or plaster, of which the former was found to be too expensive to come within the means of lecturers, and students in general; and was too delicate to be handled in the lecture-room without incurring the chance of considerable damage. The plaster of Paris models were also objectionable, on account of their great weight and brittleness. The material which he uses is paper worked into moulds taken from dissections: this produces a model of extreme lightness, and so hard that it may be freely handled without danger of damage. The external surface is painted in oil-colour, representing the appearance of the dissection.

The right side of the figure which Mr. Simpson exhibited to the Society represents the superficial arteries, veins, nerves, and muscles; the left side shows the second and third layers of muscles, the deep-seated vessels, and more particularly those parts concerned in surgical operations.

The internal parts are removeable, so as to exhibit the natural arrangement of the thoracic and abdominal viscera, the brain and its membranes, the spinal marrow, tongue, &c.

Mr. Simpson having conceived that his invention would be particularly serviceable for the study of anatomy in warm climates, where wax models cannot be used, nor the natural subject be conveniently dissected, he submitted some specimens to the Honourable East India Company, who some time since

purchased forty of his figures, which were sent to various settlements in India, for the use of the European surgeons, as also for the instruction of the native surgeons, who were assistants in the Company's Military Hospitals; the religion of the latter prohibiting their studying from dissections.

RECEIVED FOR REVIEW.

Mr. Shand's Observations on the Formation and Adaptation of Public Buildings and Apartments to the Laws which regulate the Formation and Propagation of Sound, as consistent with the Economy of Speech.

Researches in Operative Midwifery, &c. with Plates. By Fleetwood Churchill, M.D. &c.

Hints for Invalids about to visit Naples, &c. &c.. By J. C. Cox, M.D. F.L.S.

Madras Quarterly Medical Journal, edited by Samuel Rogers. Vol. 2, 1840.

Popular Cyclopædia of Natural Science. Vegetable Physiology.

Practical Remarks on the Measures proposed for Reform in the Medical Profession. By a General Practitioner.

Transactions of the Provincial Medical and Surgical Association, Vol. 9.

Dr. Marshall Hall on the Diseases and Derangements of the Nervous System.

Dr. Reid on the Philosophy of Death.

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Wednesday, May 5, 1841.

Charles Deazeley. — James Paton. — Charles Christopher Erasmus Hopkins. — Henry Payey. — John Gallagher. — George Leopold Perfect. — Thomas Thexton. — James David Brown. — James Thompson. — William Eagles Johnson. — William Newton. — James Arnold. — Thomas Barker Smart.

Friday, May 7.

George Benwell Rosewall. — George Kingston Barton. — John Frederick Owen. — James Raney Leach Allott. — John Shaw Willes. — William George Swan. — Panks James Wiggington. — John Dalston Jones. — Alfred Clarke.

Monday, May 10.

Richard Seally. — William Pearce Berryman. — Osbert Fishlake Cundy. — Archibald Dougherty. — Edward Augustus Rawlinson. — Walter Lawrance. — Nathaniel Moore. — Edward Smith. — James Richardson Johnson. — Edward Gleeson. — William Viner Bradle. — Christopher Fryer Curtis.

APOTHECARIES HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, May 6, 1841.

John Thimbleby, East Kirby, Lincoln. — William Toogood, Ashborne, Derbyshire. — William Bell, Walworth Road. — Thomas Holt, Bury, Lancashire. — George Parry, Feckenham, Norfolk. — George Pizey, Douglas, Isle of Man. — Samuel Newham, Lynn, Norfolk. — Zebulon Mennell, Whitby, Yorkshire. — John Gladdich

Sanders, Gravesend. — William Johnston Stuart, Wisbeach, Cambridgeshire. — Heynes Roger Hardwicke, Galway.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 1st May, 1841.

Small Pox	24
Measles	11
Scarlatina	9
Hooping Cough	40
Croup	9
Thrush	5
Diarrhoea	8
Dysentery	1
Cholera	0
Influenza	3
Typhus	24
Erysipelas	5
Syphilis	0
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	180
Diseases of the Lungs, and other Organs of Respiration	279
Diseases of the Heart and Blood-vessels	21
Diseases of the Stomach, Liver, and other Organs of Digestion	70
Diseases of the Kidneys, &c.	12
Childbed	5
Ovarian Dropsy	0
Diseases of Uterus, &c.	3
Rheumatism	0
Diseases of Joints, &c.	4
Ulcer	0
Fistula	0
Diseases of Skin, &c.	0
Diseases of Uncertain Seat	115
Old Age or Natural Decay	59
Deaths by Violence, Privation, or Intemperance	15
Causes not specified	5

Deaths from all Causes 907

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

May.		THERMOMETER.		BAROMETER.	
Wednesday	5	from 48 to 61		29.38 to 29.53	
Thursday	6	47 62		29.51	29.73
Friday	7	45 65		29.66	29.56
Saturday	8	48 56		29.50	29.68
Sunday	9	41 61		29.94	30.07
Monday	10	49 65		30.07	30.11
Tuesday	11	45 68		30.03	29.98

Wind, S.W. except the afternoon of the 6th, and morning of the 11th, when it was S. by E.

On the 5th, morning overcast, raining very heavily during the morning; afternoon and evening clear. The 6th, morning cloudy, with rain, otherwise clear. The 7th, evening overcast, with rain, otherwise clear. The 8th, alternately clear and cloudy, frequent showers of rain during the day. The 9th, generally clear; rain fell about one P.M. The 10th, generally cloudy, sun shining frequently during the day; a little rain fell in the morning. The 11th, evening cloudy, with rain, otherwise clear.

Rain fallen, 1 inch and .155 of an inch.

Haines and Parhelia.—These phenomena at this season of the year are frequently to be seen. On the afternoons of the 6th and 7th, shortly before sunset, both were very distinctly observable.

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FRIDAY, MAY 21, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXXV.

Epilepsy. Its symptoms and varieties; duration and recurrence of the paroxysms; periods of life at which they commence; warnings. Effects of the paroxysms, immediate and ultimate. Pathology. Anatomical characters. Causes.

THE great functions of the brain are sensation, thought, and voluntary motion. The influence of the will is a cerebral influence: it reaches and acts upon the muscles through the interposition of the spinal cord. Motions that are involuntary belong more exclusively to the system of the true spinal marrow. Yet cerebral changes, morbid states of the brain, may excite them.

I have shewn you that all these functions are liable, under disease, to be separately affected, and each in various ways and degrees. The number of combinations capable of arising out of disordered conditions of two, or three, or all of these functions, is very great. Yet the symptoms proper to the nervous system do arrange themselves into groups sufficiently definite and constant to allow of our giving them distinctive names, and making them separate objects of enquiry.

At the same time, as might indeed be expected, these several groups have strong resemblances to each other. They are obviously of the same family: "facies non omnibus una, Nec diversa tamen; qualis debet esse sororum;" occasionally the features

are so nearly alike, that we find it somewhat puzzling to determine with which of the sisters we are conversing; but usually there is some mark or other by which the individual may be identified.

Of these essentially nervous diseases, there are several in which the most prominent and obvious of the phenomena relate to the muscular system; irregular, and violent, and involuntary contractions occurring in muscles which, in the healthy state of the body, are subject to the control of the will. I have spoken of two very frightful disorders belonging to this head:—of *tetanus*, namely, in which the muscles of voluntary motion present the most striking changes, being affected with tonic spasm; while the sensibility undergoes no other alteration than what is a consequence of that spasm, pain I mean in the muscles themselves; and the intellectual functions continue undisturbed:—and of *hydrophobia*, in which the natural sensibility suffers much, and the mental functions some derangement; yet still the characteristic features of the malady depend upon the irregular and uncontrollable action of voluntary muscles.

The disease which I am next to consider is scarcely less terrible to witness, when it occurs in its severer forms, than tetanus or hydrophobia; but it is not attended with the same urgent and immediate peril to life. Yet it is, upon the whole, productive of even more distress and misery; and is liable to terminate in worse than death. You will understand that I am alluding to *Epilepsy*: a disease not painful probably in itself; seldom immediately fatal; often recovered from altogether: yet apt, in many cases, to end in fatuity or insanity; and carrying perpetual anxiety and dismay into those families which it has once visited.

The leading symptoms of epilepsy are, a temporary suspension of consciousness, with clonic spasm; recurring at intervals.

It is impossible to frame a perfect *defini-*

tion of epilepsy: nay, so various are its forms, so numerous its modifications, that no general *description* even of it can be given. It will be necessary for me therefore here (as it has been before) to describe first the most ordinary type of the disease, as a standard; and then to note the several variations from that standard which are known to occur in practice.

A man, then, in the apparent enjoyment of perfect health, shall suddenly utter a loud cry, and fall instantly to the ground, senseless and convulsed. He struggles violently. His breathing is embarrassed or suspended; his face turgid and livid; he foams at the mouth; a choking sound is heard in his wind-pipe; he appears to be at the point of death by apnoea: but presently, and by degrees, these alarming phenomena diminish, and at length cease; the patient is left exhausted, heavy, stupid, comatose: but his life is no longer threatened. And in a short time he is once more to all appearance perfectly well. The same train of morbid phenomena recur, however, again and again, at different, and mostly at irregular intervals.—This is a brief description of the most ordinary form of epilepsy.

The suddenness of the attack is remarkable: in an instant, when it is least expected by himself or by those around him, in the middle of a sentence, or of a gesture, the change takes place; and the miserable sufferer is stretched foaming, struggling, and insensible upon the earth. This fearful suddenness is expressed in the name of the disease, *ἐπιληψία*, a seizure, an abrupt invasion. The ancients, among whom the complaint was well known, superstitiously ascribed it to the malice of demons, or the anger of their offended deities. If a person was seized with epilepsy in the forum, it was considered an ill-omen, and the meeting was at once dissolved, and all public business suspended for that day: hence the disease was called *morbus comitialis*. *Morbus qui sputatur* was another of its names, because those present were accustomed to spit upon the epileptic man, or into their own bosoms; either to express their abomination, or to avert the evil omen from themselves. In this country its common designation is the *falling sickness*: or, more vaguely, *fits*. The cry which is frequently, though by no means always, uttered, is generally a piercing and terrifying scream. Women have often been thrown into hysterics upon hearing it; it is even said to have caused pregnant females to miscarry: even the lower animals seem to be sometimes startled and alarmed by a note so harsh and unnatural. Dr. Cheyne informs us that, upon one occasion, “a parrot, himself no mean performer in discords, dropt from his perch seemingly frightened to death by the appalling sound.”

The muscular convulsions are strong, irregular, and often universal. In many of the fits of which I have happened to see the commencement, the first effect of the spasm has been a twisting of the neck, the chin being raised, and brought round by a succession of jerks, towards one shoulder: and one side of the body is usually, I think, more strongly agitated than the other. The features are always greatly distorted. The brows are knit; the eyes sometimes quiver and roll about, sometimes are fixed and staring, sometimes are turned up beneath the lids, so that the cornea cannot be seen, and the white sclerotica alone is visible; the mouth is twisted awry; the tongue, thrust between the teeth, and caught by the violent closure of the jaws, is bitten, often severely; and the foam which issues from the mouth is reddened by blood. The hands are firmly clenched, and the thumbs bent inwards upon the palms: the arms are thrown about, striking the chest of the patient with great force, or bruising themselves against surrounding objects, or inflicting hard knocks upon the friends and neighbours who have hastened to the patient's assistance. It frequently happens that the urine and excrement are expelled during the violence of the spasm: and seminal emission sometimes takes place. The spasmodic contraction of the muscles is occasionally so powerful as to dislocate the bones to which they are attached: the joints of the jaw, and of the shoulder, have been thus put out; and the teeth are sometimes fractured.

When the convulsive paroxysm is over, the patient falls into a deep sleep. You might imagine that he slept from exhaustion, like a man worn out by great fatigue: but there is something more than this; the patient passes into a state of imperfect coma, or rather the insensibility continues after the convulsions have ceased. When he wakes he is often confused and incoherent for a time; by degrees, however, he resumes his ordinary appearance and condition: but he remembers nothing of what passed during the fit.

You may suppose that so much irregular contraction of the muscles of voluntary motion is not likely to occur without some derangement or modification of the functions of the circulation. The breathing is irregular, gasping, or arrested. The heart palpitates violently against the ribs during the paroxysm; the pulse becomes frequent and feeble; and sometimes it ceases to be tangible at the wrist during the height of the fit, and begins to be felt again as the spasms subside. The turgescence of the face indicates obstruction of the venous circulation; the cheeks and lips become purplish and livid, and the veins of the neck and forehead are visibly distended.

This then is one form, the most severe

and the most common as well as the best marked form, in which an epileptic attack occurs.

But there is a large class of cases, in which the symptoms are much more mild. There is very slight and transient, or even no convulsion at all; no turgescence of the face; no foaming at the mouth; no cry: but a sudden suspension of consciousness, a short period of insensibility, a fixed gaze, a totter perhaps, a look of confusion; but the patient does not fall. This is momentary; consciousness presently returns; the patient resumes the action in which he had been previously engaged, and is not always aware that it has been interrupted. Sometimes, with this temporary abeyance of the mental functions, there is some slight evidence of convulsion or involuntary action; the fingers of one hand, or less commonly of both, are moved irregularly, and without any object; or the eyes roll or are turned upwards; or the muscles of the face are twitched. Sometimes the patient is himself aware of what has been his condition, but shews some cunning in endeavouring to conceal it.

This slighter attack is called by the French *petit mal*; while the severer form is named *grand mal*. The former is spoken of also as *epileptic vertigo*, and distinguished by that appellation from the *epileptic fit*.

Of affections so different in degree, and in some respects so dissimilar in kind, you may be disposed to ask whether they really constitute the same disease. That they are essentially of the same stamp, we have this evidence; that both forms of attack occur in the same individuals. Sometimes a patient will suffer many recurrences of the epileptic vertigo, and at length will become affected with violent epileptic fits. Or the two forms will intermingle, sometimes the milder happening, sometimes the severer. In such cases we cannot doubt that the attacks are the same in nature, though different in form and degree. And when (as sometimes happens) we meet with the slighter disease alone, we cannot refuse to assign to it the character and the name of *epilepsy*.

Between the two extremes, there are many links of gradation. Sometimes the sufferer sinks or slides down quietly, and without noise, is pale, is not convulsed at all, but insensible; much like one in a state of syncope. After recovering he remains sick, languid, and confused, during the remainder of the day.

You will perceive from what I have now said, the difficulty of giving any single description of epilepsy, which will include all its varieties. It is of course still more difficult to offer a strict definition of the disease. Cullen defines it to be “*muscu-*

lorum convulsio, cum sopore.” Dr. Copland furnishes a larger and more comprehensive definition. “Sudden loss of sensation and consciousness, with spasmodic contraction of the voluntary muscles quickly passing into violent convulsive distortions, attended and followed by sopor, recurring in paroxysms often more or less regular.”

But almost every one of these circumstances may, in its turn, be wanting. There may be no convulsion; there may even be very slight and transient interruption of consciousness; there may be no subsequent coma or sopor; there may be no recurrence of the attack.

Yet I trust that you now have obtained some general notion of what is meant by an epileptic seizure. And I go on to enquire into several most important points connected with the paroxysms.

In the first place, they vary considerably in *duration*. Sometimes, as I have already stated, the seizure is slight, and does not occupy more than a moment or two of time. But even the severer attacks are often over in a few minutes. They seldom continue longer than half an hour; and probably the average duration may safely be laid at between five and ten minutes. Attacks that are spread over three, or four, or more hours, generally consist of a succession of paroxysms, with indistinct intervals of comatose exhaustion. In the long-continued fits, or in the protracted succession of fits, the patient often dies.

The periods at which the paroxysms *return* are also extremely variable. Occasionally the patient dies in the first paroxysm; occasionally, though he recovers from it, he never has another. Both of these occurrences are rare. Rather more frequently the fits occur at very long intervals; at the distance I mean of many years. Most commonly of all they revisit the sufferer at irregular periods of a few months or weeks: sometimes they are repeated at intervals of a few days: sometimes every day, or every night: and not very unfrequently they take place many times in the 24 hours. This extreme frequency of repetition belongs principally to the slighter imperfect seizure; the *petit mal*. Sometimes the fits observe a strictly *regular* period of return; but, for the most part, they are quite uncertain and *irregular*.

The *time of life* at which the fits *commence*, and the circumstances attending their commencement, are deserving of notice. They not uncommonly begin in infancy. Those fits of convulsion to which young children are subject during the first dentition, and which sometimes appear to depend upon the irritation of teething, and sometimes upon manifest disorder of sto-

mach and bowels—these fits are not distinguishable in their phænomena from genuine epilepsy; and we must reckon them as instances of epilepsy. It has been remarked by some one, that if you can trace the early history of an adult epileptic, you will almost always find that he or she suffered infantile convulsions. To what extent this is true I do not know, but I recommend it to you, as a point worth attending to in your future opportunities of observation.

The epileptic attack may come on, for the first time, at *any* age. According to Dr. Bright (whose account of the disease, though short, is particularly perspicuous), the most common periods are about the age of seven or eight years, probably about the time of the second dentition; and from fourteen to sixteen, shortly before the age of puberty. And the disease (he says) is very apt to occur for a few years subsequently to this. But sometimes the first fit has taken place between the ages of thirty and forty; in not a few cases, after sixty; and occasionally quite in the decline of life.

Dr. Bright offers a little piece of theory in respect to the periods at which epilepsy is apt to begin. It is a reasonable piece of theory, and serves to tie the alleged facts to one's memory, even if it be not yet proved to be true. Doubtless in many cases the circumstances that determine the first attack are quite accidental. But setting aside these casualties, he says, "there are leading periods in the evolution of the frame, and peculiar circumstances connected with certain periods, which may well be considered as influential in the production of the disease. In infancy, the nervous system is delicate, and easily acted upon by various causes of irritation. Then follows the trying period of teething. In a few years, the second dentition occurs. In a few years later, all the great changes connected with the age of puberty. To this follow the excesses and exposures of manhood: and, after the lapse of years, the vigour of the system fails, and many causes act to derange the nice balance of the constitution; the bowels often become sluggish; changes more or less serious take place in the structure of the arterial and venous system; and many causes, organic or functional, which had before been unable to exert an influence on the vigorous frame, acquire power from its relative weakness."

The first accession of the disease takes place more commonly before than after puberty. Of 66 epileptic women, in whom the outset of that disease and the first period of menstruation were carefully noted, 38 had epileptic fits before, and 28 not till after that period.

The attacks are very apt to come on during the night; in the commencement of

the disease they frequently are *confined* to the night: they are said chiefly to occur at the moment when the patient is sinking into sleep, or awaking from sleep. How far this is true I cannot tell. When the disease is *yielding*, the fits often happen in the night only; so that after they have, for a certain period, taken place in the day-time, or during the day and night indiscriminately, it is reckoned a good prognostic sign if they begin to restrict themselves to the night. Some patients, under these circumstances, suppose that the physician has particular remedies that will make the fits happen in the night rather than in the day; and they ask for these remedies.

Sometimes each paroxysm arrives unannounced and unexpected; sometimes distinct *warnings* of its approach are given. The latter is less frequent than the former. Georget affirms that premonitory symptoms do not occur more than four or five times in 100 cases. I am sure that this is much understated. When warning symptoms do happen, they are sometimes spread over a considerable period; several hours, or a whole day: sometimes they just last long enough to enable the patient to remove from a situation in which a fall would be attended with unusual danger; to dismount from horseback, to lie down in a boat, to get away from the fire-place, from the edge of a precipice, from the vicinity of water, to assume the horizontal position of his own free will and in his own manner, or to give notice to those about him of what is going to befall him. In some cases the warning is too short and sudden even for this. The *kind of notice* that he receives is very variable indeed. Often it consists in some unnatural state of the mind, the feelings, the temper; the patient is fidgety, irritable, low-spirited, timid, sullen; or, on the other hand, he feels unusually strong, and hearty, and cheerful. Sometimes there is a notable change in some one or more of the natural functions, or of the bodily sensations; the patient loses his appetite, or his appetite becomes voracious; a great flow of urine takes place; he smells an ill smell, is aware of a strange taste, hears extraordinary noises, or sees spectral illusions; not mere specks floating before him—*muscæ volitantes*—but distinct forms of persons and things. This is not very common, but it certainly happens. The late Dr. Gregory, of Edinburgh, was assured by a patient of undoubted veracity, that always when he had a fit of epilepsy approaching, he fancied that he could see a little old woman in a red cloak, who came up to him, and struck him a blow on the head, and then he immediately lost all recollection, and fell down.

Headache, and giddiness, and dimness of vision, are all of them common symptoms

among those which have been observed to be precursory of epilepsy. Sometimes there are circumstances which are obvious to the bystander: a flushing of the face, or lividity; difficult articulation; vomiting. Of 21 epileptics treated in the hospital, at Wilna, by Joseph Frank, vomiting announced the paroxysm in 7. Some of the uneasy feelings are apt to come on and continue even for several days previously to the attack; restlessness in particular, disturbed sleep, distressful dreams, a peculiar and sudden coldness of the extremities. An internal *working* is a phrase often used by such persons to express a sensation which is probably indescribable.

But the most curious precursory symptom of all, if we except the spectral illusions, is what is called the epileptic *aura*. This is a sensation which is likened, by different patients, to different things; to a stream of warm or cold air, to the trickling of water, to the creeping of a spider. The sensation proceeds commonly from some distant part of the body, from one of the extremities, from a thumb, or finger, or toe, or from some spot on the trunk, and runs along the skin, towards the head; occasionally it gets no farther than the epigastrium: as soon as it reaches the head, or stops at the epigastrium, or elsewhere, the patient's consciousness forsakes him, and the paroxysm declares itself.

There seems to be some analogy between this epileptic aura and the well-known sensation, to be spoken of hereafter, of a ball rising from the stomach to the throat, and constituting the globus hystericus; except that in cases of epilepsy the sensation commonly begins in an extremity, and not in the stomach; and the fit comes on when it reaches the head, and not the throat. Sometimes, I think, these two sensations are blended.

In some instances, spasms of the muscles of the part whence the aura proceeds are observed to take place prior to the more general state of spasm.

This aura is certainly a very curious phenomenon. It has been thought to depend upon some change propagated along the nerve upwards to the brain, and that it is sometimes connected with some injury done to, or some morbid impression made upon, an afferent nerve. I think that this explanation may apply to some cases.

In a patient of my own, who was subject to epilepsy, the warning sensation commenced in one of his thumbs, which presently after began to be twisted inwards; but by tying his handkerchief tightly round the thumb, he could prevent the fit. Dr. Seymour mentions the case of an epileptic boy, who had learned to protect himself

against a threatened paroxysm, by biting his tongue.

In other cases the aura probably originates in some change within the head, and is analogous to the numbness or tingling that is often felt in some part of the body or extremities immediately before an attack of palsy or apoplexy. There is no real inconsistency in this twofold explanation; the source of the aura may be centric or eccentric; as I shall explain to you, in due time, the exciting cause of the paroxysm may be.

A knowledge of these warning circumstances is clearly of importance, always as far as respects the comparative security of the patient during the attack; sometimes as affording us the opportunity of staving off the fit altogether. And it is necessary to remark, that they sometimes give, as it were, a *false alarm*; they occur, and yet, although no measures of prevention are taken, no paroxysm follows.

The phenomena that *succeed* the paroxysm are also of great interest and moment.

I have already apprised you that the convulsions terminate in general before the insensibility is over: the patient draws, perhaps, two or three deep sighs, and ceases to struggle. Some few persons are quite themselves again in a few moments; some appear to recover consciousness, and then fall into a deep and prolonged slumber; but many do not regain their consciousness at all upon the cessation of the convulsions, but remain in a state of profound stupor, from which however they can generally be roused for a time. This state of coma (for it is nothing else) has been known to last a week: after the patient emerges from it, he is sometimes merely languid and inert; sometimes he is like a person stunned, or in a state approaching to idiocy, which gradually clears up; sometimes he is furiously delirious for a short time; not unfrequently there is a degree of partial paralysis, which also usually soon goes off, though it occasionally is permanent; the eyes are fixed, or he squints, or the pupils are dilated, or he drags a leg, or he falters in speech. Most commonly he speaks of headache, or discomfort of some kind.

It is very seldom that the patients have any recollection whatever of what has passed during the fit. Many of them are not aware that they have had a fit: and those who do know it, discover the fact by finding themselves wet or dirty; by the injuries they have received during the convulsions; by the soreness of the bitten tongue; by the bruises of their limbs; or by the confused or painful sensations which they subsequently experience, and which they have learned to associate with the conviction that a fit has

happened, by having been informed on previous occasions, when they felt the same sensations, that they had suffered a paroxysm of insensibility and convulsions.

Upon the whole, it is seldom that any ill effect can be noticed as having been left behind it by any one single fit; but, alas! this cannot often be said of their repetition.

Doubtless a single paroxysm does often leave the patient in a worse condition than that in which it found him; but this does not become perceptible to an ordinary observer, until after the alteration has been rendered apparent by repeated fits, and repeated small additions to the permanent injury. The friends of the patient perceive that his memory is enfeebled in proportion to the number of the attacks; that his mental power and intelligence decline. His features even assume, by degrees, a peculiar character; and too often he sinks into hopeless fatuity, or confirmed imbecility or insanity. It is this tendency which renders epilepsy so sad and fearful a disease.

Foville affirms, that the intellectual degeneration is more constant, and comes on more early, in persons who are principally afflicted with the epileptic vertigo, the *petit mal*, the imperfect seizure, than in persons in whom the *grand mal*, the violent and decided paroxysm, takes place. Dr. Copland, on the other hand, is of opinion, that "the more severe the fits, the more is that result to be dreaded." This is a point which can only be settled by statistical facts. And as we all have the opportunity of collecting some such facts, and of adding them to the general stock, I mention this, and some other points that are still uncertain or disputed, as worth bearing in mind. More, probably, depends upon the *repetition* of the fits, than upon their precise *nature* or *severity*. Cases do occur in which epileptic persons preserve their faculties to a good old age: but those who are early epileptic do not often attain old age; and *whenever* the disease comes on, if it repeats itself frequently, it is much more often than not followed by impairment of the mind, or by some apoplectic or paralytic affection, which implies and accompanies the mental change. You will sometimes hear the cases of Julius Cæsar, of Mahomet, or of Bonaparte quoted, as examples of high intellectual power, existing and remaining in spite of epilepsy:—and it is allowable, perhaps, to make use of such cases for comforting the friends of epileptic persons; or for giving the advantage of sustained hope to the patient himself. But, in truth, these cases are not worth much. Napoleon is said, I know not upon what authority however, to have suffered something like epilepsy during sexual intercourse; this is not very uncommon in persons subject to that disease. And, with

respect to Julius Cæsar, we learn from Suetonius, that it was only in the latter part of his life that he laboured under epilepsy; and that he had two attacks while engaged in business.

Having now described the phenomena of epilepsy; the periods of life at which it is most apt to commence; its varieties; and its tendency and termination: let us next enquire what is known respecting the real seat and nature of this strange and melancholy complaint.

The functions that are affected are clearly the functions of the *brain*: sensation, thought, and motion regulated by the will, are the natural functions of that organ. The temporary abeyance of sensibility, thought, and volition; and violent and irregular action of the muscles, which are thus withdrawn from the empire of the will; constitute a paroxysm of epilepsy. We have, in this malady, another illustration of the fact, that when the controlling influence of the sensorium is suspended, the peculiar functions of the spinal marrow are exercised, not only in a disorderly, but also in an unusually energetic manner. That the brain and the spinal marrow, though physiologically distinct, are yet intimately connected with, and dependent upon, each other, a thousand familiar facts assure us; and there are good reasons for believing that the change, whatever it is, which is the immediate precursor and cause of the epileptic fit, may sometimes originate in the spinal cord, and thence extend to the brain; and sometimes originate in the brain, and communicate itself to the spinal cord. Dr. Marshall Hall's doctrine, that all convulsive diseases are diseases of the spinal marrow, cannot be properly applied to this convulsive disease of epilepsy. It is true that the spinal cord is concerned whenever there is convulsion; but it is concerned in every *voluntary* movement also, through the instrumentality of the brain itself; and it may be, and often is, irregularly influenced by a disordered and unnatural state of the brain. Tetanus may fairly be regarded as a disease of the cord, and its proper appendages; the spasms arise and reach their height, while the powers of thought and sensation are undisturbed, and while the volition remains, although the morbid condition of the cord renders it ineffectual. In epilepsy, these cerebral functions are always implicated. There is *always* a loss of consciousness: and in the epileptic vertigo, the *petit mal*, there is frequently a suspension of consciousness only, *without any convulsion at all*. The brain, therefore, we must consider to be essentially concerned in this disorder.

What the precise state of the nervous matter may be, which determines the loss of

consciousness and the spasms, we can only conjecture. A derangement in the relation between the arterial and venous circulation within the head; a temporary pressure somehow arising; a determination of blood towards the head; a diminution of the natural quantity of blood sent thither from the heart; all these have been assigned as possible causes of the paroxysms. Plausible reasons might be given in favour of the operation of each of them; but the speculation is more curious than useful. We have not yet penetrated the mystery of these remarkable phenomena, and it will be more profitable to turn to another question, which admits of a somewhat more definite answer, viz. :—what is the *morbid anatomy* of epilepsy?

Suppose that a person who has had epileptic fits, but in whom they have not been followed by any durable affection of the intellectual or locomotive functions, dies of some other malady; and you have the opportunity of minutely examining the condition of the nervous system. Often you will find nothing at all which can throw any light upon the occurrence of the epileptic paroxysms; no appreciable alteration whatever in any part either of the brain or of the spinal cord. In other cases you may discover some organic disease within the head: a scrofulous tubercle, a spiculum of bone projecting from the skull. Have we then detected the cause of the disease? All that can be said is, that the piece of bone or the tubercle was probably a *predisposing* cause of that derangement of the nervous substance which determined the paroxysms; the derangement itself, if, indeed, it was of such a nature as to be cognizable by our senses, has gone, with the symptoms; the tubercle or bone having in the meantime remained without any sign which could betray its presence.

M. Foville, whose testimony in this matter is entitled to much weight, affirms that in persons who have been subject to epilepsy, uncomplicated as yet with any permanent disorder of the intellect, or of the faculty of voluntary motion, and who have died *in the fit*, constant alterations are observable within the head; viz. a strong injection of the vessels of the encephalon: the membranes, the brain, and the cerebellum, are gorged, he asserts, with livid blood. But he goes on to say that this is to be ascribed to the mode of death; that we see the same appearances in persons who have died by hanging, or any form of apnoea; that they are not peculiar to epilepsy, and do not explain the attack, but only point out the way in which it has been fatal.

If such appearances were uniformly found in the heads of persons who die suffocated, or who die in a fit of epilepsy, this observation

of M. Foville's might be a very just one. But I doubt whether such internal congestion of the vessels of the head is met with in either case: for reasons formerly explained to you. In the only two instances in which I have had the opportunity of examining the state of the brain after death by hanging, there certainly was no such congestion.

It is, I fancy, a very common notion, both that such congestion does take place, and that it is the cause of the paroxysm: and it may be worth while shortly to state the reasons which are opposed to the conclusion, that the congestion (granting for the moment that it does happen) is a sufficient explanation of the attack.

In the first place it is not easy to conceive that the congestion could so suddenly arise and subside again, as it must sometimes do, if it be the immediate determining cause of the fit: within the space of a single minute, for example.

And in the second place, the signs of external congestion and plethora, by which signs we measure the amount, if there be any, of the *internal*, are most marked just when the symptoms of the paroxysm begin to subside and disappear. So that we cannot, I think, look upon the congestion as the *cause* of the convulsive symptoms.

Let us go a step farther, and enquire into the state of the encephalon in those persons who, having suffered epilepsy, had, before death arrived, been affected with some permanent impairment of the mental functions, or (what often goes along with such impairment) with some degree or other of muscular paralysis or debility.

The most common alterations met with in the brain in such cases are the following.

Induration of the white matter of the brain, which presents a dull appearance; sometimes, besides the hardening, a general injection of the white matter; and in the majority of cases a marked dilatation of the blood-vessels. In some instances the consistence of the white matter is diminished, it is soft and flabby; but there is the same dilatation of the blood-vessels. These changes pervade the whole of the white matter in every part of the brain. At the same time the grey matter is found irregular on its surface, marbled or of a rosy colour in its substance, and sometimes altered in consistence. And in many cases the membranes are found to be adherent in some parts to the convolutions with which they lie in contact.

Such are the results of the experience of careful observers in respect to the morbid anatomy of epilepsy; of Morgagni, of Foville, and of MM. Bouchet and Casauvielh. The changes last described are such as are produced by chronic inflammation of the brain and its membranes. They are the *consequences* (I imagine) of re-

peated paroxysms of epilepsy; they are the very same as are frequently met with in cases of insanity complicated with paralysis, and they elucidate, therefore, the connexion of these affections: but they certainly teach us little or nothing of that actual condition of the nervous mass upon which the epileptic paroxysms depend. And, in truth, to expect to find in the brain the traces of convulsions that have passed away, would be as unreasonable as to expect to find the traces of former voluntary movements.

Of those organic changes which may be regarded as strong predisposing causes of the paroxysms, my own experience accords with that of Dr. Bright; who states, that they are more frequently such as affect the surface, than the deeper-seated parts of the brain: tumors external to the cerebral matter, alterations in the bones of the skull; or in the membranes that envelope the organ. Various altered states of the spinal marrow have also been recorded.

But besides the morbid appearances that are *sometimes only* visible in the nervous centres themselves, there are others, which it is of great importance to attend to, situated in other parts of the body, and at a distance from those centres: diseased states of the liver; biliary concretions; renal calculi; stones in the bladder; worms in the alimentary canal; diseases of the uterus; and of various other parts. And these morbid conditions have often, no doubt, an intimate connexion with the epileptic paroxysms. Accordingly some authors make almost as many varieties of epilepsy as there are organs of the body; they specify the cerebral, the spinal, the cardiac, hepatic, gastric, intestinal, nephritic, genital, uterine, and so on. It will be sufficient, however, to consider two species only; that, namely, in which the disease originates in the nervous centres themselves, and especially in the brain; and that in which it originates in some other part. Most persons who have written on epilepsy make this distinction, although they employ different terms to express it. Cerebral and occasional; primary and secondary; idiopathic and sympathetic; centric and eccentric. The two last terms are the best. But let us clearly understand them. The disease may, in one sense, be considered eccentric, even when it is situated in the brain; eccentric, *i. e.* in respect to the true spinal marrow. But I apply the epithet centric to epilepsy when its cause lies in either of the two great nervous centres; the brain, or the cranio-spinal axis. The distinction itself we shall find to be an important one, both as regards the prognosis and the treatment. But I must first say a few words respecting the causes, and the diagnosis of epilepsy.

There is no doubt that a tendency to epileptic disease is frequently *hereditary*. It may be derived from parent to child; or it may skip over a generation or two, and appear in the grandchild or great grandchild; or it may be traceable only in the collateral branches of the ancestors. This is just what takes place in other hereditary maladies. You may often notice also that other forms of nervous disorder prevail in the same families.

MM. Bouchet and Casauvielh found that among 110 instances of epilepsy, 31 were hereditary. Of 321 persons afflicted with epileptic insanity, and seen by Esquirol, 105 were descended from insane or epileptic parents.

Again, a tendency to epilepsy is very often found to go along with an *unnatural form of the head*, which is pinched up like a sugar-loaf; or misshapen and unsymmetrical, one-half being unlike the other; or oddly configured in some way or other. Epilepsy is no uncommon attendant of chronic hydrocephalus.

And thirdly, the *scrofulous diathesis* is a strong predisposing cause of epilepsy. Dr. Cheyne even holds that epilepsy is as certain a manifestation of the strumous disposition, as tubercular consumption, or psoas abscess. Now of the two predisposing circumstances last mentioned, it may be observed, that they commonly merge in that which preceded them: the strumous diathesis, and a particular conformation of the head, are both very likely to descend from parents to their progeny.

Whether the sex has any influence in determining a predisposition to epilepsy, is a question that remains to be settled. Foville thinks it is most common in females; Dr. Elliotson in males. I have certainly seen more epileptic boys and men, than girls and women. But the casual experience of a single observer is not enough to determine the point. We want numerical statements on a large scale. At the close of the year 1813 there were 162 male epileptics in the Bicêtre; 289 female cases in the Salpêtrière. Jos. Frank observed that, of 75 patients, 40 were females.

REPORT UPON THE MORTALITY OF LUNATICS.

BY WILLIAM FARR, Esq., F.S.S.*

THIS report, which has been drawn up at the request of the Council of the Society, is founded on the reports of the Hanwell Asylum, returns from the Bethlem Hospital, and the valuable series of tables submitted to the Society last

* Read before the Statistical Society of London, 15th March, 1841.

year by Colonel Sykes. It was thought desirable that the mortality of lunatics in two of the largest public institutions of the country should be compared with the mortality in the licensed proprietary houses; and that, if the mortality differed, the differences should be investigated, and traced to their causes by the methods of statistical analysis which we now possess.

The condition of lunatics in this country has, within the present century, attracted much public attention; and in 1807, 1815, 1816, and 1827, the management of the asylums provided for their confinement was investigated by Committees of the House of Commons. Many abuses were brought to light; and the last committee, of which Mr. Gordon was chairman, stated in their report, after a searching and able inquiry, that the abuses discovered in 1815 still existed. They "repeated, adopted, and confirmed" the recommendations of the committees of 1807 and 1815. Enactments subsequently passed the legislature; and several county asylums (among which that of Middlesex deserves to be particularly mentioned), have since been erected. No parliamentary inquiry has been instituted since 1827; but Mr. Ewart has given notice of his intention to move in the House of Commons for the appointment of a committee in the present session of Parliament.

The persons of unsound mind in England amount to several thousands. They are usually of middle age, frequently parents, and are of all conditions and ranks of life: 494 lunatics confined under the Crown possess property yielding an annual income of £317,154*. Men of the highest intellectual rank—men of genius—are not exempted from the visitations of this disease: it stoops to the lowest, and disorders the meanest brain. It makes the labourer a pauper, and too often ruins the families of the middle classes. 6402 idiots, and 7265 lunatics, have been returned to Parliament as paupers. Such a disease, which disorders the senses, perverts the reason, and breaks up the passions in wild confusion;—which assails man in his essential nature,—brings down so much misery on the head of its victims, and is productive of so much social evil—deserves investigation on its own merits by statistical, as well as other methods. But it has an additional claim upon the attention of the Society.—A considerable portion of the insane are under confinement, and have to be provided for or watched over by the State, which, as it permits them to be deprived of liberty, is bound to afford them protection, and to assure them the best means of restoration to health.

Great improvements have taken place in the treatment of lunatics. In the best asylums they are no longer shut up in cells like wild beasts, nor punished by harsh keepers. Their chains have gradually been struck off. A further step has been attempted. At the Middlesex asylum no strait-waistcoats, straps, or other instruments of personal coercion, have been used since the 21st of September, 1839. The experiment was first tried at Lincoln, and it is now contended by persons of experience, ability, and integrity—by Mr. Hill, Dr. Conolly, and the visiting justices of Middlesex,—that in a house properly built, with skilful medical supervision, and a sufficient number of humane and intelligent keepers, personal coercion should be abolished. This is denied by other gentlemen of equal humanity, who maintain that, although all restraint *may* be dispensed with, the strait-waistcoats should still be employed as a remedy in the paroxysms of mania. A keen controversy has been waged on the subject. Asylums not only differ widely in the extent to which restraint is carried, but in the space allotted to patients in their employment, food, and medical treatment. The cost of criminal lunatics at Bethlem is 15s. a-week; of idiots or lunatics in the workhouses, 2s. 10d. to 3s. 6d. a-week. Some of the asylums are under the control of the visiting justices, others are visited by the Metropolitan Commissioners. The hospitals of Bethlem and St. Luke are not visited at all, but are managed by the officers and governors; while a very large number of lunatics are farmed out, or confined in workhouses, by the parish authorities.

Amidst these various circumstances and conflicting systems, we ask which is the most advantageous? And it will be replied by all parties,—“that is the best system under which the greatest number of lunatics recover their reason in the shortest time.” But in a slow disease, presenting so much diversity in individuals, it is evident that the superiority of any system of treatment can only be determined by the average results, by a comparison of the recoveries and deaths; in fine, by statistics. [Here follow a variety of calculations thrown into the tabular form, the general result of which is to demonstrate that the annual mortality of both male and female paupers in the licensed houses was nearly twice as great as the mortality of paupers at Hanwell, and twice as great as the mortality of other lunatics in the licensed houses.]

What is the mortality among lunatics in favourable circumstances? Is insanity a fatal disease?—Upon the latter question there has been a considerable diversity of opinion. Some lunatics live to an advanced age. Of 213 admitted by Dr. Conolly at Hanwell, 15 were aged 60 and upwards, and

* See Parliamentary Return, Session 1839, No. 378.

1 was between 70 and 80; and 58 in 753 at Hanwell had been labouring under the disorder between 20 and 50 years. In 1835 an action (*Fisher v. Beaumont*) was brought at the York Assizes to recover from the *Providence Assurance Company*, £2000, insured upon the life of the Rev. Mr. F***. In charging the jury, the judge said that they had to consider whether insanity had a tendency to shorten life? If insanity had such a tendency, they must find for the defendant; if not, for the plaintiff. The medical evidence was conflicting; and the jury, after a short deliberation, found for the plaintiff, on the ground that insanity had no tendency to shorten life!*

We have no means of ascertaining the mortality of lunatics at large; but the mortality of lunatics in asylums is much higher than the mortality of the general population, and the excess cannot be ascribed entirely, although it may partially, to the confinement, the unwholesomeness, or the usages of mad-houses. The mean age of lunatics in asylums is about 35-40. The average age of the patients admitted at Bethlem, (1830-34) was 36 years (36.2); and the mean age of 213 admitted at Hanwell by Dr. Conolly was 36½. The mortality at the age 30-40 is 1.2, and at 40-50 is 1.5 per cent. in England and Wales. In cities the mortality at a corresponding age is not more than 2 per cent. annually. Now the annual mortality at Bethlem, where dangerous cases are carefully excluded, was 9 per cent. in 1827-39. At Gloucester, one of the county asylums, at which the treatment is the most successful, the diet is generous and nutritious, and the patients live as much as possible in the open air,—the annual mortality is 7 per cent.

The annual mortality of severe cases of insanity cannot, I think, in favourable circumstances, be less than 6 per cent.; so that the mortality is three times greater among lunatics, than among the general population, at the same age. We have seen, however, that the annual mortality among the better class of patients in the licensed houses was 11 per cent., among paupers at Hanwell 12 per cent., among paupers in the licensed houses 21 per cent., and among pauper men at one licensed house 27 per cent.;—as high as the rate of mortality experienced by the British troops upon the western coast of Africa, and by the population of London when the plague rendered its habitations desolate!

To what is this excessive mortality to be ascribed—to the disease, or to the treatment?—The question cannot be positively answered, nor can the causes of the difference in the mortality be determined, without a

careful examination of all the circumstances. I shall briefly notice the chief causes to which the mortality of lunatics in asylums *has been, or may be, ascribed.*

The visiting justices of Hanwell state as “an extraordinary and disgraceful fact,” that numbers of patients are sent into the asylum, as it would seem, to die. Of 656 deaths, 64 occurred within a month after admission. A similar complaint is made at many hospitals; and there is probably a tendency to send dangerous cases, or cases in their most critical stage, to public institutions. The exclusion of such cases from Bethlem reduces the mortality, but they cannot all be excluded without giving the asylums the advantages of that *selection*, which is so profitable to assurance offices. For in a disease so fatal as insanity, a certain number of lunatics are necessarily on the verge of death at the period of the disease when admission into an asylum is usually sought; and a due proportion of such cases cannot fairly be excluded.

Reference has also been made to the fact that out of 834 patients in Hanwell on December 31st, 1839, about 655 had been in other asylums, or workhouses, for considerable periods. Many cases were admitted in the chronic stages of insanity; but this, though it will account for a smaller number of recoveries, and the high proportion of fatal cases, will not account for a high *annual rate* of mortality. The *annual rate* of mortality is greater in the acute than in the chronic state of insanity. Thus, at the hospitals of Bethlem and St. Luke the annual mortality among the class called “curables” was 11 per cent., and only 6 per cent. among “incurables” (chronic cases). At Hanwell the annual mortality of lunatics in the state of mania, monomania, or melancholia, appears, so far as it can be determined, to be about 12 per cent., while in cases of incoherence, imbecility, or dementia (chronic stages of insanity), about 8 per cent. die annually.

A return in the Hanwell Report shews the numbers admitted during each separate year into the asylum, and the numbers discharged cured, relieved, or dead, year by year. The return extends from May 16th, 1831, to September 30, 1840. From this table we learn that 422 lunatics were admitted in the year 1832; that 55 of them were cured or relieved, and 55 died during that year, leaving 312 to enter upon the next year (1833), when 27 of them were cured or relieved, and 31 died; and so on, year succeeding year, until September 30th, 1840, when 137 remained in the asylum. The patients admitted in 1831, and in 1839-40 (the two last years), have been set aside; and the 1389 lunatics admitted in the seven years (1832-38) have been followed to the

* MED. GAZETTE, August 8th, 1835.

end of 1839. The 422 lunatics, it may be assumed, were admitted at equal intervals of time in 1832, or the middle of the year 1832 may be taken as the mean time of their admission; whence it follows, that of 422 admitted in the year 1832, 55 died in the *half-year* following, 31 in the next *year*, &c. The table, therefore, permits us to trace 422 lunatics admitted in 1832, to death, recovery, or relief, during $7\frac{1}{2}$ years; and 325 admitted in 1838 to the end of 1839, or for a period of only $1\frac{1}{2}$ year. The subjoined table presents a summary of these interesting facts, which in the form of publication are analogous to the tables of lives published by the *Equitable Society*.

Table of the Deaths and Recoveries in different stages of Insanity.

Years.	Entering on each period.	In the succeeding periods.			Remained in the Asylum at the end of the Period.
		Cured.	Relieved.	Died.	
0.	1,389	125	25	152	0
0.5	1,087	142	13	130	208
1.5	594	14	9	42	30
2.5	499	9	0	45	46
3.5	399	10	2	28	51
4.5	308	6	2	20	34
5.5	246	4	0	21	66
6.5	155	4	1	6	144

The table is read thus :—Of 1389 lunatics entering upon the *first* period ($\frac{1}{2}$ year), 125 were discharged cured, 25 relieved, and 152 dead in the next *half-year*; of 1087 who entered upon the *second* period, 142 were discharged cured, 13 relieved, and 130 dead in the subsequent *year*, at the end of which 208 were lost sight of. For the reason before stated, it will be observed that the *first* period extends to the end of the 6th month; the second from the end of the 6th to the end of the 18th month; the third from $1\frac{1}{2}$ to $2\frac{1}{2}$ years, &c.

From this arrangement of the facts, the annual rate of mortality and recovery in the several stages of insanity, subsequent to admission, at Hanwell, may be deduced.

Table of the Number of Insane Persons living, cured, and dead; and of the Annual Rate of Mortality in different stages of Insanity.

No.	Period of the disease from the date of Admission.		Number living one year.	Cured or relieved.	Died.	Out of 100 living, one year.	
						Cured or relieved.	Died.
	Years.						
1	0	$0\frac{1}{2}$	619	150	152	24.2	24.6
2	$0\frac{1}{2}$	$1\frac{1}{2}$	944	155	130	16.4	13.8
3	$1\frac{1}{2}$	$3\frac{1}{2}$	1033	32	87	3.1	8.4
4	$3\frac{1}{2}$	$5\frac{1}{2}$	673	20	48	3.0	7.1
5	$5\frac{1}{2}$	$7\frac{1}{2}$	383	9	27	2.3	7.0
	0	$7\frac{1}{2}$	3652	366	444	10.0	12.2

The numbers stated to have been *relieved* were 14 per cent. of the numbers *cured and relieved*; and as the proportion remained nearly the same through the seven years, the two classes of facts have not been distinguished.

The annual rate of recovery in the *first half-year* was 24 per cent.; and the rate of mortality was nearly 25 per cent. The two rates remain high in the *second period* (the rate of recovery 16, and of mortality 14, per cent.), while they declined respectively to 3, and to 8 per cent. in the *third* period; and to 2.3, and 7.0 per cent. annually, between the $5\frac{1}{2}$ and $7\frac{1}{2}$ years after admission into the asylum.

The rate of mortality in an unit of time increases as the malady advances up to a certain point, and then declines regularly, in all diseases which have hitherto been investigated arithmetically. In cholera the rate of mortality is highest at 18 to 24 hours; in small-pox, the mortality is highest from the 10th to the 15th day; in consumption the

rate of mortality appears to be greatest from the 6th to the 9th month. The variation in the rate of mortality and recovery in small-pox is shewn below :—

Days of the Disease*	5—10	10—15	15—20	20—25	25—30	30—35	35—40
Daily Deaths in 100 living	6·4	31·5	27·2	8·5	4·3	2·8	2·0
Daily Recoveries in 1000	·1	·2	1·8	9·2	19·7	34·8	47·5

Insanity is regulated by analogous laws ; and a majority of the patients are admitted at Hanwell before the disease has passed the point at which the mortality declines, although many are admitted afterwards, when the rate of recovery is reduced much more than the rate of mortality.

At Hanwell, 18 in 100 living die annually in the first 1½ year ; and 8 in 100 annually for 6 years afterwards. If an asylum, therefore, contained none but persons in the first year and a half of the disease, (after admission is always understood,) the mortality would be 18 per cent. ; while it would be 8 per cent. in an asylum for chronic cases between 1½ and 7½ years. Without implying any disparagement to the treatment in the former case, the rate of recovery

in the two asylums would differ in a still greater degree, as it would be 19 per cent. in the first asylum, and only 3 per cent. in the second, set apart for the exclusive reception of the advanced cases. This separation seldom takes place in practice. The chronic and acute cases are always mixed in an institution like Hanwell ; but it is evident that in the first years after it was opened, the proportion of cases in the early stages must have been greatest, and the proportion of lunatics in advanced periods of the disease must have since progressively increased. According to the above laws, the proportion of deaths and recoveries should gradually have declined ; and this was the fact.

Periods of Three Years.	Lunatics† existing One Year.	Died.	Recovered.	Annually out of 100 living.	
				Died.	Recovered.
1831-3	1147	197	165	17·2	14·4
1834-6	1754	194	119	11·1	6·8
1837-9	2121	215	168	10·1	7·9

The annual mortality was 17 per cent. in the first three years, and 10 per cent. in the last three years ; the annual rate of recovery was 14 per cent. in the first, and 8 per cent. in the last period. In the licensed houses which have been many years in existence, the annual rate of mortality was 13·6 per cent. in 1833-36, and 17·2 in 1836-1839 !

When the rates of mortality and recovery in the several stages of insanity are ascertained, the effects of treatment and external influences can be compared in asylums containing the various classes of patients, in proportions as different as at Hanwell in 1831-33, and 1839-40. The rule is :—multiply the number of lunatics existing at the several periods of the disease by the corresponding rates of mortality and recovery (0·242, 0·246, &c. &c.), and the sum of the products will represent the number of deaths and recoveries. By this rule the deaths in Hanwell during the 1¾ year, ending September 10th, 1840, should

* British Medical Almanack, 1838, p. 212.
† Deduced from annual enumerations of the males and females ; the years of life=5,022. From daily enumerations the years of life=46,39.

have been about 149, and they were 128 ; the numbers cured or relieved should have been about 126, and they were 154.

It will be found by this rule that the rate of mortality among paupers in the licensed houses and in Hanwell, has differed less than the first result of the returns would lead us to suppose, although it has been excessive in both. The paupers remain little more than a year and a half (1·67) in the licensed houses, in which the annual mortality was 21 per cent. ; at Hanwell the annual mortality in the first 1½ year after admission was 18 per cent.

From the facts given at page 25, part of a table of mortality and recovery may be constructed for lunatics.—(See top of next page.)

If we take 1,000 lunatics at the stage of the disease corresponding to the time of admission at Hanwell, 217 will be discharged (108 recovered or relieved, and 109 dead) in the half-year following, leaving 783 to enter upon the second period, to be reduced year by year, until at the end of seven years and a half only 310 remain. The range of the present series of observations extends no further, but the relative proportion of recoveries and deaths remains nearly as 88 to

No.	Period of the Disease dating from the day of Admission	The Number of Lunatics who			Cases terminating in each Period.		
		Enter upon each Period.	Will Recover.	Will Die Insane.	Total Number.	By Recovery.	By Death.
	Years.	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	<i>e</i>	<i>f</i>
1	0	1000	380	620	217	108	109
2	0.5	783	272	511	213	112	101
3	1.5	570	160	410	61	21	40
4	2.5	509	139	370	48	12	36
5	3.5	461	127	334	43	11	32
6	4.5	418	116	302	41	11	30
7	5.5	377	105	272	35	9	26
8	6.5	342	96	246	32	8	24
9	7.5	310	88	222

222 during the last six years ; and to complete the scheme of the table it may be assumed that 88 of the 310 will recover, and 222 will die. The columns *b*, *c*, shew, therefore, that of 1,000 cases, 380 will recover, and 620 die ; that at the end of a year and a half, 160 will recover, and 410 will die.

The columns *e*, *f*, shewing, first, the numbers cured (“ or relieved,” is understood invariably) ; and, secondly, the numbers dying in each period are readily deduced from the table before given. Thus 150 were cured, and 152 died in the first half-year, out of 1,389 cases ; consequently, 108 were cured, and 109 died out of 1,000 cases, which form the basis of the new table. As 217 cases terminated during the first half-year, 783 entered upon the second period, when the amount of recovery having been = $\frac{155}{1087}$, and the mortality = $\frac{130}{1087}$, exactly 112 recovered,

and 101 died. By continuing the process, the deaths and recoveries are obtained for each year. The columns *a*, *b*, *c*, are obtained by the successive subtraction of the numbers in columns *d*, *e*, *f*, and the first number (620) in column *c*, is the sum of those who died in the $7\frac{1}{2}$ years, = 398×222 , the number who, it is presumed, will die insane *after* that period.

The numbers representing those who will die and recover (columns *b*, *c*,) decrease at a uniform rate after the second year, and the seven terms of the series, column *c*, are obtained by multiplying the last (222) successively by 1.1075 (log. 0.044338) ; the six terms in the series, column *b*, are deduced in the same way, from 88 raised successively by the rate 1.0966 (log. 0.040072). The two series of numbers obtained by the hypothesis agree very closely with those directly deduced from the observations—

Years . .		1½	2½	3½	4½	5½	6½	7½
Numbers to die insane	Direct observation	417	376	330	298	271	239	226
	By Hypothesis .	410	370	334	302	272	246	222
Numbers to recover	Direct observation	...	138	129	115	104	98	87
	By Hypothesis	139	127	116	105	96	88

Halley invented the Table of Mortality, which consists of three columns, shewing the number of persons who die or survive in each successive year. The events it exhibits are of one kind ; all the persons die. But cases of *disease* may terminate in two ways —by death, and by recovery. A different tabular construction was therefore required for sick persons, such as the one preceding, from which the *mean future duration*, the *probable duration*, the *probability of recovery*, and the *probability of a fatal termination* in any given time, can be determined at any period of the disease.

The *mean future duration of insanity*, or the *expectation of disease*, cannot be deduced from the preceding table, because it breaks off at the end of $7\frac{1}{2}$ years ; but if the annual rate continued the same (1.10), 7 of 310 would remain insane 40 years, and the mean future duration of insanity at the period of admission at Hanwell would be 6.7 years ; at the end of half a year it would be 8 years ; and after $1\frac{1}{2}$ year, it would be 10 years.

In the six years 1834-39, when the inmates were = 3,875 living 1 year, 706 were discharged ; one in 5.5 therefore was dis-

charged annually. If the institution had existed several years, and the numbers admitted and discharged had been equal, the mean duration would have agreed with this, and have been 5.5 years; but as Hanwell was opened in 1831, and only 1,179 out of 2,029 admitted, had been discharged on the 30th September, 1840, the 6.7 years is probably nearer the true mean duration.

In determining the mean term of treatment, which was attempted in the early part of this paper, it may at first sight seem that the years of residence should have been divided by the mean of the numbers admitted (2,029), and discharged (1,171). This method would make 4.8 years the mean term of treatment in the six years ending 1839—for $\frac{3875}{801} = 4.8$; and 1.9 year the mean term of treatment in 1831-3; when 920 were admitted, 362 discharged, and the years of residence were = 1,248. Divide 1,248 by the number discharged (362), and 3.44 years would be the given term of treatment; but even this, as the experience of the six following years evinces, is much below the truth. The errors are the same as if the division of the children under three years old by the mean number of births and deaths, or by the deaths alone, were supposed to give the expectation of life at birth; whereas the division by the deaths at that early period, though the nearest approximation of the two, gives a less number than the expectation of life in years.

Dr. Conolly ascertained the previous duration of the disorder in 191 cases (exclusive of 10 congenital cases) admitted during the year; 66 had been labouring under the disease less than six months; 26 between 6 and 12 months; 24 between 1 and 2 years; and 1 had been insane 39 years. The mean previous duration was 3.4 years. But, as little more than *half* the number had been insane *twelve months*, the time of admission may be represented by 1, or by $1\frac{1}{2}$ year.

The mean age of 213 persons at admission was $36\frac{1}{2}$ years; the mean age of 195 at the time of the *first attack* of insanity was stated to be $32\frac{1}{2}$ years.

The *probable future duration of insanity* has been shewn to be $2\frac{1}{2}$ years at the time of admission; for, in $2\frac{1}{2}$ years, the 1,000 cases are reduced to 509. The chances that a patient will, or will not, remain insane $2\frac{1}{2}$ years, are 509 to 491, or nearly equal. Among those who remain insane half a year after admission, the probable future duration of the disease is nearly 4 years.

The *probability of recovery* at admission = $\frac{380}{1000} = .380$; of dying insane = $\frac{620}{1000} = .620$. Half a year after admission the probability of recovery is $\frac{272}{783} = .347$; of dying insane $\frac{511}{783} = .653$. The numbers in juxtaposition, in columns *b* and *c*, express the

respective chances of death and recovery; thus, $5\frac{1}{2}$ years after admission at Hanwell, the chances are 272 to 105 that a lunatic will *not recover*. All these probabilities depend more or less on the assumption that 88 in 310, remaining at the end of $7\frac{1}{2}$ years, will ultimately recover.

The *probability of recovery, or of dying*, within any year, or years, up to $7\frac{1}{2}$, is accurately shown by the table. In the first half-year the *probability of recovering* is $\frac{108}{1000} = .108$; the probability of recovering in $3\frac{1}{2}$ years is $\frac{380-127}{1000} = \frac{253}{1000} = .253$. Out of 1000 cases, 253 recover in that time; hence .253 is the probability of recovery. The *probability of dying* in the first half-year is = $\frac{109}{1000} = .109$; in the two years following $\frac{511-370}{783} = \frac{141}{783} = .180$.

From a table of this kind the lives of lunatics can be insured; and, from the present table, they may be insured for a limited number of years.

The table is an instrument by which the effects of treatment on the mortality, the number of recoveries, and the duration of all diseases, can be accurately measured. It enables us to compare two or three different plans of treatment, and to determine their effects upon the principal results at which all medical treatment aims—the reduction of the mortality, and of the duration of the disease. Thus if 139, of 509 lunatics that have been $2\frac{1}{2}$ years in Hanwell, will recover under the present treatment, and 200 recover under any new mode of treatment that may be introduced, the advantages of the latter would be obvious; and still more so, if the *probable duration* of the disease were reduced from 10 to 5, or 2, years.

The returns from the licensed houses do not state the ages; and the ages of a few lunatics are given in the interesting Report of Dr. Conolly. From other observations it is known, generally, that the mortality increases, and that the probability of recovery declines, as age advances.

The sex, age, and stage of the disease, are the principal internal causes that influence the mortality, except the form of the disease which, exclusive of congenital idiocy, may be, perhaps, reduced to an element already discussed—the “stage of the disease.” The influence of complications, of sex, and of age, may be assumed to be nearly the same in the licensed houses and Hanwell, as in ordinary asylums—the asylum, for instance, at Gloucester, where the mortality does not exceed 7 per cent. annually. The mortality of 7 per cent. may be fairly ascribed to insanity. The excess above this must be attributed to the diseases generated by the limited space in which the unhappy lunatics are confined; to the collection of large numbers under the same roof; the impurity

of the atmosphere; the want of exercise and warmth; the poor unvaried diet; and the deficiency of medical attendance*. But the influence of these agents can only be ascertained by a parliamentary inquiry; and it will not be denied that the causes should be investigated which raised the mortality of lunatics above the standard—57 per cent. among private patients, 71 per cent. at Hanwell, and 200 per cent. among paupers in the large licensed houses!

The Bethlem Hospital differs essentially from the Hanwell Asylum, as well as from the majority of the licensed houses, in the stricter selection of patients for admission. By the rules, the following cases are inadmissible:—lunatics who have been insane for more than twelve months; who have been discharged uncured from other hospitals; in a state of idiocy; afflicted with palsy, or with epileptic or convulsive fits; and suffering from any dangerous disease. Notwithstanding the instructions in the admission papers, the petitions of 58 out of 311 (19 per cent.) who applied in 1836, were rejected. The patients are not allowed to remain

longer than one year. 253 lunatics admitted in 1836 had been insane 83 days, on an average; 117 had been insane less than a month.

It would be exceedingly interesting to determine the mortality of this selected class of lunatics for 12 months. But, if dangerous symptoms come on at Bethlem, the patients are dismissed, when practicable, as *improper objects*. Thus of 3026 discharged in 10 years, 829 were dismissed uncured, 483 as *improper objects*, and 145 died. A great number of the “improper objects” would die soon after they left Bethlem; and their dangerous state, or supposed incurability, was the alleged cause of their dismissal. Paralysis, however slight, even of a finger, is the forerunner of death in the insane; and of 210 dismissed as improper objects (1831-36), 87 were paralytic, 59 “sick and weak,” 24 epileptic, 4 apoplectic, 2 had “fits,” and 28 were idiotic.

The lunatics at *Bethlem* are divided into three classes: “curables,” “incurables,” and “criminals.”

Curable Lunatics, 1827-39.

	Men.	Women.	Total.
Admitted	1168	1707	2875
Discharged	1145	1654	2799
Discharged as cured	545	961	1506
Improper objects	236	166	402
Dead	75	69	144
At request of friends	19	17	36
Uncured	254	422	676
Leave of absence expired	16	19	35
Out on leave of absence	49	73	122
Years of life	637	1002	1639
,, (exclusive of those out on leave of absence) }	1371

21 of the lunatics on the list of the hospital were constantly out on leave of absence; and during the 13 years, 122 individuals were discharged as “out on leave of absence.”

Of 100 “curable” patients discharged, 54·5 were cured, 5·2 died. The mean term of treatment was ·586 year, = 7 months; or ·49 year, = 6 months, if the time spent out of the hospital, on leave of absence, be excluded. The lunatics discharged as “im-

proper objects” were 14·5 per cent.; a considerable portion of whom would have been numbered with the dead if they had remained.

The *annual* mortality was 8·8; the recoveries 92 per cent.; 24·5 per cent were discharged as improper objects; 43·4 were discharged uncured; 2·1 were out on leave of absence. 171 were discharged annually out of a constant population of 100.

If the deaths which occur among those out on leave of absence are not recorded, the annual mortality to 100 resident in Bethlem is 10·5.

Incurables.—72 “incurables” were admitted; 72 discharged (33 men, 39 women), and the average number resident for 13 years

* The diet and the condition of lunatics at Hanwell have been latterly ameliorated very considerably by the Visiting Justices, at the suggestion of the present accomplished Physician; and the mortality may be expected to be reduced in proportion. It is also right to state that in some licensed houses the mortality of private patients does not exceed 7 per cent.

was 64·2. The years of life were therefore = $64\cdot2 \times 13 = 834$. Nine incurables were cured, 39 died, and 24 were discharged at the request of their friends.

Of 100 cases 13 recovered, 33 did not recover, and 54 died. One in 11·6, = 6 per cent. were discharged annually; the mean term of residence was 11·6 years. 1 in 21, = 4·7 per cent. died, and 1 per cent. was cured annually.

"Incurables" is an improper term; but it is a recognition of the law that recovery is infrequent in advanced stages of insanity.

Criminals.—In the 13 years 71 criminal lunatics were admitted at Bethlem (56 men, 15 women); 51 were discharged, namely, 26 died, 2 escaped, and 23 recovered. The average number resident was 57·3, the years of life 745.

Of 100 cases, 45 recovered, 51 died. The annual rate of mortality was 3·5, of recovery 3·1 per cent.; the mean term of treatment deduced from the years of life, and the number discharged, was $14\frac{1}{2}$ years. The numbers admitted and discharged in the 11 years (1827-37) were nearly equal (36 and 39); and the years of life divided by the number discharged = $\frac{653}{39} = 16\cdot7$ years.

It is evident that several of the criminals, such as Oxford, cannot properly be said to labour under insanity—in the sense of a disease. It is, if any thing, like idiotcy, a congenital misdevelopment of the brain.

The number of recoveries is considerable at Bethlem, but less than at some private asylums, notwithstanding the careful selection of cases. The mortality is reduced by excluding dangerous cases, and by dismissing the patients on the verge of death as "improper objects." It is difficult, under these circumstances, to account for the death of nine or ten in 100 annually, upon any other supposition than that the mortality is high at the early stage of the disease in Bethlem.

The last Committee of the House of Commons on Lunatics, stated in their report, "It has been clearly established in evidence, that there is no due precaution *with respect to the certificates of admission, to the consideration of discharge, or to the application of any curative process, to the mental malady**." Lunatics under confinement, it should be well recollected, are *prisoners*; and every one will admit that the depriving a man of his personal liberty, or turning loose a lunatic on society, are acts involving great responsibility,—a responsibility which, if it exist at all, is very imperfect in the present state of the law. In order to deprive a lunatic of his estates, a formal inquiry is publicly instituted; but a person who has been seven days chargeable to the parish may be committed as a lunatic

to the County Asylum by two justices of the peace on the certificate of *any* physician, surgeon, or apothecary, asserting that the "said person *appears* to be insane of mind." 2,780 pauper lunatics are confined under these certificates in the county asylums*. But there are 1,389 lunatics, and 7,007 idiots, "under the care of the parish officers as in-door or out-door paupers." Many of them are necessarily under restraint, without either warrant or certificate; which is only required when the parishes think it necessary to send them to a public asylum, where their treatment costs two or three times as much as the workhouse fare.

Paupers may be sent to licensed mad-houses by a justice, or by the officiating clergyman and overseer, with one medical certificate; and other persons may be sent to a licensed house by any layman, upon the certificates of *any two* medical men†. It appears also that by law, any person whom the governors choose to admit as a lunatic, may be confined at Bethlem, or St. Luke's Hospital, for an unlimited time.

The liberation of persons in confinement as lunatics, takes place under no better regulation. Medical visitors have been appointed, in the words of Lord Lyndhurst, "to see that the Chancery lunatics are well cared for, but above all *to watch the least glimmering of returning sanity, and see that the parties are not detained one day longer than necessary.*" The relatives, parish officers, proprietors, justices in petty-sessions, and the Metropolitan Commissioners, release lunatics from the licensed houses; but the mode in which this is effected is by no means satisfactory. "When once," says Colonel Sykes, "they (pauper lunatics) get shut up in a mad-house, it is indeed difficult for them to regain their liberty‡." Lunatics are discharged at the discretion of the visiting justices from the county asylums; by the governors from Bethlem, St. Luke's, and other hospitals supported by subscription; and by the parish officer from workhouses.

Many cases of abuse have occurred under the present system, which will be probably thought by the Society to require extensive alterations. And although there would be much difference of opinion on many points, all will probably agree that *no person should be placed under restraint as a lunatic in asylums, hospitals, or houses of any kind, who has not been examined by a public officer, practically acquainted with insanity.* I would therefore suggest that by some modification of the present system of inspec-

* Return to the House of Commons, 5th July, 1836.

† 9 Geo. IV. c. 40; 2 and 3 Will. IV. c. 107; 3 and 4 Will. IV. c. 107.

‡ Journal of the Statistical Society, vol. iii. p. 146.

tion, the circumstances of every lunatic confined should be investigated personally by a crown officer, and recorded previous to committal, at the expiration of every quarter of a year after admission, and at the time of dismissal. The sex and age, the stage, form, and complications, of insanity, should be registered, on entering and leaving the several institutions, by impartial officers. This would be a protection to lunatics, and to the public; the deaths and recoveries would be registered on a uniform plan, and an invaluable statistical check on the results of treatment would be obtained.

We may then discover the causes of insanity, the laws which regulate its course, the circumstances by which it is influenced, and either avert its visitations, or mitigate their severity; perhaps, in a later age, save mankind from its inflictions, or, if this cannot be, at any rate ensure the sufferers merciful treatment.

SOME
PHYSIOLOGICAL REFLECTIONS
ON
THE NATURE AND TREATMENT OF
ANGINA PECTORIS,
AND OF ANALOGOUS STATES.

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[For the Medical Gazette]

[Continued from p. 183.]

Dr. Heberden's account of angina. Theory of the first attack; the idea of spasm; of the state of the left ventricle; of the right ventricle; of the aorta.—Cases: remarks.

OUR former introductory remarks concerning the heart in its exalted states of activity, and in its deteriorations as life advances, will be seen to apply to all forms of cardiac disorder, though in very various degrees. We may now approach more closely to some specific considerations.

It is, perhaps, impossible to find a simple history of angina, unless we should take some single recorded instance: but this remark is introduced, to explain, in a measure, the obscurity and confusion in which the subject has been involved by the additions made to the characters, without any distinction of cases, by many successive authors.

The following description of angina pectoris, which was first especially noticed and named by Heberden, is

extracted from his Commentaries on the History and Cure of Diseases, as presenting a plain account of the affection, and a proper introduction to some remarks as to its nature and pathology.

“There is a disorder of the breast marked with strong and peculiar symptoms, considerable for the kind of danger belonging to it, and not extremely rare, which deserves to be mentioned more at length. The seat of it, and sense of strangling and anxiety with which it is attended, may make it not improperly be called angina pectoris. They who are afflicted with it are seized while they are walking (more especially if it be up hill, and soon after eating) with a painful and most disagreeable sensation in the breast, which seems as if it would extinguish life, if it were to increase, or continue; but the moment they stand still, all this uneasiness vanishes. In all other respects the patients are, at the beginning of this disorder, perfectly well, and in particular have no shortness of breath, from which it is totally different. The pain is sometimes situated in the upper part, sometimes in the middle, sometimes at the bottom of the os sterni, and often more inclined to the left than to the right side. It likewise very frequently extends from the breast to the middle of the left arm. The pulse is, at least sometimes, not disturbed by this pain, as I have had opportunities of observing by feeling the pulse during the paroxysm. Males are most liable to this disease, especially such as have passed their fifteenth year. After it has continued a year or more, it will not cease so instantaneously upon standing still; and it will come on not only when the persons are walking, but when they are lying down, especially if they lie on the left side, and oblige them to rise up out of their beds. In some inveterate cases it has been brought on by the motion of a horse or a carriage, and even by swallowing, coughing, going to stool, or speaking, or any disturbance of mind. Such are the most usual appearances of this disease; but some varieties may be met with. Some have been seized while they were standing still, or sitting; also upon first waking out of sleep: and the pain sometimes reaches to the right arm, as well as to the left, and even down to the hands; but this

is uncommon: in a very few instances the arm has at the same time been numbed and swelled. In one or two persons the pain has lasted some hours, or even days; but this has happened when the complaint has been of long standing, and thoroughly rooted in the constitution. Once only the very first attack continued the whole night. I have seen nearly a hundred people under this disorder, of which number there have been three women, and one boy twelve years old; all the rest were men near or past the fiftieth year of their age. Persons who have persevered in walking till the pain has returned four or five times, have then sometimes vomited. The termination of the angina pectoris is remarkable; for if no accident intervene, but the disease go on to its height, the patients all suddenly fall down, and perish almost immediately; of which, indeed, their frequent faintnesses and sensations, as if all the powers of life were failing, afford no obscure intimation. The angina pectoris, as far as I have been able to investigate, belongs to the class of spasmodic, not of inflammatory complaints; for—

In the first place, the access and the recess of the fit is sudden.

2dly, There are long intervals of perfect health.

3dly, Wine and spirituous liquors, and opium, afford considerable relief.

4thly, It is increased by disturbance of the mind.

5thly, It continues many years without any other injury to the health.

6thly, In the beginning it is not brought on by riding on horseback, or in a carriage; as is usual in diseases arising from scirrhus or inflammation.

7thly, During the fit the pulse is not quickened.

Lastly, Its attacks are often after the first sleep, which is a circumstance common to many spasmodic disorders."—Chap. 70, *Pectoris dolor*, page 303.

Now, with respect to the term angina, we have no doubt but that it has been much too indiscriminately used. The sudden anomalous diseases of the heart cannot be all of one kind; indeed the early and simple attack, and the later and complicated paroxysm, are widely different, and it is equally certain that one specific state, with a serious train of symptoms in a strong heart, may give place to another in a

wasted organ, and thus become the cause of death. If we desire to know what to seek, and where to look, for the explanation of different cases, we must regard the relative importance of every part, the condition of each, and its burthen in comparison of its powers of resistance or contraction.

It is almost unimportant, perhaps, to enforce the fact, that Heberden's notice refers to widely opposite states: here the pulse unaltered, there actual fainting; in one case the sufferer may exert himself to produce several attacks in succession; in another, he is seized in bed, or upon the occurrence of very moderate mental excitement. The pains, the feelings of distress, and the characters of the patients, are equally various: boys and women, adults and old men, will hardly allow us to reason in the same train for all cases.

In the primitive attacks of angina in a person said to be healthy, we find little or nothing more than the full exercise of the circulatory and respiratory functions impeded for a moment—embarrassed, as it may seem, by the failure of some particular part. We place the fault in the left ventricle and its orifices. The lungs experience a transitory turgescence, which is at once relieved by standing still; as the venous valves and right heart are thus left at ease, and the left ventricle and aorta, which are also directly relieved by the comparative suspension of muscular exertion, are allowed to regain a capacity which admits of more easy play, and complete action.

Why the subsequent attacks are successively more slow in subsiding, is to be explained probably by reference to the very same opinion, that we should advance to account for the more ready production of the attacks in the later periods of the disease. It seems natural enough that the defective parts, whatever they may be, should become more and more deranged permanently, and also more susceptible of actual disturbance, as by incompetence when overtasked, and thus, in fine, less and less able to regain or return to their proper functions with facility.

Some may be inclined to inquire, can it be, as spasm is produced in young and muscular persons by exertion (as in the *gastrocnemii*), that so the ventricle may become contracted spasmodically?

We are very unwilling to suspect this, for the affection does not seem ever to be accompanied by spasm of any voluntary muscles, nor, indeed, is the age of angina that of muscular spasm or true cramp, and still less can we suppose that the subsequent attacks occur in an organ susceptible of such contractions. If the disorder were of this kind, it would hardly manifest itself as it does: not to be at once fatal, to be relieved by resting, and to be capable pretty constantly of other and better explanations (as we think), are excuses enough for a brief notice of spasm at this time. But, assuming the general fact of the exalted state of the heart's functions, previous to the first anginal seizure, the free diastole and systole of ventricles, and fullest play of the valves, let us inquire which is the part to be most suspected of failure. The first reply would be, surely, that part which is originally the weakest, or which may be already the most defective, or which possesses the least power of regulating its own efforts; and all this, as we suppose, points to the aortic valves. The force which might over-distend the left heart depends on the right ventricle, and is in a measure regulated by the lungs; the power which dilates the aorta and its orifice is the over-excited left ventricle.

As to the right heart, we cannot here enter upon its peculiarities; and as to the mitral valve, we are not unwilling to admit that it may suddenly become imperfect, and especially supposing the aorta has first yielded to excess. If the affection were located in the right heart, we should rather expect it as the sequel of sudden exertions and obstructed breathing, than of the state of second wind. But the fact of a safety valve in this part is a sufficient objection to the application of the views which may be contended for with respect to the left heart, where the valves are naturally strong and close, although they may suddenly become inadequate. Imperfect valvular action on the right side is relief; on the left side, especially if sudden, it is almost death. Supposing, however, the full play of the thoracic organs to be brought about steadily, it is evident that the less the reflux on the right side, the more the efforts of the lungs and left ventricle would be required.

When the views of a safety-valve

function in the right ventricle come to be considered with attention, there will be no need of much argument to show that the right heart, at times, has a very close and efficient propulsive action; but it may require some additional reflection to show that, with gradually increasing calls to exertion, and only moderately increased distension, the force and precision of this part of the circulation is still uninterrupted by reflux, and especially if the cavity be well nourished, as in the prime of life, or before the due proportions of this period are materially altered.

On the whole it is most manifest that, up to a certain moment, all the circulatory actions of a propulsive kind may be unrestrained and copious. We would suppose no need, or at least the smallest effect, of a safety value. We would not allow that disturbance in the right ventricle should make itself felt in the way of angina. We put the difficulty in the left ventricle. Does this cavity become suddenly inefficient, and primarily, thus involving the mitral valve, &c.? We are not inclined to this as a common explanation, with respect to a strong or healthy ventricle.

If, however, it be possible for any state to be brought about by which particular parts may be rendered inefficient, we shall leave it to others to show what is so likely to occur as the undue yielding of the aorta*, and the consequent incompetence of its valves, under the impulses of the blood from the most powerful cavity, in its most violent efforts, the arterial system being at the same time subject to varying degrees of obstruction.

Of course we are here considering only the first onset of the active form of angina, or rather of one single form.

The observations of the most severe and certain disorganizations of the aortic valves we conceive to be sufficiently corroborative of our present opinions. These are cases of marked distress, and differ chiefly from angina in being permanent, and more speedily or certainly fatal. In angina the heart more or less suddenly receives an increased quantity of blood, and is oppressed. In the original cases of retroverted valves, recorded by Dr.

* There may be a case in which the mere dilatation of the aorta gives rise to pain.

Hodgkin in the *MEDICAL GAZETTE* (vol. iii. p. 443), or at least in the most marked of these, the characters of angina are but little concealed. The left ventricle being permanently oppressed is to be compared to the same cavity in the end of old angina, or in the state of the weakest heart.

The following case of angina will serve to illustrate the defective condition of the aortic valves. It occurred under the observation of Mr. H. H. Handey, of Upper Stamford Street, to whom we are indebted for the history.

Mr. A. F. M., æt. 47, a spare and leucophlegmatic person, had an injury eight years ago, which was supposed to have fractured the spine; and five years back he suffered from a fracture of the skull. He was subject to flatulence and dyspeptic symptoms, with palpitation, and a pulse which sometimes intermitted about every eighth beat. On ascending a hill in June 1839, he experienced his first anginal attack: he was obliged to stop, and ultimately return home. Some brandy and water seemed to relieve him. The second attack was three months afterwards, and the pain was more acute. About a fortnight later, after sitting up two nights and days with a patient, another paroxysm came on; he was bled to the amount of sixteen ounces, and went to bed; but at last became so ill that he was obliged to be conveyed to his own house. On the second day Mr. Handey saw him in the morning, a paroxysm having just passed off. His countenance was anxious, and the pulse feeble. A draught of warm water appeared to have given him relief. In the evening Mr. Handey was called to him in a fresh attack; the countenance was expressive of great agony; the pulse quick, but feeble; and he complained of intense pain across the chest, extending to near the insertions of the deltoid muscles, with sensations of choaking, and of palpitation, and also of a feeling of coldness down the back, which he himself referred to the region of the descending aorta. He spoke of his lungs as if screwed in a vice. Some warm tea induced eructation; and he gradually grew easier. He took some rhubarb, and rhubarb with calomel, and continued easy for a week; after which, in one day and night, he experienced five several attacks, each lasting from five to five-and-twenty minutes.

Warm water, tincture of opium, and brandy and water, all seemed to relieve him temporarily.

At this period he was ordered

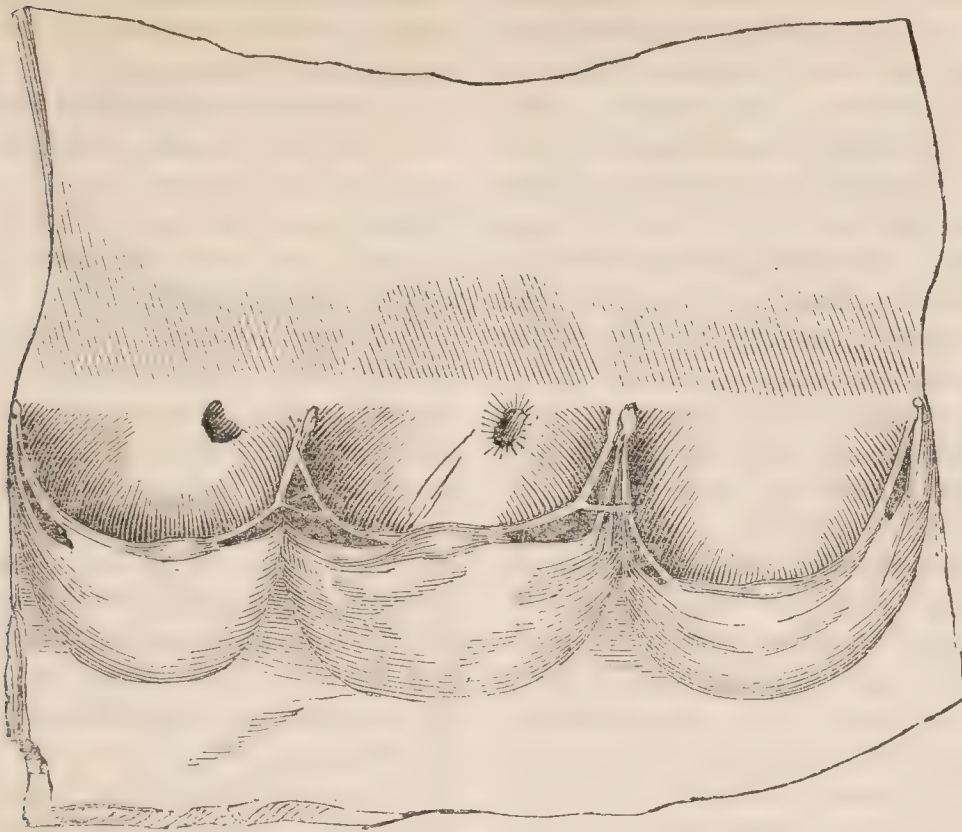
Pulv. Opii, gr. j.; Pil. Hydrarg. gr. iv.
Omni nocte sumend.

And a draught, to be taken twice a day, consisting of Magnesiae Carb. gr. xv.; Ol. Pimentæ, gtt. j.; Tinct. Gent. Tinct. Aurant. aa. ʒiss.; Aquæ, ʒix.

Good diet with bottled porter were likewise enjoined, and gradually the fits decreased in violence and duration, and were reduced to two in the twenty-four hours; and on the third day he travelled 136 miles to London, hoping that his native air might prove beneficial. One fit occurred on the journey, and for nearly a month he went on improving, with attacks diminishing in frequency and severity; but towards the expiration of this period, having spent a pleasant evening with a few friends, and retired in apparent health and spirits, he was heard to fall in his room, and was immediately after found lifeless.

The severity of this gentleman's attacks was such that all around him seemed to consider it matter of astonishment that he had lived so long.

The examination of the body was performed by Mr. Jonas King, nearly three days after death. The body was that of a tall narrow-chested man. The costal cartilages were soft. There was a considerable quantity of fat on the abdomen and chest. The lungs were pretty healthy. There was about a table-spoonful of bloody fluid in the pericardium. The heart was large, fat, and flaccid. Both ventricles were a good deal dilated: the right was of its natural thickness. The substance of the ventricles was firm and pale, and the act of cutting into the left ventricle, more particularly, elicited a sound like that produced by cutting cartilage with scissors, but in a very slight degree. All the valves were uniformly thin, except the mitral, which had two or three small patches of thickening. The aortic valves offered pretty well-marked corpuscula; and the upper parts of the valves were occupied by large perforations, angular and broad—one or two in either half of each valve. These seemed to take up nearly all the natural surfaces of contact, yet there were some partial traces of contact below them. The aorta was rather small and thin



Sketch of the Aortic Valves, shewing numerous and large perforations in those parts of the curtains which have been opposed to corresponding parts of the adjoining curtains. There may still be seen slight surfaces (or traces) of contact below the free edges, or perforations*.

throughout. In its lining, immediately above the valves, was a narrow line of opaque deposit, chiefly anteriorly. The coronary arteries seemed rather small. The stomach was healthy, and contained only a few small pieces of undigested matter. The liver was rather firm, but decomposing as well as the kidneys (which were soft), and earlier so than other parts. The spleen was large.

REMARKS.—It is to be observed, that the former injuries which this gentleman suffered had induced long rest to the whole body, and this may have been the cause of the perforations of the crescentic valves. The peculiar sense of thoracic constriction we may refer to the lungs in dependence on sudden impediment in the left heart.

We would by no means assert that all the pains described in this case were not from the physical pressure of the heart: the choking and palpitation may belong alone to the same state of distension.

With respect to the estimation of the

influence of remedies, it is most evident that posture and rest, the state of digestion, and of the general secretions, at the time of the seizures, must be taken almost momentarily into the account, as well as the tendency to spontaneous subsidence of the paroxysm, before we can form a judgment.

As to the fatal issue, we may suppose that cheerfulness was but an evil guide, whether we regard the inducements to indulgence, or the temptation to a little unusual activity of the limbs; the state of the stomach indicated plainly that digestion had just added its last to the circulating mass.

It is not our intention to assume positively that the aortic valves were concerned in the production of death, but we do not doubt that they were the cause of the primary attack, and we suspect of all.

The perforated crescents were the only object which we had previously directed attention to, and requested should be brought away.

At page 753 of the last volume of the *MEDICAL GAZETTE*, a preparation is described (No. 1396, in Guy's museum), and to this we may again refer. The aorta is much dilated and diseased; its valves are materially perforated, as in the foregoing case; and the ventricle

* Surfaces of contact indicate the quantities of surplus curtain and of ordinary valve. The surplus curtain is called into use when the great tube dilates, but if contraction of the membranes or perforations in their edges, or both, coincide with dilatation of the tube, the circulation is proportionably impeded.

is very much dilated and enfeebled. Some faintings preceded a fatal syncope. Here we may observe, that in proportion as the attack is easily produced, the painful sensations are not excited, the distension being but slight. It is not intended to advance these cases as proofs so much as illustrations of our meaning. The last approaches more to the state of feeble hearts, on which we shall not dwell at this time.

No. 1302, in the Guy's museum, is a "heart in which there is hypertrophy and dilatation of the left ventricle, and dilatation of both auricles, without adhesion of the pericardium, and little if any valvular disease. The patient, about 55 years of age, died very suddenly: he had long been affected with dyspnœa, but was not anasarcaous." Dr. Hodgkin has recorded this man's case; and I quote it for the sake of remarking, that the lacteal absorption was going on abundantly, which is an indication so far of health, while the sudden and unexplained death may well be supposed to have depended on the additions thus made to the burden of the heart or lungs.

In continuance we observe, that there is probably some degree of difference between every ulterior anginal attack and that which preceded it; but the contrast between the earlier and later attacks may serve to explain a very distinct form of the disease.

We cannot but suppose that, from the first, the disproportionate states of the heart are liable to increase; for instance, dilatation supervening on wasting of a cavity may be said to lay more than a double burden upon a part which is less than half as strong as formerly.

But now let us compare the tolerably sound heart of one case with the wasted and deteriorated organ in another. Is it not manifest that the debility of the ventricles may render them susceptible of being overwhelmed by distension from the slightest cause. Even in sleep, in the recumbent posture, when, perhaps, besides the disadvantages of the more easy access of blood to the right ventricle, digestion may still be adding to the accumulation of blood, and by slow degrees the ventricles are filled to a point beyond endurance, and the manifestation is sudden enough. When syncope is not to be feared, the erect

posture should tend to relieve this, by freeing both arteries and veins; the legs should hang over the bed, perhaps, and the muscular system be left to exert the least influence on the vessels. Of course, this is not a case to suppose distension of arteries, although they may be too full for the weak left ventricle.

We have had the pleasure of considering and discussing the topics before us with Dr. Joseph Ridge, who has very carefully and successfully investigated them. Dr. Ridge has collected some cases of sudden death in vigorous persons, from simple obstruction in the orifices of the coronary arteries; and it may be hoped that he will shortly publish his views of enfeebled and wasted hearts, as also of angina generally. For this reason, we have for the present avoided all particular reference to the absolutely failing heart, although it will be found that this is a topic on which our reflections are constantly verging.

We hope that already it may be seen that these kinds of reflections will help to explain many symptoms. And they do not derogate from the fact, that although distension is an essential condition of almost every case of which we treat, the chief inconvenience may be in different parts, and in different forms. It is evident that when the heart is most susceptible of disturbance, the patient's merely turning cautiously on his bed may so impede the aortic circulation, or drive on such an additional quantity of venous blood, as to induce a paroxysm. On the other hand, we may enforce a strong contrast in the case of an organ whose disturbance is set up with much difficulty, and with difficulty maintained or aggravated.

The gigantic farmer, mentioned by Dr. Forbes in a note, seized in good health with a simple anginal attack, set his horse into a canter, (an easy pace) let his arms hang down, and balancing himself with as little muscular exertion as possible, endeavoured, as he thought, to overcome the distressing disorder, while in truth the heart or aorta was endeavouring of itself to regain the limits of easy action, being already in a degree relieved by an easier motion of the body, and by gravitations, &c. This, at least, is our

explanation, and we can well say that this, and the preceding case, we have tried experimentally, and pretty thoroughly, with respect to dyspnœa.

[To be continued.]

ON
DILATATION BY FLUID PRESSURE
IN STRICTURE OF THE URETHRA.

To the Editor of the Medical Gazette.

SIR,

I SHALL feel obliged by your inserting in the next number of the MEDICAL GAZETTE, the following description of instruments used in stricture of the urethra.—I am, sir,

Your most obedient servant,

JAMES ARNOTT.

21, Fitzroy Square, London,
May 8th, 1841.

Although our knowledge of the pathology of stricture of the urethra has been much extended by the labours of Hunter and others, the treatment of this very common and distressing disease differs at the present day in no very material circumstance from that which was followed two hundred years ago. In the works of Wiseman, published in the reign of Charles II., the various practices now had recourse to will be found described. He mentions the use both of metallic and soft bougies; the application of caustic is noticed, a practice which was revived by Hunter; and cases are related in which the operation of opening the urethra behind the stricture was performed, instead of puncturing the bladder—an expedient of which the late eminent Sir Astley Cooper has been deemed the original proposer.

Unfortunately, this stationary condition, during the progress of almost every other department of surgery, has not proceeded from the treatment of stricture having attained perfection. On the contrary, it is acknowledged to be an opprobrium of the art. The means employed are admitted, by conscientious and intelligent surgeons, to be, in almost every case, but palliative; and although stricture may generally be much relieved by such means applied from time to time, it cannot be denied that the irritation which accompanies organic changes in a part of so much

sensibility as the urethra, will, by long continuance, often produce other disease in the neighbouring organs of the urinary and generative systems, which is sure to embitter, if it does not shorten, the life of the sufferer.

It is now many years since I introduced to the profession an account of practices in the treatment of stricture which I had had sufficient experience to recommend as substitutes for the very imperfect and sometimes hazardous measures in common use. But because the apparatus recommended was of rather a complicated description, as compared with that usually employed, and because part of it was constructed on mechanical principles, with which surgeons generally were not familiar, it has either not been used at all in this country (where the French modifications of plans of treatment I had proposed in impervious stricture, and of a new method of applying caustic, are almost unknown), or in so imperfect and erroneous a manner, as to disappoint expectation.

The purpose of this paper is to describe a modification, which I have lately contrived, of the instrument employed in the dilatation of stricture, combining the essential requisites, for general use, of simplicity of construction and easiness of application; and I cannot doubt, from its great and manifest superiority over the means commonly had recourse to, in the degree of relief afforded by it, and the safety and quickness with which this is obtained, that it only requires to be known to be immediately adopted.

Dilatation of stricture has been effected in two ways: by instruments which operate on the principle of the wedge, opening the constricted part as they advance in the canal, of which description are bougies and sounds; and by instruments which are themselves capable of distension, and which, by being made to enlarge in diameter whilst within the stricture, exert their dilating force from the centre directly outwards, or, as it may be termed, eccentrically. Amongst the principal advantages of eccentric dilatation over that of the wedge, when effected by a proper apparatus, are—that instruments so operating, having no tendency to stretch or tear the urethra in front of the stricture, by pushing on the stricture after having passed partially

through it, the surgeon is enabled by their means to use greater force (if required) with safety, than with rigid bougies and sounds, which have this tendency; that there is no danger, from the opposition to the passage of the instrument being erroneously attributed to the stricture instead of the wrong direction of its point, of the surgeon's piercing the side of the canal, and causing effusion of urine, or false passage; that the dilatation being effected without irritation from friction, and following the yielding structure, may be rapidly made; that the whole of a long stricture is dilated at once, or several strictures are acted upon at the same time, instead of the action being nearly confined, as in the case of the bougie, to the front or face of the first stricture; and that, from the power of enlarging the instrument in the interior of the canal to any size, the dilatation of the diseased part may be carried to any greater extent than the diameter of the outer orifice of the urethra, so as to afford the best means of effecting a permanent cure.

The apparatus used for dilatation, on the principle just explained, consists essentially of a strong membranous tube of fixed dimensions, which is placed, in its empty or collapsed state, within the stricture, and then injected with fluid. I have used such a fluid dilator, with various modifications, according to peculiarities of cases. The form of instrument most easily constructed and applied, and which has not as yet been described, is merely a varnished silk tube of the required diameter, and of a length to extend from the orifice to a little beyond the stricture, closed at one end, and having a small metallic connecting piece at the other, into which the injecting syringe may be screwed. This tube, by means of a slight coating of waxy composition, is, for the purpose of passing easily, rolled into the form of a common plaster bougie; and when it is not required to be of very small diameter in its collapsed state, the requisite stiffness may be given to it by rolling it upon a small catgut or stilet. A woven silk tube properly varnished would be perfectly water-tight; but this is of less importance, as a thick mucilaginous liquid will not escape but very slowly from a very imperfect tube made by sewing together the edges

of a riband. This instrument, which may be described as a dilatable bougie, is as durable, and may be made at as little expense, as any instrument used in the treatment of stricture.

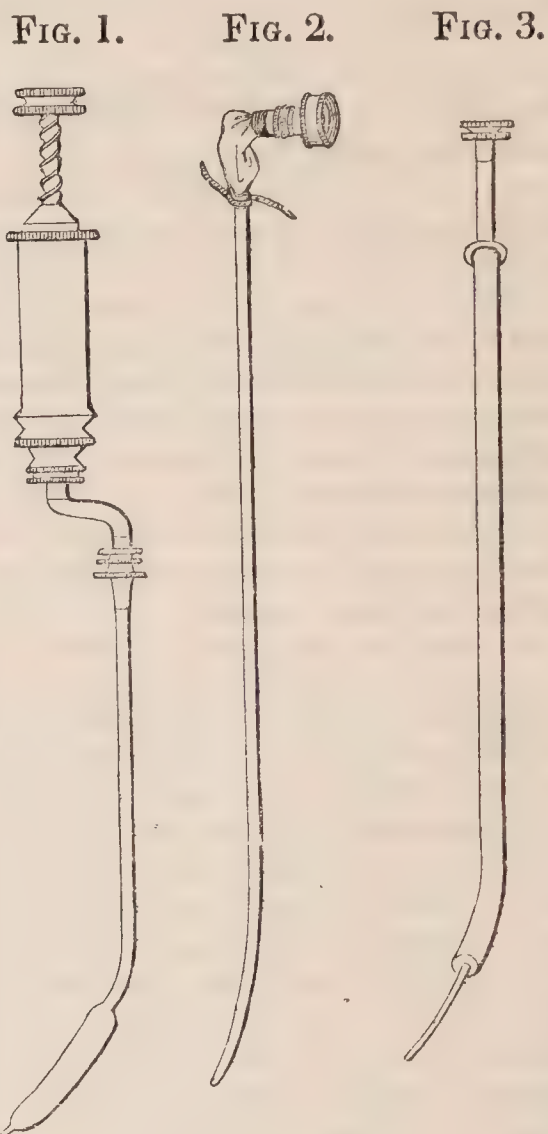


FIG. 1. A dilator with a membranous distensible tube of greater diameter than the orifice of the urethra. It is injected by a syringe having a piston-rod in the form of a screw, which turns in a depression on the top of the piston. The syringe is connected with the metallic tube of the dilator by a flexible tube of material impervious to air, or any thick mucilaginous liquid injected with it.

FIG. 2. A dilator consisting of a long varnished silk tube closely rolled into the form of a bougie; there is a small screw at one end to connect it with a common injecting syringe of half an inch calibre, and the other is closed by several turns of strong silk, which may be secured by different obvious methods. When, for the purpose of rigidity, the silk tube is rolled upon catgut or metallic stilet, it is necessary that precautions be taken against these becoming loose in the passage; the catgut by some threads between its upper end and the tying of the silk on the screw, and the wire by being brought through the screw and bent. A piece of cord is represented in the figure tied loosely upon the upper part of the tube. This retains it in its compact form.

FIG. 3. A conducting tube from which a fluid dilator is seen protruding. It consists of a short silk tube rolled upon itself into less bulk than that of the smallest bougie, and tied upon the end of a narrow tin tube. When a long silk tube, imperfectly varnished, is passed through a conducting tube, this will, if narrower than the dilator in its distended state, prevent the escape of liquid from the part which it encloses.

In keeping up distension, by means of a dilator rendered impervious to fluid by gut or caoutchouc, instead of the stopcock recommended in former instructions on the subject, a contrivance may be employed for fixing the piston of the syringe, when the required degree of pressure has been made, as by a cord passing through the ring at the end of the piston rod, or by a screw. When the piston is depressed by a screw (which may constitute the piston rod) the patient can himself increase or moderate the pressure with the greatest facility. If the distensible tube be made of strong silk, it may be thus gradually distended until it becomes as hard as a cylinder of wood. A connecting flexible tube of silk and caoutchouc between the metallic part of the dilator and syringe, prevents any jerking motion of the instrument in the act of screwing on the syringe, and is a convenient index of the degree of pressure applied.

In other applications of the fluid dilator, as in the cure of stricture of the rectum, and in the operation of slowly dilating the male or female urethra for the extraction of calculi, a long connecting tube of this description, bringing the screw which regulates the pressure conveniently to the hand of the patient, would render the apparatus very complete. I have shewn, in the appendix to the late edition of my work on Stricture and Stone, that the advantage of *slow* dilatation of the male urethra must have frequently occurred to the operators by the Marian method, who professed to follow nature in their proceedings, as it has occurred independently to several surgeons of late years, (see MED. GAZETTE, Dec. 25, 1830); but that the want of any instrument which could fulfil the indication must have prevented the success of any attempt of the kind. The equable, elastic, and controllable nature of fluid pressure, makes a dilator, judiciously constructed on this principle, incomparably superior to any other means that has been employed for the purpose; and furnishes us with a method of extracting urinary calculi, which, if I am not much deceived, will soon supersede the present painful and dangerous operations.

When stricture is to be dilated beyond the diameter of the orifice of the urethra, it is necessary to modify the instru-

ment which has been described above. The distension may be confined to the diseased part by placing a wide silk tube within another shorter tube of smaller diameter, or by passing it through a wide silver or elastic tube previously inserted as far as the stricture. In cases of very narrow stricture, only admitting instruments of the smallest size, a dilator may be passed through such a conducting tube, consisting either of a single or double piece of narrow gut dried in a compressed form, or of a silk tube rolled upon itself, and rendered sufficiently rigid by means of thick mucilage. It is unnecessary in these cases to have a distensible tube of the whole length of the conductor; a small bit tied upon the end of a long flexible tin tube, connecting it with the syringe, is sufficient.

In mentioning this mode of conveying a small instrument to narrow strictures by means of a conducting tube, I am led to notice a controversy which has been continued through several late numbers of the MEDICAL GAZETTE, respecting the invention of what has been termed "the compound catheter." Dr. Buchanan, who claims the originality of this proposal, does not appear to be conversant with the modern French writers on urinary diseases, or he would have found that the plan of conducting small instruments through others of larger size is noticed in most of the works on that subject which have appeared in France during the last twenty years. But it is contended that Dr. Buchanan's instrument is more than a mere modification of former suggestions; that the principle of it extends further: the smaller instrument, it is said, is not only conducted, but a way is prepared for it through the stricture by the dilatation effected by the pressure of the ends of the outer canulæ. Had a reference been made to the work from which M. Ducamp borrowed so liberally, instead of the treatise of M. Gerdy, who in this matter professes merely to follow Ducamp, it would have been discovered that there is no greater novelty in this idea of previous dilatation than in that of conducting. In my Treatise on Stricture of the Urethra (p. 133, 2d edition) the following is mentioned amongst other means to be resorted to in cases of difficulty:—"The plan which I have recommended above, of

passing a large canula (which in this case has a rounded end) enclosing an instrument down to the stricture, is very applicable here; pushing the canula against the stricture opens it, while the small bougie or catheter within is ready to be passed through."

ON THE
STRUCTURE AND USES OF THE
LINGUALIS MUSCLE,

AND ITS RELATIONS TO THOSE OF THE
PANNICULUS CARNOSUS.

BY JAMES MERCER, M.D. F.R.C.S.E.
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[*For the Medical Gazette.*]

IN man, and some others in the higher classes of animals, the tongue seems to serve in the function of taste alone; and on investigating the structure and conditions of its upper surface in these animals, this function can alone be assigned to it.

In taking a review, however, of its uses, in the various classes of the vertebratæ, we cannot be justified in considering it as only an organ of taste in all of these. Even in some of the highest classes—the ruminantia and the felinæ—it principally serves for the prehension and taking in of their food; and it is at least very doubtful whether it possesses the sense of taste in several others: although, on the contrary, we would not be warranted in denying the existence of this sense in these animals, nor even in such as are entirely destitute of the organ, as the function can be performed by other parts; it not being the effect of a special and limited organ, but a property of the mucous membrane lining the whole of the cavity of the fauces.

It has been long known, that in most of the herbivorous mammalia, particularly the ruminantia, independent of the epithelium, the dermo-mucous membrane covering the upper surface of the tongue is of very firm and dense texture, and that it forms, as it were, appendages to itself—what the hairs, bristles, and quills, are to the surface of the body; numerous strong pointed papillæ, imbricated on each other, in a direction from the tip to the base of the organ.

In many of the felinæ, the marsupiatæ,

and the bat tribe, the same peculiarity of structure takes place.

In several of the mammalia, the organ is not only used in the turning of the food from side to side, and for aiding in the primary part of the act of deglutition, but it also very materially, nay in some very essentially, aids in the collecting of the food; whilst it at the same time serves the purpose of a comb or rasp for the animal cleaning itself. The ox, for instance, with his free tongue, rolls the herbage of the meadow into a kind of tuft, before he tears it through with the lower incisors, and palate; and he also rasps and cleans his own coat or that of his companion with its rough and prickly surface. In the dog and cat it is the sole agent, by means of which the fluids are conveyed to the mouth, by that action of the tongue called "lapping;" and, in short, in every animal using the organ in a similar manner, we have the papillæ on its upper surface correspondingly arranged and developed.

In all these animals also the muscular lamellæ of the lingualis are well developed, and very strong, immediately beneath the dense investing membrane of the tongue; and taking this fact, with the condition of the papillæ on its upper surface, with their evident mode of production and uses, it is curious that no other function should have been assigned to these muscles but that of "shortening the tongue and depressing its point,"—actions which we shall endeavour to show are only secondary, and the effect of another.

In the following remarks, therefore, we shall endeavour to point out more specifically what the real uses of these muscles may be; not only as these can be deduced from their anatomical arrangement and distribution, but also from analogical reasoning, in regard to their situation immediately beneath the epithelium and dermo-mucous covering of the tongue; serving to these structures, and their appendages, as we conceive, what the panniculus carnosus does to that of the cutaneous textures on the greater part of the surface of the body, viz. the erection or setting on end of these appendages, the papillæ.

The upper surface of the tongue in ruminating animals, and in the dog tribe, is covered over by a very dense

and almost cartilaginous membrane, extending from the epiglottis forwards to the tip, and over its sides, where it becomes much changed in its appearance and structure, and takes on the nature of true mucous membrane.

Along the median line it is much thicker, and more dense, than at any other part, and appears in the dog, in the form of a distinct and rounded cord, commonly called the "worm of the tongue." This, by Caldani, was called the "*linea albescens**."

Posteriorly this membrane is intimately attached to that which extends between the base of the tongue generally, the epiglottis and the os hyoides, and, in structure, appears evidently to be similar to the yellow elastic tissue.

On making an oblique section of the entire thickness of this membrane, it appears to differ somewhat in its two anterior thirds from that which is seen in the posterior; and hence it has been looked on by Gerdy, as warranting two distinct terms—the "*membrane folliculaire ou muqueuse linguale*," and the "*tissu folliculaire linguale*."

The first of these, says he, "S'observe dans les deux tiers antérieurs de la membrane linguale, est grise, très-dense, fort résistante, cartilaginiforme, et comme si elle était tapissée par un lame cartilagineuse."

The second, the "*tissu lingual folliculaire*," est placé sous la muqueuse, très-mince de la partie supérieure de la base de la langue. Il occupe le tiers postérieur de la surface supérieure de celle-ci, environ tout l'espace qui est au-delà des papilles lenticulaires et coniques jusqu'à l'épiglotte, et on s'ouvrent les follicules si sensibles. Il adhère aussi très-fortement à la folliculeuse qui le recouvre immédiatement†."

This difference, however, appears to me to be more apparent than real; for, after the most careful examination of their minute structure in the ox and dog, no appreciable difference could be detected, and the only cognizable differences, the thickness and density, appearing rather to be dependent on the comparative absence of the papillæ on its upper surface.

On the two anterior thirds, where these projections most abound, there

the tissue is most dense and strong, and its sensibility less than in the posterior third, which is well known to be, next to the margins of the tongue, in the animals above specified, the situation where the sense of taste is most acute over the surface of the organ. This opinion is still more strengthened by a comparative examination of this investing membrane, with that which is found along and under the margins of the tongue. There the structure seems to be in every respect similar to the "*tissu folliculaire**."

Projecting from the upper surface of the whole of this investing membrane, we have a great number of papillæ, varying much in size, form, and arrangement. They are largest and most prominent in the middle and anterior thirds, particularly along the centre of each section, but more scattered along the margins. In their directions, when viewed laterally, and the tongue of the animal is kept in the bottom of the mouth, they appear to lie flat along the surface, but imbricated on each other from before backwards; their apices looking towards the epiglottis.

No sooner, however, is any sapid body, or any other stimulus, applied to the surface, than they are immediately elevated. When thus seen in the tongue of the dog or cat, they appear arranged in comparatively regular and transverse lines, the apices then being lifted up, and their posterior surfaces rendered concave, so that we have now a series of small channels, well adapted for containing and supporting fluids, placed in them, and thereby transporting them into the back of the mouth, during the pendent position in which the head of the animal is then placed.

In their structure, they are nearly similar to that of the investing membrane, and their adherent surfaces are intimately attached to the anterior extremities of the fibres of the superficial lingual muscles.

Immediately beneath this investing membrane, we find the irregular bands of muscular fibres, to which the term *lingualis* has been applied. These bands have been arranged by Gerdy into four sets—"the superficial and

* *Icones Anatomicæ*, Venet. 1804.

† Gerdy, *Anat. et Physiolog. de Langue*: *Archives Gén. de Médecine*, tom. vii. p. 363.

* See also Blandin, *Archives Gén. de Méd.*, 1823; also his *Thèse inaugurale sur la structure de la langue du bœuf*.

deep, the vertical and transverse lingual muscles*: but it is only the first of these which is the most conspicuous, and can alone act on the surface of the tongue and its papillæ. This muscle consists of two distinct lamellæ of fibres, extending from the base to the tip of the organ, and separated from each other by the *linea albescens*. In their general outline and configuration, they are slightly parabolic; their concavities looking towards each other along the median raphè, and their convexities to the margins of the tongue.

Posteriorly they arise by means of the glosso-epiglottic elastic tissue from the upper surface of the *os hyoides*, and from thence proceed forwards to different portions of the adherent surface of the investing membrane. The inner set of fibres are considerably the shortest; but they gradually become more lengthened as they are examined near the margins.

Their free or inserted extremities are undoubtedly into the whole of the lower surface of the investing membrane. This can be proved by two facts—first, by the roughened appearance of the dissection, after its most careful removal; this condition evidently depending on the severing of muscular fibres from it in their transverse direction; and secondly, from our inability to trace, even with a glass, any of the muscular fibres forming it, curving downwards to the base of the organ,—the general tendency of all the muscles of the tongue, the intrinsic and extrinsic, being towards its upper surface and tip. Immediately beneath these layers of muscular fibres, we find another layer of comparatively loose cellular tissue, surrounding the free extremities of the different muscles of the body of the tongue. This layer is very vascular, being freely supplied from the terminal branches of the lingual arteries. The chief use of this layer, as we conceive, being to enable the superficial lingual muscles to be freely and easily moved from before backwards over the surface of the tongue, unconnected and unrestrained by the influence of the other muscles, from which they are thereby completely separated. Analogously considered, it is similar in situation and use to the

adipose layer on the surface of the body, which separates the *panniculus carnosus* from those muscles which are in immediate connection with the trunk, and thereby confines its action to the common investing membrane and its appendages.

Having thus traced the fibres of the muscles, and seen that they are extended along the whole of the upper surface of the organ, and fixed posteriorly to the *os hyoides*, it will be easy to conceive that this latter must be the part whence it can become fixed before it can be called into action. Taking this, therefore, as its point of fixation whenever it contracts, either by the will of the animal, or by the presence of some stimulus, it becomes shortened in its length, and puckered, as it were, into transverse folds; and, it will also be observed, that, in consequence of the intimate attachment of the anterior fibres to the adherent surface of the investing membrane, this must also be shortened and wrinkled in a corresponding ratio.

No sooner, therefore, will this be effected, than its immediate free prolongations, the papillæ, will have their bases drawn backwards along with it, and with the natural consequence of lifting up and tilting forwards their apices; so that when the entire muscles are in a complete state of contraction, and the investing membrane shortened to its greatest possible condition, the whole of the papillæ on the dorsum of the tongue are raised up, and arranged, as already stated, in nearly regular undulating lines: serving thereby as so many tenter-hooks, for assisting in laying hold of the grass, in the herbivora; and forming so many hollow channels, or buckets, for lifting up, and containing safely—like the boxes of a circular chain-pump or dredging machine—the fluids, in such of the *felinæ* as take in this part of their food by the process of lapping.

The generally received opinion of the action of these muscles, viz. “of bending up the tip of the tongue,” can only, therefore, be a secondary effect, following the perfect evolution of the papillæ; but we doubt much if the muscles ever act in that manner, there being a sufficient number of more powerful muscles to perform it.

The situation, form, and uses of the *panniculus carnosus* are already per-

* *Recherches d'Anatomie, &c.* Paris, 1823; page 20.

fectly understood; and I shall therefore only allude to the great analogy that subsists between this muscle and the linguales now described. These latter muscles, the linguales, like the panniculus carnosus, are thin, broad, and lamellar; like it, it is also placed immediately beneath an investing membrane—that of the tongue, and intimately connected to it and its appendages; and, like that muscle, it is correspondingly developed where these appendages are large and well developed, and when the organ does not perform so much the office of a limited and special function; but rather that of a general kind. Is it not probable, therefore, that, its situation, connections, and varying peculiarities, being so strikingly similar, in its uses it will not also be similar?

ON STAMMERING,

AND THE METHODS PROPOSED FOR ITS
REMOVAL.

BY EDWIN LEE, ESQ.

[Continued from p. 306.]

[*For the Medical Gazette.*]

FEMALES, it is well known, are much more affected by nervous disorders than men. Stammering, however, presents this peculiarity—that it is rarely met with in women or girls. Of the 100 cases mentioned in Mr. Yearsley's pamphlet, not more than three or four are females. Of 300, treated by M. Colombat, 286 were males, and only fourteen females. Of between thirty and forty stammerers who presented themselves at M. Amusat's, only three were women; and other statistical accounts represent the number to be equally small. This circumstance has never been satisfactorily accounted for, and perhaps the only rational explanation that can be given of it is, that women have naturally a finer organization of the parts concerned in speech, with a quicker apprehension, and that they think quicker than men; hence their articulation is more easy and fluent, they are capable of greater volubility, and experience less hesitation in speaking. This is even found to be the case at an early age. When children are required to appear and speak in public,

as on the stage, girls are almost always found to be best adapted for the purpose. How often do we not see a boy become confused, stammer, and hesitate, on being questioned upon particular subjects, and how seldom in comparison does this occur with girls of moderate intelligence*. Probably, if inquiry had been made of all the females who had presented themselves for the cure of stammering, it would have been ascertained that in several of them the infirmity had been acquired by imitation, as it must evidently have been with the two sisters mentioned by M. Magendie, who both stammered badly, and whose mother was also affected. This appeared likewise to be the case in a female whom I have recently seen, and whom I questioned upon this point. She, as well as her sister, thought her stammer was owing to her having been accustomed to go to school with another child who stammered. At all events, the circumstance of the comparative infrequency of stammering in females would be of itself a sufficient refutation of the opinion of those who regard this affection as dependent upon physical causes; as enlarged tonsils, and other anormal appearances of the throat and tongue, are in all probability as commonly to be met with in the one sex as in the other. With respect to the comparative facility with which the majority of stammerers sing, it appears to depend partly upon the circumstance that the attention is concentrated upon keeping time and in the variations of the voice,

* "Little girls have the organs of speech more supple and flexible than boys; they speak sooner and easier, and women speak more agreeably than men. They are accused of speaking more: such ought to be the case, and I would willingly convert this reproach into praise. The eyes and the mouth have in them the same mobility. Always occupied in pleasing; observing, with the most persevering attention, every thing which passes around them; always expert to profit by their advantages; and reduced by the state of our society and manners, to shine only by singing, dancing, but especially by conversation, they give themselves up to these exercises with ardour, and excel in them more than men. The whole nervous system is also more developed in them; the impressions which they receive are more powerful and multiplied, and hence they have a greater number of sensations and internal feelings to make known; anxious to penetrate the secrets of men, and to ascertain the state of their hearts, speech is for them the most useful instrument, and the most indispensable to their happiness."—J. J. ROUSSEAU.

Here we have at once a rational explanation why stammering should be so rare, and other nervous affections so common, in females.

and partly that in singing the words run one into another more than in speaking, and the difficulties may be more easily eluded.

I will now briefly notice the opinions of some writers on the complaint, and the methods which have been proposed for its removal. The earliest instance on record of the cure of stammering is in the case of Demosthenes, who cured himself by reciting slowly, in a loud voice, long passages from the Greek poets, holding, at the same time, several pebbles in his mouth; and somewhat analogous methods have been found successful in more recent times. M. Itard proposed, in the year 1817, a sort of fork with two prongs, which carried the point of the tongue backwards, combined with the practice of speaking in a foreign language with which the person was not very conversant, by which means both the attention and the memory were exercised during the conversation; and it will generally be observed that stammerers, if required to read before strangers, will do so much better if they can take an interest in the book or paper presented to them. The attention being thus for the time diverted from the thought of their being observed. The plan of Itard, however, was not very successful: that of Mrs. Leigh, of New York, was more so, especially after the improvements of it by M. Malbouche, who introduced it into France. Mrs. Leigh considered that the difficulty of articulating depended on the circumstance of the tongue not being carried upwards, so as to touch the palate in speaking, she therefore made those under her care persevere in beginning each word by turning the tongue up to the palate; and between the years 1828 and 1830 had sent away from her establishment 150 persons cured. M. Malbouche superadded to this plan the drawing the lips backwards, so as to make the mouth appear larger, and when the phrase was terminated, the lips were again drawn back against the teeth, in order to recommence a fresh one. It was also required of the pupils to maintain a perfect silence between the interval of the exercises, and to practise declamation and reading aloud before attempting a familiar conversation, which was not permitted until the cure was considered to be effected.

M. Malbouche, however, divided

stammering into three kinds, according to the faulty positions of the tongue in speaking, and varied his method accordingly; but it would carry me too far to enter into the consideration of these varieties, which subsequent observation has shewn to be in great measure hypothetical. He says that the energy of the will is the most essential condition of success; and "that it is important to concentrate it exclusively upon the object to be obtained by the treatment. Children, and that class of men of the world who are accustomed to dissert upon and discuss every thing, without ever concluding upon any thing, are incapable of this concentration of the attention, and for that reason are difficult to cure: whereas experience has demonstrated that peasants, working men, and uneducated persons in general, are more easily cured. These individuals having but few ideas, seize with energy those which they can understand, and in which they are deeply interested." A want of confidence may cause any of the methods of treatment to fail altogether. In general those cures which are the most quickly effected are the least durable.

M. Serres divides stammering into two kinds: the first is characterized by a sort of St. Vitus' dance of the muscles concerned in articulation; the second by a tetanic stiffness of the muscles of the voice, and of respiration. In the first kind the will loses its influence over the rapid movements of the lips and tongue; in the second kind, the breath fails*. His method of treatment consists in making the sufferer breathe regularly, and combine his breathing with a certain cadence in speaking as in declamation and singing. Dupuytren likewise recommended stammerers to speak in a manner analogous to the recitative of operas, by which means much of the difficulty was obviated.

M. Rullier considers stammering to be a cerebral affection, and that it is attributable to some modification of the action of the brain. "But," says he, "in what does this modification consist? Without pretending to explain it, the following conjecture may be hazarded. In the stammerer, the cerebral irradiation which follows thought,

* *Mémorial des Hopitaux du Midi*, 1829.

and becomes the principle which induces action in the muscles which are necessary for the expression of the ideas, rushes out with so much impetuosity, and is reproduced with so great a rapidity, that it exceeds the measure of the movements which are practicable by the agents employed in articulation. Hence, as if suffocated by this accumulation of the ordinary exciting cause of their movements, they fall into a state of spasmodic immobility, and of convulsive shocks, which characterize stammering*.

This hypothesis is, however, not borne out by observation. That there is a want of accordance between the cerebral action which occasions the muscular movements and the agents by which they are effected, will, I think, be clear to all who have paid any attention to the subject; and that this is the essential cause of stammering I have endeavoured to prove, as well as the manner in which it takes place. M. Magendie justly observes, in refutation of the above theory, "I have seen many stammerers; and if I have met with some in whom the degree of intelligence appeared to be very great, I have likewise seen others in whom time was not wanting to the muscles for the expression of the ideas, which were any thing but abundant or vivid. Stammering is evidently a modification of the contraction of the muscles concerned in speech; and since by means of physiology we can give no explanation of this contraction itself, how can we expect to explain its various degrees? Without occupying ourselves with researches which can lead to no useful result, let us limit ourselves to remarks on the kind of contraction of the muscles which take part in the formation of speech, and of which the action is more or less altered in stammering. M. Magendie then continues in the words which I have already quoted in the first part of this paper.

M. Colombat, whose work is the fullest and most recent on the subject†, regards stammering as a nervous affection, the principal character of which is a repetition by catches, or convulsive shocks, of a greater or less number of syllables; or a painful and temporary

suspension of the voice before some consonants or vowels which require a certain effort for their articulation. He divides the affection into two kinds, which he terms *labio-choreique*, and *gutturo-tetanic*. These are subdivided into several varieties. The first kind consists in a sort of chorea of the lips, and in a succession of movements, more or less rapid, of the tongue and lower jaw: it chiefly gives rise to disagreeable repetitions of b, b, b, t, t, t, d, d, d, or q, q, q. "In the second kind the stammerer remains with his mouth half open, incapable of producing any sound; sometimes, even when the word begins with a vowel, the face and neck become swollen, and the jugular veins distended. In some individuals the tetanic state predominates, in others the choreal, though the two kinds are not unfrequently united in the same person. Thus a stammerer in beginning to speak is stopped at the first word, if it commence with a consonant which is difficult to pronounce; then the fear of not succeeding renders the breathing hurried, and he becomes dumb, like a person affected with the second kind of stammer. In both kinds the stammering ceases by making a full inspiration; but it will soon return if the person do not take care to breathe regularly, and recurs more or less frequently, or with greater or less intensity, according as the breathing is more or less agitated, and according as the moral emotions which he experiences tend to increase or diminish the act of the will. "Now," continues the reviewer, "let us try to teach the stammerer to breathe regularly: let us connect his breathing with a certain cadence, as is the case in singing and declamation, the stammer will not occur; and if the individual can subject himself to the same constraint in conversation, he will be able to speak without stammering. To this result the method of treatment of M. Serres directly leads; whereas the other methods only attain it indirectly*."

It is foreign to my purpose to enter into the details of these educational methods of treatment: I therefore refer those who are particularly interested in the matter to the work of M. Colombat. In order to enable those under his care to keep, during their exercises, a regular

* Dictionnaire de Médecine: art. Bégaiement.

† Traité de tous les vices de la parole, et particulièrement du bégaiement, 2d vol. 3d edition, 1840.

measure, or cadence, in speaking, this gentleman makes his pupils use an instrument for counting, termed the *muthonome*, by which the rhythm may be accelerated, shortened, or kept at the same degree, at pleasure. After having enumerated several of the means which he employs, M. Colombat says, "The combination of the orthophonic means which have been described, constitutes a vocal exercise which has the advantage of acting at the same time physically and morally. It acts physically upon all the muscles of respiration; on the lungs, the larynx, and particularly on the glottis, on the tongue, and on the lips; in fact upon the entire vocal apparatus."

"It also acts morally. Thus the measure which exerts so beneficially its influence over all the organs, by regulating their movements, fixes the attention of the stammerers conjointly with the other parts of my curative method, and becomes an accessory idea, which, joined to the principal one, must necessarily place the nervous action which follows thought more in harmony with the relative mobility of all the vocal organs."

The following are the results which M. Colombat obtained from the year 1827 to 1833. Of three hundred stammerers treated at his institution, and in town practice, two hundred and thirty-two were cured without relapse, thirty-two relapsed, in fifteen the complaint returned after a second treatment, and twenty-one were incurable. The average duration of the treatment was twenty-five days. Two hundred and sixty-five were adults, twenty children under twelve years of age, and only fourteen women.

One of the worst cases of stammering which have fallen under my observation was that of a Savoyard boy, who presented himself at M. Amussat's. The difficulty in commencing most phrases was extreme, and the attempts were accompanied with strong spasmodic actions of the face, neck, and upper extremities. M. Colombat took him out of the room, and brought him back in about a quarter of an hour, when, by attending to the directions he had received, and keeping time by striking the fore-finger of his right hand against the left, he could articulate with much greater facility. The tongue and the muscles beneath it were

very much developed, and contracted strongly on the introduction of the finger into the mouth. The operation of dividing the *genio-glossi* muscles was performed, which produced a considerable amelioration, though it did not effect a perfect cure.

After an examination and comparison of the different methods of the educational treatment of stammering, M M. Rullier and Itard remark that every measure which has been advantageously employed may be reduced to a sort of obstacle or moderator, which is opposed to the irregular, anormal, and embarrassed movements of the organs of speech. This moderator acts physically or materially; whether it be the pebbles employed by Demosthenes, the instrument for fixing the tongue of M. Colombat, or the fork of M. Itard. It acts also upon the tongue and other parts employed in articulation morally or mentally, by means of the attention, the will, the action of the memory, or the efforts at imitation. "The mechanism of speech," adds M. Itard, "is then performed under new conditions, associated as it is with certain voluntary movements, or with certain positions of the tongue, which are ordered, and which had previously been unconnected with the action of speaking. Such are the movements of the thumb upon the forefinger, or frequently repeated inspirations, as recommended by M. Colombat; the movements of the arm, as directed by M. Serres; and the different positions of the tongue, according to the precepts of Mrs. Leigh, and MM. Malbouche and Colombat. Such are likewise the exercises of the memory, and of imitation, the difficulty the stammerer experiences in endeavouring to learn, and in speaking only in a foreign language, or to adopt, in his familiar conversation, the emphasis of theatrical declamation*."

[To be continued.]

CASES OF SUDDEN DEATH.

To the Editor of the Medical Gazette.

SIR,

IF you consider the annexed cases possess sufficient interest for the pages of

* Op. cit.

your valuable journal, they are quite at your service.—I am, sir,

Your obedient servant,
GEORGE DOWNES.

Stockport, April 23, 1841.

I was desired to visit William Smith, about 11 o'clock in the evening of April 5th, 1841: he had been attacked with faintness whilst talking with the housekeeper, sat down on a chair, put his head upon the edge of the dresser, and suddenly expired. I saw him in a few minutes after the seizure, and found him quite dead: his fellow-servants had placed him on his back on the floor, and were applying friction to his chest and extremities, and ammonia to the nostrils. The face, lips, skin, and conjunctivæ, were pale, and free from congestion. On the following day the body and head were carefully examined, but not the smallest vestige of disease was found in any of the viscera that could at all account for his death, except the heart, which was empty of blood. The right auricle was much dilated. The right ventricle was flabby in texture, and covered with a layer of fat. The valves were sound. A section of the arch of the aorta exposed incipient steatomatous and earthy concretions.

Remarks.—The subject of this case was 40 years of age, a butler, and a temperate liver, apparently in previous good health. I conceive that Mr. Chevalier's theory of a failure in the capillary circulation will not be tenable in this case: there was an entire absence of stagnation of the capillary, and of congestion of the larger blood-vessels and lungs. It is also evident that the immediate cause of death was not in consequence of respiration becoming imperfect, but probably the effect of syncope, resulting from a depressed state of the vital energy and circulation through the brain, by which the action of the heart was suddenly arrested. Spasm of the heart could not account for the absence of congestion in the lungs and great blood-vessels.

CASE II.—On the morning of the 17th of April, 1841, I was called to see Rebecca M'Ghee, a soldier's widow, of a spare habit of body, aged 62 years, who the night before had been drinking egged ale, and brandy and water, in the company of her daughter and two

others, at an adjoining public-house. She was intoxicated, but was able to walk home, where her daughter left her, to obtain a light from a neighbour: on her return, she found her mother lying on the floor, perfectly insensible. She was assisted to bed, and in half an hour became sensible, so as to speak, and to get up to make water: she staggered, fell forward with her head upon the bed, and never spoke afterwards.

When seen next morning, she was lying on her back comatose, had a pale countenance, soft pulse, cold surface, contracted pupils, and stertorous breathing. She was cupped. Enemata and sinapisms were administered; and, in the afternoon, six or seven ounces of blood were drawn from the arm. The pulse sunk during the operation, and the symptoms continued unabated till her death, on the morning of the 19th instant.

On dissecting back the scalp, an ecchymosis was seen, the size of a half-crown piece in the centre, and above the occipital tubercle; and, on removing the calvarium, a coagulum of four ounces was found between the dura mater and arachnoid covering the right hemisphere of the brain. There was no laceration of its substance, nor was it possible to trace the source whence the extravasation had proceeded. Query?—did the effusion of blood arise from violence, or from natural disease? It is highly probable that the internal hæmorrhage in this case arose in consequence of the excitement from intoxication, and of the morbid predisposition to it from frequent attacks of headache and drowsiness; that the effusion took place when she first fell down, and gradually increased, so as to prove fatal by impairing respiration.

CASE OF OTITIS,

COMPLICATED WITH CEREBRAL DISEASE.

To the Editor of the Medical Gazette.

SIR,

SHOULD the following case appear worthy of a place in your journal, it is very much at your service.—I am, sir,

Your obedient servant,
GEORGE FIFE, M.D.

Newcastle-upon-Tyne,
May 11th, 1841.

E. T., æt. two years and three months, had for some time seemed unwell, being very listless, feeble, and drowsy. This last symptom existed to such a degree as to cause her frequently to fall asleep during the day, even when in the sitting posture. When walking across the floor she appeared for some weeks afraid to step out, and to have difficulty in maintaining the erect position. This was at one time, by her parents, ascribed to shoes hurting her feet. I, however, apprehended some spinal cause; but, on the most careful examination, could detect nothing corroborative of such opinion. The bowels had been irregular and disordered for some time, in consequence of which occasional alteratives and aperients were given. After continuing for some weeks in this state, and complaining of pain in her left ear, it gave exit to a fœtid, muco-sanguinolent discharge, manifestly attended with temporary relief. This was of short duration, as head symptoms became too prominent to leave any doubt as to the sensorium being implicated. Strabismus, convulsions, and coma, now took place, and continued till death put a period to her sufferings. It is enough to say, that the ordinary remedial measures, as leeches, blisters to the nape of the neck, calomel, turpentine enemata, and cold to the shaven scalp, were assiduously employed.

Autopsy.—Development of head peculiarly good; scalp attenuated and exsanguineous. Bony parietes thin. Dura mater perfectly healthy; the venous trunks engorged. There were very strong adhesions, over the left ear, between this membrane and the arachnoid, which on the right side was nearly natural; whilst on the left it was highly vascular, with some slight adhesions between it and the dura mater, near the longitudinal sinus. Pia mater free from any morbid appearance. Considerable serous effusion about the base of the brain. The substance of the brain, on a horizontal section being made, was found quite healthy. Serous effusion in considerable quantity in both ventricles. Foramen of Monro inordinately large, being capable of affording transit to the largest goose-quill. Slight softening of the anterior lobes of the brain, especially the right. Plexus choroides unusually pale and flaccid, exhibiting no sign of congestion. On examining the

petrous portion of the left temporal bone, it was found very soft, being easily penetrated by the point of a scalpel, but without any appearance of caries, or separation of its membranous covering. On being sawn into, the internal ear and cells were filled with pus, which exuded abundantly so soon as the saw penetrated the cellular structure. For the above very accurate examination I am indebted to my friends, Mr. Potter and Mr. Maugham, by whom the dissection was most carefully conducted, and who, at the time, regarded, along with me, the case as an excellent illustration of the complication of otitis with cerebral disease; in which opinion we conceived ourselves borne out by MM. Itard and Rostan.

Reflection on this case suggests an interesting and, in a practical point of view, not unimportant question; viz. was the disease primarily in the ear, and the brain, &c. implicated merely in a secondary manner; or was the brain primarily affected, and the ear ultimately took on the inflammatory action? This question is difficult of decision; but one which is well deserving of consideration. I shall now offer a few very brief remarks upon it.

That the complication is of common occurrence there can be no question with any practical man, who has had much to do with the care of infantile patients. At the same time candour will compel him to acknowledge, that in no case can it be more difficult to distinguish cause and effect: this, at least, is the result of a tolerably extensive practice in such cases with me. The supposition of the primary disease being in the ear is favoured by the following circumstances. The child, at an early period, complained of pain in the ear, which was soon followed by suppuration; and from which, at the time, relief was evidently experienced. The principal indications of disease, as met with on dissection, were also found on the left side. On the subject of the extension of disease of the ear to the brain and its membranes, I cannot do better than make the following brief quotation from M. Rostan, who, speaking of otitis, says, "*Elle peut se propager jusqu'aux méninges et à l'encéphale; alors les symptômes des phlegmasies de ces organes se joignent à l'otite.*" The idea that the disease originated in the brain is favoured by

the fact that the child had been evidently out of health, with loss of appetite, deranged bowels, &c. for some weeks before it made any complaint of the ear; which, it must be well known, is a condition which very often precedes the unequivocal indications of cerebral disease, more especially that form of hydrocephalus to which the term *chronic* is not perhaps justly applicable, but which is *de facto* subacute. That, in these cases, the ear is often affected, I can testify from personal observation; and am happy in being able to corroborate this by again referring to M. Rostan, as the following passage sufficiently proves. "D'autres fois les maladies cérébrales commencent, et l'otite n'est qu'un effet consécutif. La mort peut alors terminer cette affection." The same writer, under the head of chronic hydrocephalus, gives the following emphatic description of symptoms, and which is but too graphic, so far as the present case is concerned, in its early stage. "Les sens sont d'abord affaiblis, l'intelligence est presque nulle, la mémoire abolie; il existe une grande indifférence, la marche est chancelante, quelquefois impossible. La malade éprouve souvent de la somnolence," &c. &c.

In conclusion it may be observed, that whatever the primary seat of disease was in the case before us, extreme vigilance on the part of the physician is required to detect the very first appearance of cerebral symptoms, and to use every means calculated to oppose their assault; as even where such symptoms are not present, it is often useful to apply a few leeches and blisters in such a way as to affect the nervous centre.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à alonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Hints for Invalids about to visit Naples; being a Sketch of the medical Topography of that City. Also an Account of the Mineral Waters of the Bay of Naples, &c. With Engravings. By J. C. Cox, M.D., &c. London, Paris, and Nottingham, 1841. 8vo. pp. 190.

THE chief subjects treated of in this work are the medical topography of

Naples; the question whether Naples is a good winter's residence; the mineral waters of Naples and its neighbourhood; the mineral and thermal water of Pozzuoli; Ischia and its hot springs; and the mineral waters of Castellamare and Torre del Annunziata. When discussing the medical topography of Naples, Dr. Cox gives the following hints:—

"1. That the apartments for an invalid should have a southern aspect: this is quite essential throughout Italy. A south room, at Naples, has the sun in winter, and receives the sea-breeze in summer. There is sometimes a difference of from 30 to 40° of Fahrenheit between the north and south side of a house at noon; and in a room with a south aspect, the thermometer will rarely fall below 60° throughout the winter, even without fires. During the night there is but little difference.

2. None of the apartments should be exposed to the Tramontana winds, or NE.

3. The apartments should not be too near the Tufa rock, through which the offensive fluids from the drains often percolate, causing unpleasant and unwholesome effluvia."—p. 27-8.

The months of October, November, and December, are generally mild and pleasant, the mean temperature of these months being respectively, 62°, 55°, and 51°; in January (the coldest month) it is 45°.

Dr. Cox cautions the invalid against leaving Italy too early in the spring. In 1839, he left Naples, in charge of an invalid, on the 4th of April, when the weather was deliciously warm: the Judas tree was in blossom in the Villa Reale, and nightingales were singing in the Pontine marshes. On the 6th, however, it was cold at Rome; and though it was warm on the 14th at Bologna, he passed the Alps in a snow storm on the 23d.

Naples and its neighbourhood possess mineral waters of high repute. The Acqua de' Pisciarelli is a very remarkable one. A pint of it contains nearly 22 grains of acid sulphate of alumina; five of sulphate of iron; and twelve of sulphate of lime; besides seven grains of "free sulphuric or sulphurous acid."

Dr. Cox's book contains very useful hints, both for invalid and physician.

Medicinische Fragmente betreffend eine allgemeine Lehre des Seebadens und der Seebäder, und die Identitätsfrage der Kuhpocken und Menschenpocken. Von DR. CARL MUHRY, u. s. w. Herausgegeben mit einigen Zusätzen von DR. ADOLPH MUHRY, u. s. w.

Medical Fragments relating to a general system of sea-bathing and sea baths, and the question of the identity of cow-pox and small-pox. By DR. CHARLES MUHRY. Edited, with some additions, by DR. ADOLPHUS MUHRY. Hanover, 1841. 12mo. pp. 168.

DR. CHARLES MUHRY, whose life, too short for science and his friends, terminated at the age of thirty-four, was court-physician at Hanover, and official physician (*Badearzt*) at Norderney. This is a sea-bathing place on the German Ocean, and his appointment there excited Dr. Mühry to visit a number of similar spots in the autumn and winter of 1839-40, in order to compare their respective advantages. He visited Scheveningen, Ostend, Dunkirk, Calais, Boulogne, Dieppe, Havre, Barmouth, Aberystwith, Tenby, Plymouth, Torquay, Dawlish, Weymouth, Brighton, &c.; but his health, previously feeble, sank under the hardships of such a tour at such a season, and he died in March, 1840.

These fragments make us regret that his career was so brief; for Dr. Mühry was evidently a man of talent as well as of industry.

MEDICAL GAZETTE.

Friday, May 21, 1841.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”
CICERO.

THE STATE OF THE PROFESSION.

AMONG the numerous letters which we have received on the subject of Medical Reform, none more plainly points out the origin of the great mass of evils which now press upon the members of our profession, than that published last

week under the signature of J. R. B., in which they are described as arising mainly from the superfluity of practitioners—from the “overstocked medical market,” as the writer calls it. Our correspondent evidently writes from a practical knowledge of the subject; and we are glad to have received a confirmation of the opinions which we have often advocated from so respectable an authority.

Look where we will, we find illustrations of the same fact, that the supply of medical men of all classes is greater than the demand for them, or if not greater than the demand, yet certainly greater than is proportionate to the means, or the disposition, of the public to remunerate them. In those positions which are deemed the highest in the profession, and which have at all times required either the greatest amount of talent or industry, or else the most fortunate concurrence of external circumstances for their attainment, how great is now the disproportion between the number of candidates and that of the places that can be found for them! It is but a few years since physicians were *invited* to take high stations in great hospitals; and a young man had but to attach himself to a hospital, in order to be sure of arriving in a few years, with little or no opposition, at some honourable and profitable post in it. Such was the case with the majority of the elders among our present hospital physicians and surgeons; they obtained their office as a matter of course, with little trouble, or with, at most, the exercise of a few years' patience: the reputation which they had acquired for talent or industry, or their good connection, was sufficient to displace the few and unimportant competitors for office. But now, how different is the case! Not only do offices of value become vacant less frequently, but, when vacant, there is

not one that is not contended for by a number of candidates—all, it may be, of high merit, and all bringing forward powerful personal claims. We may safely say that there is not now a hospital in the metropolis, or in the provinces, but has at least three candidates for the next office that may fall vacant, be it what it may; and in many the number of expectants is yet much larger: so that in general he who determines on entering upon this high ground of medical practice may reasonably anticipate that he will have to wait not less than a dozen years before his time for election will arrive; and unless he is most favourably placed by personal connection (a kind of influence of which the value is now rapidly decreasing) he must be content to maintain his claim for even this distant privilege by constant and intense application to his studies.

Such, we believe, is strictly the case with the aspirants for the highest advantages and honours which are to be found in the medical offices in our large hospitals. If we descend a step, and look at the state of the smaller institutions and the dispensaries, the case is the same, but the competition is yet greater. And this is the more remarkable when it is considered how small, how insignificant, an amount of advantage is to be obtained from a connection with these minor institutions, the majority of which offer neither direct nor indirect remuneration of any kind whatever. Yet, in spite of this, no office even in them falls vacant, but it is earnestly contested for: the little amount of fame that may result from publishing one's name in a legitimate advertisement, the imagined connection to which it may lead, the erroneously anticipated advantage of possessing a title of any sort, and the necessity of doing something to maintain one's knowledge and one's spirits,—these are

sufficient reasons for incurring expense, labour, and responsibility. Nay, more than this, so great is the anxiety for the little opportunities which dispensaries offer to young practitioners for raising themselves above the mass, that they are often *got up* for this only purpose; the relief of the sick poor being merely an end which they accidentally fulfil. And what is worse, they are got up not always by the exertions of the intended medical officers, but sometimes even at the expense of those officers themselves. It is but a few months since we heard it mentioned with much satisfaction, that the poor of a certain district were likely to obtain considerable advantage from the establishment of a new medical charity there; for that two young practitioners had promised to contribute each a thousand pounds towards its funds on the (*implied*) condition that they were to be the medical officers.

The case is nearly the same with the still lower medical offices; surgeoncies to Unions and parish-workhouses are urgently canvassed for, the contract system is humbly submitted to, and gentlemen by birth and education are obliged to allow themselves to be treated like little tradesmen by their inferiors, in order to obtain what seems to afford the only chance of a decent maintenance,—the reputation of conducting a practice. And if we pass from the endeavours to obtain public offices, for the sake of the private practice to which they are (often unwisely) expected to lead, to the events of private practice itself, the competition only becomes more severe in direct proportion as the stake contended for is more palpably valuable, and more immediately at hand. It would be too long to tell even a brief tale from our own knowledge of it; let our readers look again over our correspondent's letter; he is one of themselves, and he has told then plainly enough their dis-

order, though he may not have suggested the right remedy ; let them see if what he has described be not as true, as it is a vivid, picture of what may be seen every day in our profession. The simplest statement of the case is, that a maintenance must be had by all ; those on whom the necessity falls most heavily cannot afford to be very nice or etiquetteish about the means of obtaining it, so they be but honest ; and each, “in his fall, drags down with him the whole profession :” few or none can afford to have their whole practice taken from them, and all, therefore, descend a step or two that they may adapt themselves to the disposition of the public, whose eye is as keen to the price of medical attendance as to that of any other article, and who, when the charges of the practitioners are different, can easily persuade themselves that all the *cætera* are *paria*, and find a good excuse for employing the cheapest.

Now this is an evil not capable of remedy by any of those schemes of medical reform that are so fondly and so loudly agitated : it is absurd to charge this upon errors of medical government, or to regard it as any thing but the natural result of our profession having, not long since, offered at a low price such advantages, both pecuniary and social, that more were induced to enter it than it can honourably maintain. Add to this, that the want of power to prevent the practice of unlicensed men has enabled the whole multitude of chemists and druggists and charlatans to come into open competition with the legitimate practitioners of every branch of the art, and the fundamental source of the evil condition of the medical profession is easily explained.

Would that its remedy were as easy, and that it could be effected without much endurance of hardship and annoyance by those whose lot has

fallen in these evil times ! Still, it is some comfort to know that the natural remedy is working, and that if the remuneration of medical services cannot be raised sufficiently to keep pace with the increased number of those who claim a maintenance for them, the number of claimants is slowly, but certainly, decreasing ; and that in the course of some few years it may reasonably be hoped that it will be reduced to such a number as may find enough to serve them all. At the commencement of the winter season we pointed out the fact of the general decrease of the number of students who had arrived in London for the completion of their medical studies. At that time we stated it as our opinion, that the increased facilities of education in the provincial schools, to which the decreased number of students in London was generally ascribed, were not sufficient to account for more than a small part of the great deficiency at that time evident. And our opinion is fully confirmed, by the fact that the provincial schools have all, or very nearly all, suffered decrease in the number of their students, at the same time, and in the same or a greater proportion, than the London schools have. Add this to the fact, which must be familiar to every provincial practitioner, that the number of pupils apprenticed in each town without a medical school is considerably less than it was ten and twenty years ago, although the number of surgeons is considerably greater, and no further evidence can be needed, that the number of practitioners of every class will, in a few years, be greatly diminished, if not absolutely, at least proportionally to the increasing population and means of remuneration.

But meantime inconveniences must be felt, and upon some must fall heavily. Yet it behoves all to consider how they may in some measure relieve themselves ; and there can be little

doubt that they will best do this by taking the highest ground they can; by avoiding, as far as possible, all those plans of gaining money or repute which they feel to be derogatory, and by enduring even a limited remuneration patiently, rather than seek a competency by practice which is dishonourable to their profession. The misfortune is, that the public, finding the hold they have upon the medical man—that by the competition he has to encounter he is often driven to his last resources, and compelled to be content with almost any terms that afford him a prospect, however distant, of bettering his condition—take advantage of him; and, in the ordinary fashion of the day, grind him down. Hence it is that public offices once paid are now sometimes required even to be paid for; hence that in charities, and almost all other institutions, the only officers not paid, or inadequately paid, are the physicians and surgeons, who have to look on calmly and unprofitably, while solicitors, agents, architects, and collectors, are drawing handsome incomes from the sources on which *they* gratuitously attend. Hence, too, it is, that shillings, or nothing, are offered for vaccination; that medical services, like butcher's meat, and flour, are to be tendered for; and that when a practitioner would give his gratuitous services in the cause of charity, he must ask leave, as a great favour, and incur what are regarded as deep obligations by those who give their votes in his favour.

The fact is, that though, on the one hand, there are certainly more practitioners than the public can be reasonably expected to remunerate; yet, on the other, the public are not made to give nearly so much of either respect or money to the medical man as they can well afford. And the only mode in which they can be made to do so, is by force. However

much the legitimate practitioner may suffer at the first by refusing to do what he honestly regards as dirty work, he may rely on it that in the long run he will be repaid for his honesty: his services cannot be dispensed with; he may proceed upon it as upon a certainty that good medical attendance will at last be paid for at any price, and that if he can but wait he must in the end get his own terms. This belief is the best guide that we can offer for overcoming all the hydra-evils that spring from the present superfluity of medical practitioners: they are indeed but temporary; but they are for the time so heavy that any relief from them ought gladly to be made available.

MEDICAL REFORM.

RECENT PROCEEDINGS OF THE COLLEGE OF PHYSICIANS.

To the Editor of the Medical Gazette.

SIR,

My attention has been called to an editorial article in a late number of the *Lancet* (for May 8), purporting to give to the readers of that periodical some account of the more recent proceedings of the College of Physicians in regard to medical reform. The true spirit of the *Lancet* has never shone forth more conspicuously than in those few pages. The aim and object of the article is to traduce a certain portion of the College, namely, those *petitioning Licentiates*, who have within the last five or six years been elected into the fellowship. These gentlemen have incurred the high displeasure of the governing powers at the *Lancet* office, in that they accepted the hand of good fellowship held out to them, and have lent their aid to reform abuses, instead of holding back to participate in the work of destruction. These are heavy crimes in the eyes of the *Lancet*, and such men must be held up to public contumely lest their conduct should become contagious. The *Lancet* evidently dreads the labours of those who set themselves seriously to the repair of a time-worn edifice. For years past it has been struggling to pull down, in the hopes of erecting, on the ruins of

the old medical corporations, a new order of things, which shall be perfection unimproveable: in plain English—a medical parliament, with the Lancet as Speaker.

To have looked to the Lancet for a simple statement of the events that have recently occurred within the walls of the College—to have anticipated in its pages a candid acknowledgment of the difficulties to be overcome—to have expected that its columns would have attributed honourable motives to those who took the lead in the discussions, and have acknowledged in their efforts a zeal for the welfare and honour of the profession; this would indeed have been to have hoped to gather grapes from thorns, and figs from thistles. Far different is the course of the Lancet. That moral obliquity of perception which has distinguished its career throughout, could see only in the efforts of the petitioning Licentiates “an abandonment of the principles for which they originally contended,” and an ardour in the upholding of injustice “which might have deceived the very elect.”

It is high time, sir, that the profession at large should be set right on these points. I feel assured that the great bulk of the profession throughout this country are willing to listen to the truth when it is set before them, and take more pleasure in a plain unvarnished tale than in the distortion of some facts, and the concealment of others. I do them only common justice, when I attribute to them no wish to misrepresent either the motives or actions of those who are engaged in the business of repairing the defects of the old medical corporations, and adapting them to the spirit of the age in which we live. To such men I now address myself; and I feel persuaded they will bear with me while I state, as briefly as the extent and importance of the subject allows, the principal points which have lately been under discussion at the meetings of the College, and the practical results to which those discussions have led.

The first sketch for the improvement of the College of Physicians has been for some time before the public. After enumerating the several grievances alleged by the petitioners for medical reform, it proceeded to suggest a va-

riety of measures calculated to obviate these objections, and to meet the wishes of the great body of physicians scattered throughout the country. The College of Physicians virtually acknowledged, by sanctioning the publication of their Committee's report, that the altered circumstances of the times demanded great and important changes in their constitution. Some of the projected alterations had reference to the interior economy of the corporation, contemplating more especially the relation in which the Fellows and Licentiates should hereafter stand towards each other. Others had reference to the relation which might hereafter advantageously subsist between the Members of the College of Physicians of London, and those of the Colleges of the sister kingdoms. The matter brought under discussion involved both principles and details. It was felt to be more imperative on the College to lay down general principles than to suggest measures of detail. It was considered also, but not until after ample deliberation, that it would conduce to a better end, and work out more practical benefit to the profession, if the College of Physicians confined its recommendations, in the first instance, to the improvement of its interior economy, thereby giving an earnest of its readiness to assist more largely afterwards in the great work of medical reform.

The first question, then, was, how should the Licentiates stand in regard to the Fellows, keeping in view the general interests of the profession? It was agreed on all hands, that the term *Licentiate* should be abolished, and that of *Member* substituted. To prove that this change was not in name only, it was further agreed that the Members or Licentiates should have access to the library and museum of the College. It was next proposed to endow them with certain substantive powers. The original idea was to grant to them the exclusive right of initiating all successions to the fellowship, reserving to the existing Fellows the right of veto upon one half of their nominations. This proposal having been objected to, it was then agreed that Fellows and Licentiates should have an *equal* right of voting at the election of Fellows, and that all Members, of whatever standing, should be eligible to the Fellowship.

But it was felt that a bond of union

between the Fellows and Members was still wanting. To supply this deficiency, it was proposed, and agreed to, that once in every year the Fellows and Licentiates (or Members) should meet together at a general conference or concio, when Fellows should be elected (by ballot), and such other business transacted as might hereafter be agreed upon. To prove the anxiety of the College to act *bonâ fide* in these matters, and to give, not the semblance but the substance of corporate improvement to the great body of physicians practising in this country, it was then agreed, that all physicians now practising throughout England and Wales with a diploma of M.D., obtained, after examination, from any British University, should be entitled to admission as a Member of the College without examination, provided application should be made within twelve months from the passing of any new act or ordinance for the regulation of the College of Physicians; and that, at the expiration of such year of grace, the list of Fellows should be augmented by about one-fourth of their existing number.

In the progress of the discussions on those points, it was, as the *Lancet* stated, proposed by certain members of the College, that the order of Licentiates should be abolished altogether. This proposal was objected to, on the ground that it was desirable to uphold reasonable objects of ambition to the physician at all periods of his career. His first object would be, after acquiring academical honours, to become Member of the College of Physicians, then Fellow, then one of the Consiliarii; reserving, as the highest object of ambition, in his maturer years, the dignity of President. The *Lancet* misrepresents the petitioning Licentiates, when it avers or implies that they contended for an absolute equality of privileges between the orders of Licentiate and Fellow. Their complaint was, that the graduates of Oxford and Cambridge *claimed* admission to the Fellowship as a right, while graduates of other Universities (including, *ex necessitate rei*, all Dissenters) were *excluded* from making such a claim. This was the master-grievance of the petitioning Licentiates. They never prayed that the distinction of Fellows and Licentiates should cease, but that the distinction as to admission to the

Fellowship, founded on the locality where the diploma had been received, should thenceforth cease and determine. Greatly is it to be regretted that the prayer of that petition was not then (1833) freely and unconditionally granted by the College. Had such a course been taken, I am strangely deceived if the march of medical reform had not then advanced, slowly but steadily, and the world been spared that sad display of jarring interests and contending factions which the medical profession presents at the present moment.

On the desirableness of amalgamating into one College or Commonalty all the practising physicians throughout England and Wales, and of giving to them all one common interest, the College of Physicians is now agreed. They have come to the determination of recommending measures which will, I sincerely believe, contribute to that most desirable consummation. They are willing to go further, and in the same spirit to discuss with the government, and the other corporate bodies, such measures as may be necessary to establish a proper relation between the practising physicians of this and the sister kingdoms; to organize a council of revision; and to secure, by proper enactments, a due amount of elementary medical and scientific education.

While the College is proceeding in this just and honourable course, let the readers of the *Lancet* hesitate ere they give credence to the stories they may hear of renegade Licentiates, and their abandonment of principle. The cause they have at heart, the improvement of the medical profession, will, I am persuaded, be better served by ample deliberation, and the careful improvement of existing institutions, than by the adoption of untried schemes, and the substitution of a visionary simplicity, inapplicable to the varied state of society in the different portions of this extended empire. To co-operate with their fellow members of the College of Physicians in attaining such an object, will be, as it has been, the constant aim of the petitioning Licentiates, in spite of all the sarcasms of the *Lancet*.—I am, sir,

Your obedient servant,
PHILODEMUS.

London, May 18, 1841.

ROYAL MEDICAL & CHIRURGICAL
SOCIETY.

May 11, 1841.

DR. WILLIAMS in the Chair.

Colica Pictonum treated with Warm Water.

By JOHN WILSON, M.D. Physician to the Middlesex Hospital.

IN this paper, the author, after referring to the complicated treatment of the disease pursued at the hospital of La Charité, proposes to illustrate two of the remedies there used, in combination, namely, enemata and hot baths.

In six cases of colica pictonum, some of them of much severity, and complicated with paralysis, Dr. Wilson has employed enemata administered in the hot-bath, in one case by the patient himself, and consisting of the water of the bath. The effect of this combination has been very successful, both in regard to the symptoms of pain and constipation, and to the paralysis where that existed. In the course of his statement the author notices the presence of looseness of the gums, blueness at their edges, and a foetor like that of mercury, where none had been taken; and informs us that he has noticed this latter circumstance in other cases of colica pictonum.

He next details a case of constipation not attributable to lead, in which the above remedies had proved equally successful. In some of the above cases this treatment is used alone; in others it is followed up by doses of pil. saponis c. opio, of oleum ricini, with tinctura opii, or with a solution of sulphate of magnesia and carbonate of magnesia in mint water.

The President said that the method of treating the disease at La Charité, to which the author had alluded, had long been proved in this country to be unnecessarily complicated and severe. At St. Thomas's Hospital it was always found that the disease would yield to the administration of purgatives of almost any kind, combined with opiates. He believed the only novelty in the present paper was the recommendation that the warm water enema should be administered in the bath, and that the patient should be allowed to discharge his faeces there also.

Dr. Mayo said he feared that, by omitting the greater part of the author's histories of cases, he had not acquired for the method of treatment recommended in them all the credit that it deserved. He was bound, therefore, to say, that it had been very successful even in cases of great severity.

A paper was afterwards read—

On the Results of Amputations performed at the North London Hospital. By Mr. POTTER. Communicated by Mr. LISTON.

Mr. Perry said he should like to ask of Mr. Liston whether the exactly transverse manner in which the limb was described as being transfixed, in forming the flaps, had been adopted in order to avoid the oblique division of the femoral artery, which had been stated as one of the objections against the flap amputations of the thigh. In many cases that he had witnessed of the ordinary flap amputations, the artery had always been very obliquely divided, so that it could be tied only with considerably greater difficulty than in circular amputations.

Mr. Liston said that the formation of antero-posterior flaps was adopted without any reference to the mode in which the femoral artery was divided. It had considerable advantages in securing the stump from the protrusion of the bone, which was very likely to take place after amputations with lateral flaps. In them there was nothing left by which the action of the psoas and iliacus muscles could be prevented from pulling the thigh upwards towards the abdomen; nor could any artificial means be adopted to prevent this, especially in children; and its consequence very frequently was, that the sawn end of the bone was so drawn up as to protrude at the anterior angle of the wound. The antero-posterior flaps permitted no inconvenience of this kind. As to the oblique division of the femoral artery, he did not consider that a matter of any importance; it was easy enough to dissect its cut extremity a little backwards with the amputating knife, and apply the ligature quite above the division, however oblique it might be.

Mr. Rutherford Alcock said that the question of amputations generally, and of the different modes of performing them, and of the effects of different circumstances upon the results of the cases, had engaged much of his attention for several years. Many of the results which he had attained were already before the public, and he would now limit himself to observations on the principal topics of the present paper, and especially to the question of the comparative advantages of the flap and circular modes of operation, and the results of each with reference to the ultimate terminations of the cases, and the several accidents that supervene on amputations. With respect to the occurrence of secondary hæmorrhage, he quite agreed with Mr. Liston that the oblique division of the femoral artery was not a matter of importance. But he was not sure that the oblique division of the smaller arteries had not some influence in bringing on or permitting that accident. He had often thought

that, in flap amputations, the small vessels retracted only partially, which, in circular amputations, would have retracted completely; or that the vessels in the former case were overlooked, which, in the latter, bleeding distinctly, would have been secured. Nothing, however, had convinced him more of the necessity of having accurate records of series of cases, and of not trusting to general impressions, than the results of amputations. In these questions, more than in any others, general impressions were apt to be erroneous, from the circumstance of the strength of the impression which single events, deemed at the time remarkable or important, make upon the mind. With respect to this very question of secondary hæmorrhage, for instance, he had himself gained a very wrong impression from two cases, which was not corrected till he had accurately ascertained the results of the whole series of those that had fallen under his observation. The two cases were these:—In an epidemic from which the troops under his charge suffered at Vittoria, the legs in several cases became gangrenous, and required amputation. In the first two instances he amputated in each patient one leg by the flap, and the other by the circular operation; and in both these cases the flap operation was followed by secondary hæmorrhage; but after the circular none supervened. The circumstance made such an impression on him that he entertained no doubt that flap amputations were more obnoxious to secondary hæmorrhage than circular, till he came to sum up the results of all his cases, or, at least, of all those of which he had a regular and well-kept series. The result of this examination was, that in 115 cases, of which 90 were circular amputations, and 25 flap amputations, the proportion of secondary hæmorrhages was somewhat greater in the former than in the latter. In the second place, with regard to the liability to necrosis of the sawn bone in the two classes of cases, he did not think that any difference could be found between them: and it was the same with regard to the conical form of the stump, an event which he believed could only very rarely occur after either kind of amputation, if dexterously performed by a surgeon practised in the operation. As in the 115 cases alluded to it had occurred only twice, he could not draw any conclusion respecting its frequency in one or the other operation. With reference to the main question of the comparative mortalities of each mode of operation, he has found that his results were by a fraction disadvantageous to the flap amputations. He was not, therefore, prepared to agree in what was said to be the growing opinion that this operation would in time completely supersede the other. It was true that the flap amputation was a

more rapid mode of operating than the circular, and to this full weight must be allowed; for pain is in itself an absorbent of life, and is always followed by a reaction and a febrile disturbance, that may have an important influence on the recovery of the patient. At the same time he thought that the difference in this respect between the two modes of operation had been overrated. Eighty or ninety seconds were ordinarily sufficient for the performance of the circular operation; and if the flap amputation were done in fifty or sixty, the difference of the times during which the patient was in each exposed to pain was really not so great that much importance could be attributed to it. It did not appear to him, therefore, that on this ground there could be any just reason for discarding the circular mode of operation. A more important point seemed to him to be involved in the question of the after treatment of the stump. There was nothing of which he was more certain than that the idea of the advantages of union by the first intention, which was so commonly held in this country, was greatly exaggerated, and he would go so far as to say that, in some cases, such a union was not only of no advantage, but was actually mischievous, and dangerous to the life of the patient. For patients, after immediate union of the wound, would often, as he had himself seen, die of the very diseases which were said to result from the wound remaining open. In a primary amputation, after an accident in a healthy man, it was no doubt most natural to desire that the wound should be healed as quickly as possible; and probably, in these cases, it was best to try to obtain a union by the first intention. But in secondary amputations, and in those performed for diseases of the limbs, when the incisions were made through diseased tissues, there was no use whatever, but, he believed, a disadvantage, in bringing them together. They would almost invariably open again, and place themselves in the position in which they had better have been left from the first. This was especially the case when limbs were amputated for diseases which had been connected with a considerable discharge. In all these it was dangerous to stop the discharge too suddenly, and it had been proposed to make an issue near the end of the stump, to keep up an artificial drain to replace that which had been cut off. But this method, he thought, would be very inconvenient, and such indeed as could scarcely be put in practice; another, which he had sometimes adopted, and which seemed to fulfil all the intention, consisted in passing a skein of silk through the lower part of the wound, and thus maintaining, for a time, a constant irritation and a discharge of matter, which afterwards might be gradually reduced.

Mr. Benjamin Phillips said that it was a source of great gratification to him to find that the results of the inquiries which he had made some years ago had been now, of late years, repeatedly and completely confirmed. Some years ago a paper of his had been read at this society, detailing the results which he had drawn from an analysis of all the cases of amputation that he could meet with; but the conclusions to which he had arrived were at that time so contrary to the generally received opinions in this country, that it was believed that there must have been some great error in his calculations, and the council of the society had decided that it would not be safe to publish his paper. At that time the North London Hospital was the only one in London at which any regular record of the results of operations was kept, but now he had been glad to find the system considerably extended, so that the number of cases admitting of analysis had lately very much increased, and allowed of conclusions being drawn with a degree of certainty before unattainable. Nothing was more important than that conclusions should not be drawn from a small number of cases, and, as an example, he could have nothing better than the statements contained in the paper just read. When he made his inquiries, the records of the North London Hospital showed that 23 amputations had been performed, and of that number only one case had died; whereas now, 66 amputations, it seems, have been performed, and 10 have terminated fatally. The same could be illustrated by an examination of the cases of amputation at the Boston Hospitals; the proportion of deaths among the cases occurring in successive periods of two years each being widely different, and varying as much as from four per cent. to near fifty per cent. It was plain, therefore, that certain results could be drawn only from the analysis of very large numbers of cases. In his paper he had compared 650 cases, and the result was that the mortality was $23\frac{1}{2}$ per cent., and that result was deemed so extraordinary and improbable that every one thought there must be some error in it. But since that time several other reports had been published from Boston, Massachusetts, the Glasgow Infirmary, and the Liverpool Infirmary, and these had enabled him to make up his list to 1150 cases, and the result of all these was, that the average mortality was 27 per cent. He did not regard it as a matter of so much importance as some seemed to think it, to determine what was the amount of mortality in this or any other operation; it could not much influence the question of whether in any given case the operation should be performed or not; yet, at the same time, the average chance of recovery ought in all cases to be taken into consi-

deration in recommending any operation. He thought it important that we should possess not merely a knowledge of the gross amount, but, if possible, of the circumstances which render it greater or less, and this could only be obtained by tables marking the limbs amputated, the modes of operation, the disease or injury for which it was performed, the age and other circumstances of the patient; for on all these the greatest differences of result depended: if twenty arms were amputated, it was probable that there would not be more than one fatal case, but the deaths would probably be much greater in twenty amputations of the thigh. And so again in any hospital where the greater number of amputations were performed for diseases of the joints, as at St. George's, the results would always appear much more unfavourable than where the same operations were performed for accidents. And again in the case of accidents, the results obtained by a surgeon, who always did his best to save a limb from being amputated at all, would constantly seem less favourable than those obtained by one who usually amputated at once. All these things required much further investigation; nor would it be possible to make them out except by the accumulation of a considerable number of cases bearing upon each.

UTERINE HÆMORRHAGE.

To the Editor of the Medical Gazette.

SIR,

If you judge the following case of uterine flooding worthy of a column in your valuable periodical, it is at your service.—I am, sir,

Your obedient servant,

A STUDENT OF OBSTETRICY.

Guy's Hospital, May 4, 1841.

C. D., a stout Irish woman, æt. 35, living in White Street, Borough, requested my attendance at eleven o'clock on the morning of Friday, March 19th, she being then in labour with her sixth child. Uterine pains had commenced five hours previously, and had continued constantly and severely until half-past ten o'clock, at which time the membranes ruptured, and she sent for me. Immediately on reaching her residence, I proceeded to make an examination, and found the child's head presenting naturally, and distending the external parts: in a few minutes it was expelled.

Uterine action was now for a short time suspended, but when resumed, one strong pain forcibly expelled the child, and with it a large quantity of dark-coloured blood. I grasped the lower parietes of the abdomen

with my hand, and felt the uterus gradually getting smaller and harder, although a considerable flooding still continued. In a short time the uterus became firmly contracted, and the hæmorrhage very trivial. I then tied the umbilical cord; the child was full grown, and healthy. After remaining in the house nearly an hour, during which time but a very little blood was lost, I proceeded to withdraw the placenta. But the slightest attempt to accomplish this by traction of the umbilical cord was followed by its rupture, and thus for five or six times, whilst using the utmost care, the funis snapped—at last close to its placental insertion. I then introduced my fingers into the vagina, to withdraw the placenta, but this, in like manner, broke down beneath the pressure of the fingers; and it was not until the entire mass had been grasped, that its withdrawal could be effected: it was much discoloured, and of offensive smell: the membranes came away with it. A large gush of fluid, and partly coagulated blood, immediately followed the removal of the placenta. The uterus was still apparently firmly contracted, notwithstanding which, however, I applied cloths dipped in cold water to the lower part of the abdomen and vulva. The woman now complained of feeling faint, but as the pulse was still, comparatively speaking, strong, and no alarming symptoms, excepting the hæmorrhage, were present, I did not attempt to arrest the approaching syncope. She continued in a state of faintness for some time, during which the bleeding was almost entirely stopped, a small discharge only occasionally taking place: the *uterus remained contracted*. Knowing that the flooding must depend on an extraordinary disturbance of the circulation, I had the doors and windows opened, and allowed my patient to be but very lightly clothed. She recovered from her state of syncope slowly, and the bleeding did not return. I remained with her half an hour, and then (my residence being but a few minutes' walk distant) left her, having tightened a bandage previously applied, and forbidden any conversation to be held with her, or remarks which might excite her to be made in her presence.

In about an hour I was hastily summoned, the messenger informing me that she had begun to "bleed worse than ever, and that the bed was soaking through and through." I made as much haste as possible to the house, taking with me a small bottle containing tincture of digitalis, which I had obtained, fearing that the hæmorrhage might return.

On reaching the patient's room, I found that, in consequence of her having complained of chilliness, her friends had kindled an immense fire, which made the small room

insufferably hot, closed the windows, and heaped all the clothes of which they could possess themselves upon her person. I had the fire immediately diminished, and removed all the clothes, excepting a sheet. The woman had been some time in a state of syncope; the countenance and lips were bleached, the breathing hurried, the pulse was very feeble, the extremities cold. On placing my hand over the abdomen, I found the uterus quite as perfectly contracted as when I had left, an hour before, or as it is usually found after labour. Any person not being aware of the circumstances, on feeling the state of the uterus through the abdominal parietes, would have pronounced the woman, for the time at least, perfectly secure from flooding. In a short time the pulse became stronger and more frequent, and consciousness returned; a small discharge of blood also threatened a return of the hæmorrhage. I then gave twenty drops of the tincture of digitalis, and in a short time its beneficial effects were remarkable; the pulse became softer, and its rapidity diminished. I watched the case until the evening, and repeated the tincture of digitalis twice. The flooding never returned with alarming violence after the administration of the first dose.

I have troubled you with the above communication, because I consider the case very remarkable. I am aware that disturbance of the circulation, violent action of the blood-vessels, does occasionally excite uterine hæmorrhage, even when the uterus is *disposed* to contract; but in this case the contraction was perfect. I am fully satisfied that there was no *sudden relaxation* of the uterine parietes, which, by opening the mouths of the vessels, permitted the flooding to take place, because for nearly three quarters of an hour I was firmly grasping the abdomen, and feeling the hard contracted uterus within my hands. I must not omit to state that before I was sent for to the case at all, the woman had been well plied with spirits "to comfort her," and this of course would excite and disturb the circulation: her previous labours, however, had been all attended with flooding, in a degree more or less alarming. The quantity of blood lost was amazing: I will not venture to say how much. Is it possible, considering the carious state of the placenta and funis, that the uterine vessels might be in the same way weak or diseased? But supposing this to have been the case, does it in any way explain this strange occurrence?

CASES
OF
DISLOCATIONS AND FRACTURES
IN THE CERVICAL PART OF THE
VERTEBRAL COLUMN.

BY DR. SCHUK, of Vienna.

CASE II.—*Dislocation without fracture.*—

Far more rare than the dislocations of one oblique process are the cases in which both articulating processes are luxated, so that one remains fixed on the point of the corresponding process of the vertebra next below it; while the other, having been raised up to the top of the corresponding process of the next inferior vertebra, has passed over it and to its anterior surface. This cannot take place without disruption of the bodies of the vertebræ, tearing of the intervertebral cartilage, narrowing of the vertebral canal, and violent crushing of the spinal marrow. In such cases the part of the column above the situation of the dislocation suffers a slight twisting on its axis, and does not lie in a line with that which is below; there are thus two axis lines, of which the upper is somewhat the nearer to the middle line of the body, and forms a very obtuse angle with the lower. The direction and position of the spinous processes of the vertebræ above the luxation are correspondingly altered; but, in the living subject, this can be discerned only in very lean persons. The symptoms during life are, therefore, pain, impeded motion of the neck, turning of the head to one side, and all those of crushing of the cord. The same symptoms would undoubtedly arise from a fracture of the vertebræ, with displacement of the portions of bone. When only one articulating process is dislocated, and is raised to the point of that below it, there is no encroachment on the vertebral canal, and therefore no crushing of the cord; but the pressure dependent on the slight curve of the canal, or an effusion of blood on the spinal marrow, as well as the concussion which the latter receives simultaneously with the luxation, may produce similar disturbances in the nervous system; in practice, therefore, the diagnosis of one kind of luxation from another, or from fracture with displacement of the portions of bone, is sometimes quite impossible.

F. F., a robust man, thirty-three years old, fell down in his sleep from a beam several feet high, and lay a long time unconscious. When he became sensible he complained of severe pain in the neck, and difficulty of moving the upper extremities. On the same day when he was brought to the hospital the following symptoms were noticed:—Intellect and the external senses

undisturbed; pain in the nape and back of the neck increased by pressure; head twisted and somewhat turned to the left, but not so firmly fixed in this position but that it could be slightly moved backwards and forwards; pain extending from the neck in the direction of the nerves to the axilla; incomplete paralysis of the extensors of the fingers of the left hand, with full power of moving the hand and elbow-joint; motions of the lower limbs free; urine and fæces healthy, and passed naturally; pulse slow. The spinous processes could not be traced through the deep layer of fat by which they were covered. Attempts at reduction were made; the first was of no avail, but the second succeeded in setting the head straight, and rendering it tolerably capable of motion in all directions; but the back of the neck still remained sensitive on pressure. Cold lotions, and the abstraction of a pound of blood, were ordered.

Next day the patient had retention of urine, and, in the evening, creeping pain and loss of motion in the left arm, as well as in the left foot, but no destruction of sensibility; his pulse beat only about thirty times in the minute. He was again bled to a pound.

On the second day after the accident, he had difficulty of swallowing; his respiration was effected only by the diaphragm and abdominal muscles, and he made constant efforts to expectorate hard and tough mucus; the pain in the arm was diminished, but it was frequently twitching about. The pulse was 42; leeches were applied to the back of the neck.

On the third day he had paralysis of motion, but not of sensation, in all the limbs, and had still no power of passing his urine. His intellect was undisturbed; his pulse varied, and, notwithstanding his difficulty of breathing and of expectoration, he was in good spirits.

On the night of the fourth day he had a severe rigor, followed by heat; he was restless and slightly delirious, and had frequent twitchings in all his limbs; he could turn his head round with ease, but could not so easily move it backwards and forwards. The neck was again leeches.

In the two following nights he had the same symptoms; on that of the seventh day he was again restless and slightly delirious, and his sensibility was diminished both on the extremities and on the trunk; he lay motionless on his bed like one dead, or like a living head set on a lifeless trunk. The temperature was equal over the whole body, and not increased; but the patient complained of an excessive internal heat, so that he could not bear to be covered by more than a single sheet. His respiration continued the same; and thus, with little change, he went on till the twelfth day after the reception of the injury, his respiration

growing gradually worse, till, with a sudden aggravation of its difficulty, he died.

At the examination of the body there was found, as soon as the vertebral column was exposed, a separation between the fifth and sixth vertebræ, so that the whole of the column above the latter projected for half an inch forwards. The right articulating process of the fifth vertebra had mounted over the corresponding process of the sixth, and was now in connection with the anterior surface of the latter; while the tips of the left articulating processes of the same vertebræ were impacted one upon the other. By extension the dislocated parts could be replaced; and again, by pressure and sliding them upon one another, they could be returned to the same abnormal position, and no other; they could, therefore, have had none but this position during life. The intervertebral cartilage was torn and crushed, and the spinal marrow, from the fourth to the sixth vertebræ, squeezed flat, very soft, and reddened by effused blood, with its grey substance nearly fluid, and its dura mater injected. The lungs were cedematous.

CASE III.—*Dislocation without corresponding fracture.*—J. P., æt. thirty-five, fell from a cart, first with his neck upon the shaft, and then with his head upon the ground. He felt severe pain in his neck, and became immediately paralytic in his hands and feet. He was bled, and two days afterwards was brought to the hospital, quite conscious, speaking plainly, though slowly, with his respiration weak and slow, his deglutition easy, his pulse weaker and slower than natural, his abdomen slightly distended, retention of urine, which when drawn off was normal in its characters, costiveness, and a decreased temperature of the skin's surface. From the middle of the back up to the head, and especially at the lower part of the cervical region, the vertebral column was excessively tender; the pain was increased by moving the head backwards, but not materially affected by the rotatory motions. The upper and lower limbs were completely paralyzed as to their power of motion, and the arms, from below the insertion of the deltoid muscles, and the whole of the body below the mammæ, were completely insensible.

On the next day, his speech was slower and more indistinct; the respiration and circulation still more feeble and slow; the abdomen more distended; the heat still further diminished. In the evening he had delirium, and his pulse could not be felt; towards midnight his consciousness again became clear, and at 3 A.M. he died.

On examination, the fifth cervical vertebra, with all those above it, was found projecting three or four lines before the sixth,

and the inferior articulating processes of the fifth were placed in front of the superior articulating processes of the sixth. The anterior vertebral ligament, and the ligamentum subflavum, as well as the intervertebral cartilage between the two, were torn; the spinal marrow was squeezed flat, and was crushed soft, and there was some blood around the dura mater. There was also a vertical fracture between the bodies of the fourth and fifth vertebræ, but there was no mobility of the fractured portions, for a small portion of bone at their anterior surfaces remained and held them together.—*Medicin. Jahrb. des k. k. Oesterreichisch. Staates*, Bd. xxi. St. iv.

HARROWGATE WATER.

THE water [of the Montpellier sulphur spring] is perfectly colourless and transparent, and almost brisk from the escape of gas. The first impression on the tongue is intensely salt, followed by the peculiar bitter taste of salt water, but leaving an *après-goût* like that which remains after chewing bitter almonds. It goes down oily, and at the temperature at which I drank it (115°) the sulphuretted gas is scarcely perceptible. I repeated the same quantity four times, diminishing each time the artificial temperature, until I drank it cold, thermometer then marking 52°, while the external atmosphere was at 60°, and the nauseous taste had increased with the descending temperature.

The whole quantity I took in four times, I noticed people to drink at twice only, and quite cold. Writers on this water have recommended the latter practice. This is an error which I was sorry to see committed at all the English Spas. There are few stomachs which can bear with impunity the weight of two doses of three-quarters of a pint each of a cold, salt, and sulphuretted water, drunk with a short interval between. Few stomachs can stand the slow extrication of the imprisoned gas, which, once ingested with the cold water, is gradually disengaged by the warmth of that organ. It then mounts into the head, and produces a confused, heavy, and unpleasant feeling.

This I have put to the test of my own personal experience. Drunk quite cold, I found the water particularly heavy on the stomach, and in an hour's time my head ached not a little. Some of my younger patients in this place experienced similar effects; and, indeed, upon inquiries among strangers, who were religiously following the recommended practice, I ascertained the case to be precisely the same. At all events the first glass or two should be warmed, but not so much so as to drive off the whole of the sulphur gas. With respect to quantity, that point has been determined by long

experience, and by very competent authorities. It did not appear to me that people on whom devolved the management of the water, at the several Spas I visited in this country, were sufficiently aware of the importance of this consideration. The quantity drunk, at one time, should be such that, during the fifteen minutes' walk which is to elapse between one dose and the next, the stomach may have got rid of the first before it receives the second. Four ounces of liquid ingested will nearly disappear from the stomach in the course of twenty minutes, particularly when assisted by walking exercise.

Such is the opinion and practice at the Spas in Germany, where beakers holding hardly as much as four ounces are used by every individual, and not the half-pint and *whole-pint* tumblers which we see employed here, at Cheltenham, Leamington, and elsewhere.—*The Spas of England, by Dr. Granville.*

RECEIVED FOR REVIEW.

The Diseases of the Digestive, Urinary, and Generative Organs of the Horse, with the different methods of castration. By W. Percivall, M.R.C.S. &c. &c.

On Gout: its Cause, Nature, and Treatment. By John Parkin.

Dr. R. Quain's Anatomy of the Arteries of the Human Body, Part 7.

Dr. Thomas West's Treatise on Pyrosis Idiopathica, or Water-brash, as connected with certain forms of Indigestion, and of Organic Lesions of the Abdominal Organs, &c. &c.

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Wednesday, May 12, 1841.

George Dickson.—Francis George O'Kearney.—Edward Stock.—Frederick Hodgkinson.—James Rogers.—Hugh M'Clelland Graham.—James Bell Metcalfe.—Alexander Duncan.—Edward Lawrence Ireland Gaine.

Friday, May 14.

John Miller.—Nathaniel Henry Clifton.—Samuel Coates.—Thomas Lancaster Bill.—William Briggs.—Josiah Heelis.—Taylor Hannah Murison.—William Knight Erskine Allan.—John Christie.—Thomas Jackson Graham.

Monday, May 17.

John Cartwright.—William V. W. Langley.—John Woodward.—William E. Baddeley.—William Heygate.—Michael Doyle.—Philip T. Scott.—James Henry.—Rowland Rowland.—Edwin Fennell.—Moses Drake.—C. L. Bradley.

APOTHECARIES HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, May 13, 1841.

Raymond Levi Haynes, Holloway, Middlesex. Thomas Guy, Howden.—John Beau Armour,

London.—Henry Piers, Weymouth.—John Cornish Trestrail, Truro.—John Ellison, Liverpool.—William Wilson, London.—William Okell, Knutsford.—John Froggatt, London.—Edward Henry Chase, Bocking, Essex.—James Moline, Godalming.—John Harry Evans, Seaford, Sussex.—John Wilson, Huntingdon.—George Harvey Williams, Holywell, North Wales.—George Tennyson Moody, Grimsby.—John Rayner Hatfield, Birstall, Yorkshire.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 8th May, 1841.

Small Pox	17
Measles	13
Scarlatina	8
Hooping Cough	44
Croup	4
Thrush	3
Diarrhoea	3
Dysentery	1
Cholera	0
Influenza	4
Typhus	9
Erysipelas	4
Syphilis	0
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	148
Diseases of the Lungs, and other Organs of Respiration	269
Diseases of the Heart and Blood-vessels	15
Diseases of the Stomach, Liver, and other Organs of Digestion	42
Diseases of the Kidneys, &c.....	5
Childbed	6
Ovarian Dropsy	1
Diseases of Uterus, &c.	2
Rheumatism	2
Diseases of Joints, &c.	3
Ulcer	0
Fistula	0
Diseases of Skin, &c	0
Diseases of Uncertain Seat	100
Old Age or Natural Decay.....	63
Deaths by Violence, Privation, or Intemperance	23
Causes not specified	1

Deaths from all Causes

790

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N. Longitude 0° 3' 51" W. of Greenwich.

May.	THERMOMETER.		BAROMETER.	
Wednesday 12	from	48 to 64	30·08 to	30·22
Thursday . 13		47 64	30·23	30·26
Friday . . . 14		36 65	30·28	30·22
Saturday . 15		40 70	30·14	30·03
Sunday . . 16		40 70	29·89	29·78
Monday . . 17		47 62	29·64	29·55
Tuesday . 18		45 63	29·51	29·54

Wind, North on the 12th, N.E. on the 13th, and following day, West and S.W. on the 15th, since S.W.

Except the morning of the 17th, and afternoon of the 18th, when rain fell, generally clear.

Rain fallen, '02 of an inch.

Halones.—On the 14th, and two following days, these phenomena were very remarkable, particularly on the 14th and 15th, when highly coloured and visible nearly from sunrise to sunset.

CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, MAY 28, 1841.

LECTURES

ON THE

PRINCIPLES AND PRACTICE OF PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXXVI.

Epilepsy, continued. Recapitulation. Exciting causes. Simulated epilepsy. Diagnosis. Prognosis. Treatment; during the fit; during the intervals; during the warnings.

ON Tuesday last I began to speak of epilepsy: but as some of the gentlemen belonging to the class were prevented by the weather, or by other circumstances, from being present at our first re-assembling after the holidays, I will very rapidly retrace the substance of the last lecture.

An epileptic seizure may be very severe; or very slight. The very severe attacks are characterized by a sudden cry, an immediate loss of consciousness, general and violent convulsions, and subsequent coma or heavy sleep. The very slight attacks consist in a momentary abeyance of the mental faculties, sometimes with and sometimes without slight and partial convulsion. These extreme forms of epilepsy we judge to differ only in degree, inasmuch as they both attack the same persons at different times; or the one form conducts to the other. Between these the gradations are innumerable. We call the extremes the *epileptic fit*, and the *epileptic vertigo*; the French call them the *grand mal* and the *petit mal*.

These *fits* may last from a few seconds to half an hour. Paroxysms apparently longer than this commonly consist of a succession of fits. The average duration is from five to ten minutes.

704.—XXVIII.

The fits recur at variable intervals; which are sometimes periodic, mostly irregular. There may be many in a single day; there may be only one for many years. They are commonly more severe in proportion as they are less frequent.

The epileptic seizures sometimes begin in early infancy: another period at which they often commence is about the age of seven or eight: another about fourteen or sixteen, or for some few years after that age. They more often begin before puberty than after it. Sometimes the first fit takes place in the middle period of life; sometimes even in declining age. They often occur in the night, especially in the outset and the decline of the disease; usually when the patient is between asleep and awake; *i. e.* at the commencement or the termination of his slumber.

In the majority, perhaps, of cases, the fit is unexpected, and preceded by no warning. But in other instances there is some alteration perceptible by the patient himself, or by his friends, giving notice of its approach; some change in the temper, feelings, appearance; some disturbance of the senses; ocular spectra; or what is called the epileptic aura, a creeping sensation arising in some part of the surface, generally of the extremities, and gliding towards the head. Some of these warnings precede the paroxysm by a day or two, or a few hours; some by two or three seconds only. Sometimes the blow is threatened by their appearance, but it does not fall.

The fit is almost always, in its severer forms, attended and followed by coma; sometimes after the coma by temporary confusion of mind; deafness; slight paralysis; delirium; inarticulate speech. There is seldom any marked permanent alteration effected by a single fit.

A repetition of the fits leads, in a large majority of instances, first, to a defect of the memory, and of the general intelligence;

and at length to a peculiar expression of countenance, to decided imbecility of mind, to complete fatuity; and with this there is often associated some paralysis or muscular debility.

The convulsions take place, necessarily, through the medium of the spinal cord and nerves—just as voluntary movements do; but the suspension of sensation, thought, and volition (which suspension is seldom absent, while the irregular muscular action often is wanting) shews that the *brain* is essentially involved in the disease.

Accidental organic lesions are sometimes (and sometimes only) found in the encephalon, or in the spinal cord, of persons who have suffered epilepsy uncomplicated with any permanent mental or paralytic affection; tubercles, for example, or bony growths from the interior of the skull; but as these are *constant*, and the paroxysms are *occasional*, and as in the intervals they give no signal of their presence, we can only regard them as being probably predisposing causes of the seizures.

When the epilepsy *has* been complicated with permanent alienation of mind, or some degree of paralysis, evidence of chronic inflammation of the brain and its membranes is generally discovered. This has been the consequence of the repetition of the paroxysms. This explains the frequent connexion of fatuity and palsy with epilepsy of long standing.

The diseased condition which excites the paroxysms may be situated in the nervous centres themselves, or in some other part of the body. In the one case we call the disorder centric, or idiopathic; in the other, eccentric, or sympathetic. We cannot always be sure with which species of the disease, the centric or eccentric, we have to deal: but the distinction, when it can be made, is of considerable importance, in respect to the prognosis, and in respect to the management of the case.

The predisposition to this fearful complaint is often hereditary: malformation, or defect of symmetry in the two sides of the head, is a frequent predisposing cause; so, pre-eminently, is the scrofulous diathesis: and these two, viz. the scrofulous diathesis, and a peculiar conformation of the head, are both liable and likely to be propagated from parents to children. But the predisposition is found to be hereditary even when the shape and structure of the body is, to all appearance, quite perfect and natural; and when no outward indication of the strumous diathesis is perceptible.

At the very close of the lecture I informed you that it is an unsettled question—and it is not a question of very great importance—whether the disease is more common in

females or in males: whether the *sex* has any thing to do with the predisposition.

There are certain vices, which are justly considered as influential in aggravating, and even in creating, a disposition to epilepsy: debauchery of all kinds; the habitual indulgence in intoxicating liquors; and, above all, the most powerful predisposing cause of any, not congenital, is masturbation—a vice which it is painful and difficult even to allude to in this manner, and still more difficult to make the subject of enquiry with a patient. But there is too much reason to be certain that *many* cases of epilepsy owe their origin to this wretched and degrading habit: and more than one or two patients have voluntarily confessed to me their conviction that they had thus brought upon themselves the epileptic paroxysms for which they sought relief.

Exciting causes.—Among the *exciting* causes of epilepsy, fright is conspicuous. And any *strong mental emotion* is apt to produce the fit, in a person who is already subject to the disease. This fact alone would be enough, I conceive, to forbid our ascribing the paroxysms exclusively to an affection of the spinal cord. Bodily pain; manifest and great disturbance of almost any of the principal functions of the body; may act also as exciting causes. Sometimes the cause is obvious, sometimes it is quite inscrutable. If the attack occurs every night, Dr. Bright thinks it may be attributed to the “congestion” of sleep: if it takes place at monthly intervals in women, we may “often trace it to nervous irritation in sympathy with the uterus; and when long periods have intervened we may usually trace each distant paroxysm to the repetition of some excess, or to a neglected state of the bowels.” In these latter cases, the epilepsy is of the sympathetic, or eccentric kind; the irritation being seated in some part at a distance from the nervous masses, in the stomach, or intestines, or uterus. Now I would suggest the expediency of observing what muscles or sets of muscles are *first affected* by the spasm in such cases, and in what part the warning aura (if there be any) arises: because by accurately noting these particulars, we may be led to a knowledge of the part or organ in which the irritation operates: and if we know the *seat* of the irritation, we shall be more likely to know its *nature*, and its *cure*.

Among the exciting causes of epileptic fits are also enumerated—and I believe, from what I have myself noticed, with great justice—the repulsion of eruptions, and especially of eruptions about the head, when proper artificial evacuations are not employed at the same time; the cessation of habitual discharges; and, on the other hand, profuse

and unusual discharges. Hæmorrhage certainly will often bring on convulsions and a state of insensibility exactly like certain forms of epilepsy. Persons who are bled till they actually faint, are often at the same time convulsed. And animals that are killed by loss of blood, are always affected with convulsions before they expire.

There is yet another very singular occasional cause of epilepsy that deserves to be mentioned; viz. the sight of a person in a fit of that disease. This has been noticed over and over again. Not only will a patient who has already suffered such attacks often fall into one upon seeing another so affected; but people will even sometimes do so who have never before shewn any symptom of epilepsy. In this way the disease will now and then run through a boarding-school; or through a ward in a hospital. There is a very good example of this recorded in the 11th volume of the *MEDICAL GAZETTE*, by Dr. Hardy, of Bath. A strong healthy young man was hired to take care of an older patient, who suffered frequent and exceedingly violent paroxysms of epilepsy. He remained with the patient night and day; and at the end of seven weeks became himself epileptic in a very high degree. An acquaintance of his, of equally robust make, but some years older, occasionally visited the parties. In a fortnight from his first visit he also was seized with similarly violent attacks. Dr. Hardy quotes the following short case also from Baglivi:—"Vidimus, anno 1690, in Dalmatiâ juvenem gravissimis correptam convulsionibus, propterea quod inspexerat solummodo alium juvenem dum epilepsiâ humi contorquebatur."

Dr. Cullen, who, as well as many others, had noticed the same thing, starts the question whether this mode of propagation of the disease be imputable to dread and horror; or to the mere force of imitation, which is often so strong, in health as well as in disease: and he decides in favour of the force of imitation: and in fact there are many other sights equally horrifying with that of a person in convulsions; yet there is *no spectacle* of horror so efficacious in producing a fit of epilepsy in others, as that of a person suffering under epilepsy.

This principle of imitation holds good in many of the spasmodic diseases; and in some of them, especially in hysteria, its influence is more remarkably seen than in epilepsy; and I shall therefore have to recur to it again. There is one very curious fact, however, which relates to epilepsy in particular. You are aware that this disease is often feigned, by impostors. Now I believe it is ascertained beyond the possibility of doubt that fits and actions which were at first, in these pretenders, strictly voluntary,

have at length become involuntary and uncontrollable, and have passed into paroxysms of real epilepsy. The rogue is caught in his own trap.

And the mention of these impostors leads me to consider the *diagnosis* of epilepsy. First, how are we to distinguish the feigned disease from the true? Secondly, are there any other real diseases which may be mistaken for epilepsy, or for which epilepsy may be mistaken?

Simulated epilepsy.—In the number of *feigned* diseases epilepsy is one of the most common. Soldiers and sailors pretend to have epileptic fits, in the hope of obtaining their discharge from the service. Cases of simulated epilepsy occur also continually in our streets among mendicants, and impostors who think to excite the compassion and pecuniary charity of the credulous; and are even sometimes actuated I believe by a desire to obtain admission into hospitals, where they live tolerably well, and quite idly. It is easy enough, they think, to throw their legs and arms about, and to grin; and many of them get up a capital shew of foaming at the mouth, by placing a bit of soap between the gums and cheek. The means of detecting these vagabonds are of some importance to us all; and it is more particularly necessary that they should be well known to those who are likely to join the medical department of our fleets or armies.

It is of course desirable, in questionable cases, to witness a fit. But pretenders are not very willing to perform when they know that a medical man is looking on. You may sometimes convict them, in the absence of the fits, by cross-examination. A cheat will seldom be consistent in the account which he gives of his fits; as to whether they are regular or irregular; and as to the times and places in which he has suffered them. An impostor chooses such situations for his exhibition as are most suitable to his own purposes; a crowded street, or a well-frequented public walk. True epileptics almost always select retired places to take exercise in; especially if they have any warning or expectation of the approach of a paroxysm. You will find also that the impostor is not attacked at his own home; but always fixes upon some spot in which he is not only sure to be seen by others, but in which he is not likely to sustain any injury by tumbling down. True epileptics often get themselves seriously hurt by their falls; feigned ones generally come off without much bodily damage. However, if the fits are alleged to be frequent, and if also they are regular, you may soon expect one, and must make a point of being present; and then you will seldom fail to remove or to verify your suspicions. In the first place the muscular power of epileptics is far beyond what is

natural. It will sometimes take four or five stout men to hold a weak emaciated lad, in a fit of epilepsy. Of course no impostor can command more than his natural strength. In the second place a real epileptic fit, if it lasts long, is seldom violent; whereas impostors, for obvious reasons, make their fits both long and violent. You may often get much information from the state of the eyes, which in true epilepsy are generally partly open, with the eye-ball visibly rolling and distorted. In feigned epilepsy the actor almost always prefers to shut his eyes completely. Sometimes, if he be closely watched, and no suspicion is expressed, he will be seen to open his eyes occasionally, to ascertain the effect of his exhibition upon the bystanders. In real epilepsy, too, the pupils are often considerably dilated, and do not contract when stimulated by light. This is a very sure criterion; for no impostor can prevent his iris from acting on exposure to vivid light. The pulse, in true epilepsy, is not only frequent but often irregular also; a circumstance which never can be imitated. The skin of an epileptic, during the fit, is commonly cold; but that of an exhibitor is hot, and covered with sweat, obviously the consequence of his violent and voluntary exertions. In this respect, also, it is scarcely possible for him to deceive us. Again, an impostor will not bite his tongue, as epileptics often do; nor very willingly void (like them) his excrements and urine during the fit: indeed it would not be very easy for him to do so, and at the same time to carry on the necessary pretence of convulsions. Besides, epileptics, during a fit, are quite insensible to external impressions; and hence the vulgar modes of detection, though harsh and not to be recommended, are often effectual ones; such as dropping melted sealing-wax upon the patient; putting some gin in his eye; burning him with a hot poker; or (what I believe is more fashionable among beadles and police constables, when they have to administer to such patients) the pressing your thumb-nail violently under that of the supposed impostor. This causes exquisite pain, yet inflicts no lasting or serious injury; and I believe that few pretenders stand out against this expedient. It is astonishing, however, how much torture some of them will bear before they can be brought to confess their imposition. If we speak of having recourse to some of these painful tests in the hearing of the pretender, we shall find that the fit will soon come to an end. Dr. Cheyne mentions an instance in which one table was placed upon another, and a soldier, who was supposed to be shamming, was laid upon the upper one, while his paroxysm was on him; and the fear of falling from such a height soon stopped the convulsions. Mr. Hutchinson relates the

case of a sailor who was suspected to be a cheat, in whom the convulsions were instantly removed by blowing some fine Scotch snuff up his nostrils through a quill. This brought on *another* kind of fit, viz. a fit of *sneezing*, which lasted nearly half an hour; and there was no return of the epilepsy as long as Mr. Hutchinson remained in that ship. He tried the same expedient in cases of *real* epilepsy, but never could produce any similar effects, although the patients were not snuff-takers. There was a beggar in Paris, who often fell into epileptic fits in the streets: one day some compassionate spectators, fearing that he might injure himself in his struggles, got a truss of straw and placed him upon it; but when he was in the height of his paroxysm, and performing remarkably well, they set fire to the straw; and he presently took to his heels.

There is another ingenious plan, very likely, I should think, to detect an impostor, and yet not calculated, like the one last mentioned, to injure a real sufferer; which is, to propose gravely, in his hearing, to pour *boiling* water upon his legs, and then to proceed actually to pour *cold* water upon them.

Diagnosis.—Of the *real* diseases which are apt to be confounded with epilepsy, *hysteria* is the chief. The question whether a given case be one of epilepsy or of hysteria, very often arises. By a careful attention to several circumstances, the discrimination is generally to be made. In the first place the total suspension of consciousness, which is so constant an accompaniment of the epileptic paroxysm, does not take place in the hysterical: in epilepsy there is no globus hystericus, no alternations of laughter and tears: the solitary cry which ushers in the epileptic attack so frequently, and which is so characteristic, is not heard in hysteria; not that hysterical girls do not scream, for they often do; but then it is repeatedly and continuously. The heavy comatose sleep that succeeds epilepsy is not common in hysteria. Hysterical patients contrive also to avoid hurting themselves by their contortions: they do not bite their tongues, nor foam at the mouth. Dr. M. Hall tells us that, in epilepsy, there is a forcible closure of the larynx, and expiratory efforts which suffuse the countenance, and probably congest the brain, with venous blood. In hysteria the respiration, on the contrary, is rapid and sobbing.

Observe that I have been speaking, all along, of what has been sometimes called *habitual* epilepsy. It is not every attack of convulsions with insensibility which ought to be so named. Such attacks are apt to follow sudden injuries done to the brain; stunning blows on the head, fractures of the skull, the eruption of blood in sanguineous apoplexy, and even

overwhelming emotions of the mind. With these casual occurrences of epileptiform convulsion I do not here meddle.

Prognosis.—Epilepsy is one of those complaints concerning the *probable issue* of which the patient, and still more the patient's friends, are sure to make repeated and anxious enquiries. It is seldom that we can pronounce with any confidence a favourable prognosis; but there are some cases in which the prospect is much worse than in others.

If we have any reason to believe that the disease is *centric*, and connected with any organic derangement of the nervous centres themselves, the prognosis must be bad. *Cæteris paribus* it is rendered worse by the coexistence of any sign of scrofulous disease, or of the well-known bodily characteristics of the scrofulous diathesis: it is rendered worse, also, when the disease has occurred in the parents, or among the more immediate ancestors, of the patient; whenever, in short, there is reason to think the disposition to it is inherited. The prognosis is bad when the complaint occurs in persons who have slanting foreheads and misshapen skulls; and when the epileptic physiognomy has become established. The prognosis is always the more unfavourable the longer the disorder has lasted; the oftener the fits have been repeated; and the more habitual they have become. And when the memory is permanently enfeebled, or fatuity has come on, or the disease is complicated with any form or degree of paralysis, the case is hopeless; so far, at least, as a perfect cure is concerned.

On the other hand the prognosis is better when the disease is *eccentric*; *i. e.* when there is any obvious exciting cause of the paroxysms, manifest in structural or functional disorder of some part of the body other than the nervous matter. And when this eccentric cause is removable—a stone in the bladder for instance, worms in the intestines—then the prognosis still farther improves. On this account the prognosis is better in children than in older persons, for the exciting cause is often clearly eccentric, and likely to be transitory; the irritation of teething for example: and besides this, it is stated by many practical writers that even repeated and habitual attacks of epilepsy in children often go off as the patients grow older; and especially at the age of puberty. The experience of Heberden, however, was against this. He says that he had known several persons *become* epileptic at that time; but that he had never met with one who had *then* got rid of the disease. He had known a few who had recovered before, and some after the age of puberty. Dr. Elliotson mentions a case in which a girl had epilepsy prior to the first period of menstruation: then the fits stopped; and she remained free

from them until in advanced life the catamenia ceased to recur; and then the epilepsy returned. In all those cases in which we can assign some evident cause for the fit—such as the use of improper food, uterine irritation, mental emotion, and so on—the prognosis is somewhat better than usual. “The eccentric epilepsy (says Dr. Hall) is to be viewed as *curable*, however *difficult* of cure.” And however unfavourable the prognosis may be, there is nothing that can excuse any apathy or neglect of it on the part of the practitioner. Though few cases of habitual epilepsy admit of a *cure* under any treatment, yet there are few which may not be *relieved* by treatment, so far as regards the *frequency* or the *violence* of the fits, or *both*.

Treatment.—The treatment of epilepsy resolves itself into the measures to be adopted during the fit; and the measures to be adopted during the intervals between the fits.

During the fit.—In the paroxysm itself we have to provide against the risk of injury from the struggles and contortions of the patient; and if possible to mitigate the violence, and to shorten the duration of the fit. The patient should be placed in the centre of a large bed; his neck-cloth, and any ligatures about his person, should be loosened; his head should be somewhat elevated. When the risk of his hurting himself cannot be avoided in any other way, his limbs should be restrained by the bystanders, or secured in a waistcoat. Some persons have advised that a piece of cork or soft wood should be placed between his teeth, to prevent him from biting his tongue, or breaking his teeth. But it is not easy to manage this expedient cleverly. If the head be visibly congested, and hot, cold wet cloths may be applied to it with propriety; and if, at the same time, the extremities be cold, means of restoring warmth to them should be adopted.

I do not know whether art can abbreviate the paroxysm. Some years ago the late Barry O'Meara wrote a letter to some of the newspapers, saying that he fancied he had seen a popular remedy useful in such cases; that, namely, of cramming salt into the patient's mouth: he thought he had succeeded in bringing the patient about by that expedient. Of the epileptic patients that come into hospitals, the physician, not being always on the spot, does not see all, or even many of the paroxysms; but after reading that letter, I desired the nurses to treat all my patients who might be seized with epilepsy in the wards upon that plan: and upon comparing the length of the paroxysms when the salt was used, with their ordinary duration as reported by the friends of the patient, or as previously observed in the hospital during some of the earlier fits, it certainly

did seem to curtail the convulsions. Probably it is more calculated to relieve an hysterical than an epileptic fit. In the epileptic fits of children much benefit often results from immersing them in warm water: particularly if there be any coldness of the extremities.

It is very much the fashion to bleed persons who are seen in a fit, of whatever kind; and to bleed them largely. I have already given you my opinion of the indiscriminate use of this decided measure in apoplectic attacks. If it be clear, from the phænomena, or from the known history of the patient, that the case is one of epilepsy, bleeding, *during the fit*, will seldom be necessary or proper; unless, indeed, the evidence of cerebral plethora is very strongly marked: and even then I would advise you not to do more than take a moderate quantity of blood, by cupping, from the neck or temples. The convulsions and the sopor may be expected soon to pass off; as soon, probably, and as completely, without as with any abstraction of blood. Whereas the difference of the alternative is not trifling, in respect to the condition in which the patient will be left when the fit is over. The injurious effect of excessive blood-letting upon the system at large, is manifest, sometimes, for months afterwards.

During the intervals between the attacks we seek to prevent their recurrence; and this end is to be attained, when it is attainable at all, by getting rid of the predisposition to the disease, on the one hand, and by protecting the patient against its exciting causes on the other. Now there are certain kinds and causes of predisposition which we *cannot* get rid of: such are the tendency that is *inherited*; the strumous diathesis; malformation of the head; the presence of some organic lesion in the brain or spinal cord. Vicious and dissolute habits are also difficult, but not impossible, to eradicate. It will be our duty, when such are discovered, to set strongly before the unhappy patient the dreadful end to which he is hastening; the certain loss of reason to which, when once the disease has shewn itself, the continuance of his baleful indulgences will drive him; and to urge upon him the necessity for a short and sudden turn on his part, if he would expect any aid from medicine. Where no physical cause of the proclivity exists, or can be detected, it is of much importance to ascertain whether there be any deviation from the standard condition of health; towards general plethora in the one direction, or towards emptiness and asthenia in the other. The first of these unnatural states may be redressed by regimen and exercise, by abstinence from stimulating food and drink, and by a slender diet also; and, if need be, by

direct depletion. The second, which, perhaps, is the most common of the two, and which often leads (as I have explained before) to *local* plethora, may often be removed or lessened by a tonic treatment. The object in both cases is to give stability and firmness to the nervous system; to diminish that mobility, or readiness to be impressed, which is so strong a characteristic of the class of patients affected with epilepsy, although it may not be very apparent in some few individuals among them. It is upon this principle that *mineral* tonics sometimes do good in epilepsy, and not by any *specific* virtue which they possess in restraining the fits.

It is owing, perhaps, to a neglect of these two somewhat opposite conditions of general plethora and general debility, or to the difficulty that sometimes is met with in distinguishing these conditions, that such a variety of opinions have been expressed concerning the proper treatment of habitual epilepsy. Plethora is to be reduced without causing hurtful debility; tone is to be given without inducing dangerous fulness. It requires some nicety to carry the balance even; to attain the hoped-for good, and at the same time to avoid the evil that is apt to wait upon it. In very many cases the requisite extent and measure of the tonic plan on the one hand, or of the lowering system on the other, can only be learned by careful trials. But sometimes the indications of treatment are more plain. When the patient is young and strong, and full of blood, and not of a particularly moveable temperament; when he has a hard pulse, or any degree of feverishness; when the disorder has supervened upon the suspension of some customary discharge, so that there is an obvious cause of plethora; and when the disease is in its early stage, and the recurrence of the fits has not yet been established by habit; in any or all of these circumstances it will often be proper to abstract blood from the patient, and it will always be right to purge him actively, and to insist upon an abstinent regimen. When former paroxysms have been preceded by signs of fulness of the vessels of the head, by headache for instance, throbbing of the temporal arteries, distension of the superficial veins, a flushed or loaded countenance, you may sometimes, by a timely use of the lancet or the cupping-glass, avert an attack that was apparently impending.

On the other hand, if the patient is pale and weak; or unduly susceptible; or if his malady has been fastened upon him through many repetitions of the fit; you will generally find that any form of active depletion is injurious, and learn to place your best hope in measures which are calculated to invigorate the frame.

One of the most useful of the particular remedies employed for strengthening the body, is the cold shower bath. This tends, more perhaps than any other single measure, to give permanent firmness and stability to the system. The best test, in all cases, of the tonic and bracing effect of this remedy is the occurrence of a pleasant and general glow after each application of it. It is the only safe mode in which the cold bath can be used by an epileptic person.

You will find, in books, a great many tonic medicines recommended for this disease, which medicines you will have opportunity and ample time for trying. Of the mineral tonics, the salts of silver, zinc, copper, and iron, have been chiefly praised. The nitrate of silver used to be highly thought of; but there is one very serious objection to it which must never be forgotten: viz. that it is apt to produce a permanent discolouration of the skin, a frightful lead-colour. There is a footman in a house near Cavendish Square who has been thus blackened: and there is a gentleman of property resident at Brighton in the same predicament: his face looks as if it had been thoroughly and carefully pencilled over with plumbago. A barrister and friend of my own had a narrow escape from a similar misfortune: in fact his skin has acquired a just perceptible tinge of grey. Now if the remedy were sure to cure the disease, I am not certain that every one would accept of a cure on such terms. It would be proper, even on that supposition, to tell the patient that though he (or, *a fortiori*, she) would get rid of the epilepsy, there was a likelihood that this unamiable complexion might ensue. But the truth is that in giving this nitrate of silver we run a great risk of obtaining its disfiguring effect, for the sake of a very small chance of curing the epilepsy. I have been assured, by one of his friends, that the Brighton gentleman has carried a dark outside for a quarter of a century at least; and that he is as subject to epileptic fits now as ever he was. If the lunar caustic is to do good, it must be given for some time together, and the probability is that it will not do good even then; and if it be given for some time together, there is great danger of its changing the colour of the skin. For these reasons I never give it myself, and therefore I cannot recommend it to you. If you wish to try it, or if you have a patient that insists on trying it, as some will, you may begin with half a grain in a pill three times a day; and the dose has sometimes been carried as high as fifteen grains. And it is worth observing that in the larger doses this drug proves purgative: it is possible that its good effect, when it has any, may be attributable to its operation in that way.

There is no danger of spoiling the beauty of your patient by administering the oxide or the sulphate of zinc; or the cuprum ammoniatum. The liquor arsenicalis has been thought useful; but it requires to be exhibited with great caution. Of all the metallic remedies I should prefer some preparation of iron. I think I have seen much good done by the vinum ferri; not by any specific agency, however, but by its giving what is called tone to the nervous system, and rendering it less prone to be affected by the slighter exciting causes of the disease. I cannot pretend to weigh the merits of the long list of substances which have been lauded as efficacious in keeping off and curing the disease; and which, when they have been useful at all, have operated, I conclude, in diminishing the disposition to epilepsy by corroborating the nervous system. The most renowned of them are valerian, asafoetida, wormwood, the misletoe of the oak, the cardamine pratensis, rue, the sedum acre, indigo; narcotic vegetable preparations, stramonium, belladonna, hemlock, lettuce; animal substances, musk, castor, ox-gall; and the number might be many times multiplied: and this long array of drugs, all of which have been known, or supposed, to accomplish a cure, affords, in truth, one of the strongest evidences of the intractability of the disease under any plan of treatment. There is a shrewd remark of Esquirol's, which I believe to be quite true, however difficult it may be to account for the fact, which is, that epileptics are apt to improve for a time under every new plan of treatment.

Whatever drug you may see reason to prefer (and the patients will have drugs, and you must be prepared to ring the changes upon them), there are certain other points in the management of the disease which are of considerable importance. The patient who is subject to epilepsy should live by rule, and be temperate in all things. His diet should be simple, nutritious, but not stimulating: he should renounce all strong liquor, and become, in the new-fangled and vulgar phrase, a teatotalter. He should rise early, and take regular exercise in the open air; keeping his head cool, and his extremities warm. He should avoid all mental excitement, and the fatiguing pursuit of what is called pleasure; all probable sources of sudden anger, surprise, alarm, or deep emotion of any kind; all striving and contention of the intellect. The student, of whatever age and sort, in whom epilepsy has declared itself, should shut his books, the man of business abandon or abridge his professional toil: at least they must be instructed to abstain habitually, in their respective callings, from such application as would task and strain their powers.

whether mental or bodily; and endeavours should be made to engage their thoughts and to interest their minds in less engrossing objects of attention. No minute rules can be laid down on these points, but, keeping the general indication in view, it will seldom be difficult to follow it up in practice.

When the fits appear to have been brought on by a species of moral contagion, or by imitation of the same disease seen in others, care should be taken to exclude as much as possible those objects or trains of thought which produce the mental emotion or the morbid propensity. In these cases, and, indeed, I may say in almost all cases, it is more rational to expect benefit from such measures as tend to calm the mind and to fortify the nerves, than from this or that substance thrown at random into the stomach.

There is sometimes cause for suspecting that epileptic fits depend upon a syphilitic affection of the bones of the skull; I am much mistaken if I have not seen such cases. When that suspicion arises, it will be proper to give mercury a full and fair trial. Such a plan has been followed by success. I should always premise, however, in such cases, the iodide of potassium; the efficacy of which in getting rid of syphilitic nodes is no longer doubtful. I am accustomed to recommend a gentle and long-continued course of mercury whenever organic disease of the brain is suspected; the influence of that remedy being carefully watched. It will be right and proper also to try the effect of counter-irritation; of blisters, a seton in the neck, or the tartar emetic ointment. But I must confess to you that, often as this expedient is employed, I have seldom witnessed any such result from it as would encourage me to expect benefit from repeating it in another case. There is one form of counter-irritation which I have never seen put to the test, but which has of late been strongly recommended by a very able and observing physician, Dr. Pritchard; and which I have heard very good accounts of from a gentleman who had seen it extensively employed in Bristol; I mean the making a long issue in the head itself, dividing the integuments down to the bone by means of a scalpel in the direction of the sagittal suture, and keeping the incision open and discharging for some time, by means of issue peas. The formation of the issue is said to be not so painful as one might suppose.

Dr. Quain, in his edition of *Martinet's Pathology*, relates the following case:—"Some years ago I saw a boy who was epileptic from infancy, and who, in one of his usual fits, fell over a cliff by the sea-side, and received a very severe lacerated wound

of the scalp, which healed slowly and with a copious suppuration. While the discharge continued he was free from any epileptic attack; but as soon as the wound healed, the fits returned as before."

Twice I have seen similar good effects from the insertion of a seton in the neck. Twenty times that measure has disappointed my hopes.

When the disease is ascertained or believed to be of the eccentric kind, we must search diligently to find the seat of the distant irritation, in some disturbance of function; and apply our remedies accordingly. The irritation may be found, as I have already intimated, in almost any organ of the body. Painful or irregular dentition is perhaps one of the commonest of the eccentric sources of epilepsy. Sometimes the attacks are attended with symptoms of disease in the liver; slight yellowness of the skin, uneasiness and tenderness of the right hypochondrium, and lowness of spirits. In such a case we must rectify that state of the liver, by such means as I shall have to specify hereafter. If the disorder depends on a stone in the bladder, the cure must be committed to the surgeon. I have a patient at present under my occasional inspection, who from time to time has slight fits of epilepsy; on most occasions he passes about the same time a small calculus by the urethra. I make no doubt that in his case the exciting cause of the epilepsy lies in the kidney.

You will find that most persons, in respect to such diseases as that which we are now considering, have some favourite or usual mode of treatment; and if I were called upon to name any single drug, from which, in ordinary cases of epilepsy, I should most hope for relief, I should say it was the oil of turpentine. And I find that other persons have come to the same conclusion. Dr. Latham the elder was, I believe, the first person who made known its efficacy in this disorder. Foville states that he has seen excellent effects from it. It is highly spoken of by Dr. Percival, in the *Dublin Hospital Reports*. It is not to be given in large doses, but in smaller ones that are frequently repeated; from half a drachm to a drachm every six hours. You are aware that it sometimes produces strangury, and therefore the patient must be forewarned of this, or carefully watched. Occasionally turpentine has done good in virtue of its anthelmintic properties. I know that a physician of my acquaintance cured a case of epilepsy in this way, somewhat to his own surprise. Without having in his mind any notion of worms, he thought it might be well to purge his patient, who had laboured under epilepsy for some time, with the oleum terebinthinæ. The patient, who is the brother of a person

holding at present a high office in this country, was residing two or three miles out of town. In the middle of the night the Doctor was summoned to him in a great hurry; the messenger said he was supposed to be dying. He was only intoxicated, however, by the free dose of turpentine he had swallowed: the next morning he voided into the close-stool a large tape-worm; and he has never had epilepsy since. The late Lord Hardwick, the father of the present Earl, had epilepsy, and he too got rid of his epilepsy and of a worm at the same time. I believe that the cure was effected by turpentine in his case also; but I am not certain of that. Such cases are remarkably interesting: they shew that irritation of the stomach or intestines may be sufficient to cause the fits; they illustrate excellently well the eccentric form of the disease; and they deserve to be always borne in mind when we are asked to prescribe for an epileptic patient. A cure from so dreaded a complaint, by such simple means—the cause of his malady, and the certainty of his having got rid of that cause, being both so obvious and intelligible to the patient—may be enough, sometimes, to make a practitioner's fortune. But I think you will sometimes find the oil of turpentine very useful, even though it expels no worm, and when there is no worm to expel. If the bowels should be costive, the oil of turpentine and castor oil, in equal proportions, go exceedingly well together.

During the warnings.—When the patient has a distinct warning of an approaching paroxysm, can any thing be done to ward it off? Why, in some cases, by interrupting the precursory symptoms, it certainly may be prevented. A pupil of the class informs me that a brother of his, 12 or 13 years old, has been subject to epileptic fits for two years. They occur in the night, especially if he is waked, even though the awakening cause has no tendency to startle him. He often is dull and drowsy the evening before, and if he is roused from this lethargic state by conversation, or amusements, the attack expected that night sometimes does not happen. Another student knows a young girl, in whom the occurrence of very high spirits is always precursory of the paroxysm: when this extreme vivacity is moderated by those about her, the threatened fit is sometimes averted. I mentioned before an instance in which the aura, proceeding from one of the thumbs, was frequently checked by tying a ligature tightly round the thumb. Other examples of exactly the same kind are on record. Mr. Wardrop cured a case, beginning with aura in one finger, by amputating a joint of the finger. Dr. M. Hall states that the immediate accession of the paroxysm may some-

times be prevented by dashing cold water on the face, or by exciting the nostrils by snuff. In this manner the disposition to closure of the larynx and to expiratory efforts is exchanged for sudden acts of inspiration. Another patient of my own, an old College friend indeed, who is afflicted with epilepsy, feels convinced that he sometimes prevents a fit by applying smelling salts to his nose; and he always carries a bottle about with him for that purpose: but, unfortunately, the warning (which consists chiefly in giddiness) is generally so short, that he has not time to have recourse to his preventive before he falls down. It is a question whether the fit may not be obviated by a strong mental effort in some cases. I make no doubt that it may, especially in the imitative form of the disease, which originates in and depends upon mental and moral causes.

It is scarcely necessary that I should do more than advert generally to those precautions which every one who is subject to epilepsy ought to observe, and which it is the business of his medical adviser to enforce both upon the patient himself, and upon his friends. His bed should be large; or if not large it should be enclosed with some netting or other defence against his falling out of it. If he sleeps in a room by himself, care should be taken that, in the winter, a proper temperature is kept up, for if in his attacks he gets out of bed on the floor, he may be seriously injured by the cold. He should not, however, be left alone if it can be helped. Guards should be placed over every grate near which the patient may come; he should avoid ascending and descending stairs as much as he can; he should not ride on horseback, nor on the top of a coach, nor even in a gig; nor go about, especially in solitary places, without an attendant. A patient of Dr. Cheyne's, a young man of 20, was drowned in his own garden by falling into a little runnel of water which was not four inches deep. Neither, on the other hand, will it be proper or safe for him to frequent crowded or hot rooms; or the streets of a populous town, in which the multiplicity and distraction of objects are apt to produce, even in a healthy person who is not accustomed to them, a degree of vertigo and confusion. Dr. Cheyne advises that when the patient's circumstances will admit of his having a constant attendant with him, the latter should be provided with some diffusible stimulus; a potion, for example, composed of camphor mixture and æther, by swallowing which the impending paroxysm may sometimes be repelled.

ON
CERTAIN PHYSIOLOGICAL INFERENCES
WHICH MAY BE DRAWN FROM THE STUDY OF
THE NERVES OF THE EYEBALL.*

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It has been justly observed that the great discovery of the appropriation of the different portions of the nervous system to the exercise of the different functions, would never have been clearly established, but for the fortunate circumstance that, in certain parts of the body, especially on the face, the nerves of sense and of voluntary motion are distinct throughout their whole course. And this consideration may instruct us that, when we have an organ supplied with a variety of nerves, known to be of perfectly different endowments, the study of the peculiarities of these nerves may give us an insight into the purpose or use of some of those pieces of structure in all parts of the nervous system, in which we must still admit that we see much contrivance, without understanding its intention.

In the case of the eyeball, it is generally allowed that we see, separated for us by nature, almost every kind of nerve which the physiology of any part of the body includes; we have the nerve of special sensation, and that of common sensation; we have the nerves which excite motion in obedience to the will, and those which excite motion over which the will has no control; we can point out the incident nerve and the efferent nerve, concerned in two distinct examples of the reflex function of the spinal cord; and we can specify the nerve by which the nutrition of the whole organ, and more than one secretion contained in it, are liable to be influenced and controlled. And when we attend to the peculiarities of these nerves, and to facts which have been observed in regard to their action, I think we have sufficient data for certain inferences applicable to other parts of the nervous system, which have not yet been distinctly pointed out, and which are steps in the progress of that most difficult, but likewise most interesting department of physiology, where our object is to detect the laws by which mental acts are connected with the physical changes of living beings; and where, accordingly, the intimations of our own consciousness must be admitted as part of the foundation of our inferences.

I. The first peculiarity in the nerves of the eyeball to which I wish to direct attention is this, that those supplying the muscles by which the eyeball is instinctively or volun-

tarily moved are, if not wholly, (as Scarpa and others have maintained), at least almost entirely devoid of any of those filaments which we now regard as the organs of common sensation; the straight and oblique muscles having their nerves from the 3d, 4th, and 6th, to the almost complete exclusion of the ophthalmic branch of the 5th*.

I think we cannot doubt that the reason of this peculiarity, by which the muscles of the eyeball are distinguished—perhaps from every other muscle in the body, viz. the absence of purely sensitive filaments in their composition—is that already assigned by Van Deen†, viz. that these muscles are intended to be regulated and guided in their contractions, not by sensations excited in their own substance, or in parts directly in contact with them, but by the sensations of the retina; and I think farther, that to this peculiarity we are to ascribe, both the positive fact, that the movements of these muscles are naturally *consentient* in the two eyes, so as to preserve the parallelism of the optic axes; and likewise the negative fact, that we have hardly any power to insulate an act of the will on one of these muscles, so as to move the one eyeball in a different direction from the other; *i. e.* the left eye, for example, turns inwards when the right eye turns outwards, because both are habitually guided by the sensations of the retina, which are similarly affected by these movements of the two eyes; and we have little power of moving either eye independently of the other, because we have hardly any sensations, consequent on the movement of the one eye and not of the other, whereby to guide the efforts of the will for this purpose‡. And this con-

* “Certum et inconcussum ut,” says Scarpa, “quinti nervorum cerebrorum ophthalmicum, orbitam transgradientem, ne minimum quidem filamentum valde conspicuis cæteroquin nervis oculum moventibus addere.” (De Gangliis, &c. Isis, 1832)

† De Differentia et Nexu inter Nervos vitæ animalis et vitæ organicæ, p. 162.

‡ It has been stated by Sir Charles Bell, that he believes the 3d nerve to be sensitive as well as motor, because it has an origin from behind as well as from before the grey matter of the crus cerebri; and although the examples of the portio dura and the spinal accessory nerves (which appear to be purely motor, although originating in part from the posterior portion of the cord) render that inference doubtful, yet I am bound to admit that, according to the statement of Valentin (a), there is experimental evidence of sensations being felt on irritation of the 3d nerve. But this author is equally confident, from experiment, that there is no sensibility in the 6th nerve (b); and it should be remembered that movements are often performed by the 3d nerve,—such as rolling the eyes inwards, and raising the eyelid,—which are not prompted by the sensations of the retina, and for the regulation of which sensations in the moving parts themselves may therefore be required.

(a) De Functionibus Nervorum Cerebrorum, &c. p. 16.

(b) Ibid. p. 30.

* From the Transactions of the Royal Society of Edinburgh, Vol. XV. Part 1.

sideration suggests some important reflections on the *office of sensitive nerves and of sensations* in regard to all movements of voluntary muscles.

It appears to me, notwithstanding some difficulties recently raised, that the essential peculiarity of all strictly animal motion is, that it is motion dependent more or less directly on sensation; that if we are certain of any movement in an organized body being altogether independent of sensation, and affording no indication of any mental act, we should refer it to the same class as movements in vegetables; and that in designating such movements as organic, but not animal, we express a distinction of essential importance in physiology.

It has indeed been lately maintained by several eminent physiologists, who have studied the indications of what is now called the reflex function of the spinal cord, that many living actions, such as respiration, deglutition, coughing, sneezing, and vomiting, the evacuations of the bowels and bladder, and even the movements by which irritations of the surface are avoided or repelled,—certainly attended in the natural state by sensations, and usually thought to indicate sensation, and therefore to belong to the department of animal life,—are independent of sensation, and ought, therefore, according to the principle above stated, to be referred to that of organic life. But although it is well ascertained that movements may be excited in perfectly paralytic limbs, by irritations applied to the surface, which must be carried back to the sensitive, and cross from thence to the motor portions of the spinal cord connected with those limbs; and therefore that the whole series of nervous actions, which takes place when any of these reflex or sympathetic actions are excited, may be in some degree imitated by mechanical irritation of the nervous matter, independently of sensation; yet, when it is inferred from this fact that, in *the entire and healthy body*, sensation does not intervene, as a part of the sequence of cause and effect, on which such actions depend, this theory overlooks so much of what has been formerly ascertained and pointed out in regard to them, that I do not think we can expect it long to hold its ground in physiology.

The movements which are excited by irritation of the sensitive nerves, in the *undoubted* absence of sensation (which of course can only be known in the human body in the state of disease), are general and irregular, and have not the character of *selection and adaptation* to particular purposes, which is essential to the useful application of any such actions in the living body. And when it is supposed that such movements as respiration, coughing, or deglutition, are equally independent of sensation, we

not only overlook this, their essential character, of selection of individual nerves and adaptation to particular ends, but disregard the following facts, long ago stated in evidence, that sensations intervene in the process by which they are excited.

1. In various cases, impressions on the sensitive nerves of *different* parts of the body excite *the same* sensation, and then *the same* reflex or sympathetic action follows,—as when intense nausea results from changes whether in the brain, fauces, stomach, bowels, liver, or kidneys, and is in each case followed by the same act of retching,—or when a full inspiration follows the dashing of cold water on the face, breast, abdomen, or extremities.

2. Conversely, in various instances, different impressions made on *the same* parts of the body, and therefore on the same sensitive nerve, exert *different* sensations, in which case they are *not* followed by the same reflex actions. Thus certain impressions on the nostrils and face, followed by the sensation of cold or of tickling, excite the act of inspiration; but other impressions on the same parts, fully as strongly felt, but exciting different sensations, as in cutting or bruising, have no such effect; and the same is remarkably observed as to different impressions on the fauces and on the stomach, some of which excite nausea and *then* retching, while many others have no such effect. These facts plainly indicate that, in the natural state, the reflex actions, characterized as above stated, follow not the impressions on particular nerves, but the excitement of particular sensations. And it is easy to shew that many phenomena seen during sleep, or in decapitated animals, (when the medulla oblongata has been left in connexion with the cord), and which have been thought indications of even well-regulated reflex movements, independent of sensation, may be reconciled to the same doctrine, if we remember that sensations may be quite distinct, but momentary, and so leave no trace on the recollection.

Then it is to be remembered, that several of these reflex actions are absolutely identical with those which are excited by emotions and passions, *i. e.* by changes which are peculiar to the mental part of our constitution, as in the cases of sighing, weeping, laughing, even retching and vomiting; and again, that they are observed to be remarkably obedient to well-known laws of mind. Thus they are, like the strictly voluntary action, obedient to the law of habit, which, as applied to the mental changes preceding muscular contractions, is merely the law of association of ideas; and they are so effectually controlled by the occurrence of any very engrossing mental act,—sensation, emotion, or voluntary effort,—as plainly to imply, that

they are not only attended by the consciousness, but modified by the agency, of the mental part of our constitution.

I stated and illustrated these facts, chiefly by commenting on the writings of Whytt and Monro, before the offices of the brain and the cerebellum, in animal motion, had been clearly distinguished from those of the spinal cord*; and it does not appear to me that their force is in the least impaired by the facts which have been since ascertained, touching the portions of the nervous matter with which sensation, or recollection, or any other mental act, is especially connected.

The case now before us, however, is one in which we see exemplified, not merely the power of sensations, directly, or through the intervention of other mental acts resulting from them, to *excite* muscular motions, but more especially their office in *guiding and regulating* those muscular actions which are excited through the nerves. The difference between the muscles of the eyeball and other muscles of the body, in the respect above stated, illustrates perfectly the importance of the sensitive nerves of muscles, whether these are bound up with their motor nerves, as in most parts of the body, or separated from them, as in the face; and the importance of those muscular sensations, excited by the contraction of muscles, on the efficacy of which, as a means of acquiring knowledge, the late Dr. Brown dwelt with so much earnestness and ability, but perhaps with somewhat exaggerated ideas.

The office of the sensitive nerves of the voluntary muscles in general, and of the retina and the optic nerve in the eye, in regulating the animal motions, is obviously to furnish the sensations by which the mind is guided, in selecting the muscles and portions of muscles, and in determining the degree of contraction which is requisite for the attainment of any object. And of the necessity of such a regulator in the case of the eye, we have an instructive example when one eye is affected with amaurosis, the effect of which is to prevent that insensible eye from following accurately the movements of the sound eye, when turned in different directions, and thus to cause occasional and temporary distortion. In fixing on the muscles, or portions of muscles, on which it must act, when it feels certain sensations, in order to attain certain objects, the mind sometimes merely yields to that mysterious impulse, independent both of experience and of reasoning, to which we give the name of instinct; but in the greater number of cases, in our species, it is guided by experience and education. The sensations which result from any particular muscular action are recollected; and it is the anticipation, or rather

I believe we should say the *commencing recurrence*, of these sensations, which determines the repetition of the action. Thus the faculty of memory is essential to all strictly voluntary, as distinguished from instinctive, movements; and the experiments of Flourens and of Hertwig instruct us, that it is the cerebellum, not the brain proper, which furnishes the physical conditions requisite for this recollection of muscular sensations.

Although there appears at first some difficulty in understanding how sensations which are only *anticipated*, or the *beginning* of which only is felt, can guide the contractions on which their perfect recurrence is to depend, we shall have no difficulty in conceiving this, if we recollect that it must necessarily be precisely in the same manner that a musician is enabled to go over any piece of music from recollection;—the anticipated sensation is, throughout that operation, the guide to the motion by which its own recurrence is to be secured.

In the performance of any such complex successions of muscular movements, we must allow that it is difficult to conceive that there is not only a continual transmission *downwards*, perhaps to different parts of the body, of certain definite nervous actions resulting from efforts of the will,—by motor nerves,—but likewise at least as many transmissions *upwards* by the sensitive filaments, of changes produced by the movements excited,—sensations thereby felt,—and mental determinations consequent on these, by which the successive volitions are guided. But it is admitted that, in all sciences, “reason can sometimes go farther than imagination can venture to follow;” and in no department of science can we more reasonably expect to meet with such examples than in tracing the actions of that exquisite mechanism, by which the sensations and powers of living animals are placed in connection with the world which is given them to inhabit.

But we may go a step farther, and understand more distinctly the mode in which sensations continually regulate and guide muscular actions, if we reflect on the phenomena to which Müller has very properly directed the attention of physiologists under the name of *consentient motions*, and of which the study of the eye furnishes us with some of the most instructive examples.

I need hardly say that this term is applied in cases where different nerves, and thereby muscles, are excited to action *simultaneously*, and where it is difficult or impossible to separate the combination. Such cases occur very frequently, both as to the strictly voluntary and the sympathetic or reflex movements, but especially as to the latter; and the following are the facts most important to be observed in regard to them.

* See Edinburgh Medico-Chirurgical Transactions, vol. ii.

1. The strictly voluntary motions thus simultaneously performed, are chiefly where the action that is willed requires considerable exertion, and is performed with difficulty. "Thus when we wish to contract the muscles of the external ear, we induce contraction of the occipito-frontalis muscle at the same time, without wishing it. During the most violent muscular action, many muscles act by association, although their action serves no apparent purpose. Thus a man making much exertion moves the muscles of his face, as if they aided him in lifting a load," &c.

2. In regard to most of the cerebral motor nerves, and nerves moving the trunk of the body, particularly when these act in obedience to sensation or emotion, the most important fact regarding their consentient action is, that this tendency is observed especially in *the opposite nerves of the same pairs*. Thus in the latter description of movements performed by the irides of the eyes, by the muscles of the face, by the pharynx, diaphragm, intercostal muscles, abdominal, lumbar, and perineal muscles,—in the actions of winking from bright light, of deglutition, breathing, coughing, sneezing, vomiting, laughing, sighing, weeping,—straining for evacuation of any of the viscera of the abdomen or pelvis,—it is certain, and is essential to the due performance of each action, that the corresponding portions of the nerves of the same pair, on each side of the body, should be affected, and should act on the muscles, exactly alike; and this is observed, even when the sensation exciting the movement is felt only through one nerve, and on one side of the body; as in the contraction of both pupils from bright light acting on one eye, or in the simultaneous and successive contractions of all the muscles of respiration, in consequence of a sensation excited in one of the nostrils, or one of the bronchiæ.

3. Another fact as to these consentient movements is satisfactorily observed *only in the eye*, but is no doubt extensively applicable in many parts, viz. that the stimulus of this consentient movement of voluntary muscles passes *through the ganglia*, and thereby affects muscles of strictly involuntary motion; the iris being distinctly observed to contract whenever the eyeball is voluntarily and forcibly rolled inwards by the action of the third nerve. And Müller relates experiments in his own person, distinctly shewing that this effect takes place even on the pupil of the right eye, in consequence of forcible voluntary exertions made through the third nerve of the left eye, and when the right eyeball is not moved.

I think it impossible to doubt that Müller is so far right in ascribing these phenomena to what he calls "the conducting power of the cerebral substance at the origin of the nervous fibres, whereby those which are con-

tiguous to each other are liable to be affected simultaneously, and the influence of the will (or of any mental act) is with difficulty confined or insulated on individual fibres," or something is required to insulate it; and that these observations put us in possession of an important fact regarding the influence, either of volition or of sensation, or of the changes in the nervous matter attending these mental acts, in exciting muscular action, viz. that this influence naturally extends to some distance in the larger masses of the nervous matter, and requires the action of some additional cause, to *insulate* it on individual muscles, or portions of muscles. And in so far as the motor influence dependent on sensation is concerned, this is strictly in accordance with what is observed as to the imitation of that influence, in experiments on the reflex function in paralyzed or decapitated animals.

I think Müller is also certainly right in supposing that the tendency to consentient movement in the similar or corresponding portions of any pair of nerves, is the reason why the third nerve is not employed to give the movement outward to the eyeball; two other nerves (the fourth and sixth) being employed to give this movement, because it is a movement which must always be consentient with that excited in a dissimilar part, and therefore through dissimilar nerves, on the other side of the body. And although this tendency to consentient motion is much less seen in the nerves of the same pair going to the extremities, yet Müller justly observes, that the extreme difficulty always felt in rotating one arm in one direction, and the other in the opposite at the same moment, must be ascribed to the violation implied in that effort of this tendency to consentient action in the corresponding portions of the same pairs of nerves.

But I think it also certain, particularly from what we see in the eye, that this observation goes but very little way in explaining the general phenomenon of consentience. The tendency to consentient action in the nerves of the same pair in any part of the extremities, is so slight, as to show that the conducting power at the origins of these nerves cannot be very strong, and, therefore, that proximity of origin can afford but a very imperfect explanation of the very strong tendency to consentience remarked in almost all the motions of the trunk of the body. Consciousness informs us that, although it is very difficult to act at the same moment on dissimilar portions of the same pair of nerves, yet there is in general no difficulty in refraining from acting at the same moment on the corresponding portions; and in no case any difficulty in acting, at the same moment, on dissimilar and distant nerves. And there are facts observed in the eye, which have

quite the value of the *experimentum crucis*, as showing that the chief cause of consentient movement in our muscular organs is very different from the connection of nerves at their roots or in their course. These facts are, that while those corresponding portions of the third nerve, which elevate and depress the eyeball, *i. e.* those which go to the superior and inferior recti, always act simultaneously, those which go to the rectus internus and inferior oblique do not usually act together in the two eyes. Again, the fourth and sixth nerves never act together on the two sides of the body, but each is uniformly combined in its movement with a portion of the third on the other side. The reason obviously is, that the *sensations* which result from the action of the fourth and sixth nerves of the one eye, cannot be identified with those which result from the action of the nerves of the same pair in the other eye, and cannot be separated from those which result from the action of that portion of the third pair in the other eye. There is no other circumstance, but the identity of the resulting and guiding sensation, which can be pointed out as existing where the consentience is observed, and not existing where it is not observed.

From these facts, therefore, we learn that the main cause of consentience of muscular movement is simply *identity of the guiding sensations*. Whether it is by an original instinct, or by repeated trials and acquired experience, that the acts of volition are directed to the nerves in each eye, which so turn the eyeballs as to keep the optic axis parallel, and so produce the single sensations, is a different question; but what has been stated seems to me quite enough to show, that it is because the single sensations result, that these nerves are consentient.

I have no doubt that this principle, deduced from the movements of the eyeball, is strictly applicable to all the cases of consentient movement excited by the nerves of the same pairs on the face, fauces, thorax, abdomen, and pelvis, in the different actions which have been already mentioned. The movements which these nerves excite are always followed by certain sensations, generally grateful, influenced by the degree in which the actions are performed; and by these sensations, the extent to which the actions are carried, and the energy with which they are performed, are felt to be habitually regulated. These resulting and guiding sensations are felt to be affected exactly alike by the movement which is excited on both sides of the body; and hence we instinctively carry the movement to the same extent in both.

It was a speculation of Darwin, that the actions of inspiration and expiration are originally determined by the uneasy sensation

of anxiety in the chest of the new-born child, leading to irregular and convulsive movements, out of which those are quickly *selected*, which are found by rapid experience to be effectual in appeasing that uneasy feeling; and although I do not agree to this statement, as expressing the order of events at that early period of life, and can assign no cause but instinct for the original selection of the proper nerves and muscles for this purpose, yet I believe that, at all periods of life, it is the sensation felt to result from the action of inspiration already in progress, which determines the energy with which it shall be performed, the extent to which it shall go, and even the number of muscles that shall be excited to partake in it.

And that this is the true account of the matter, we have farther and satisfactory proof, in the fact that in various cases of disease, particularly in cases of empyema, the contractions of the muscles of inspiration on one side of the chest become ineffectual for inflating the lungs, and for appeasing the sense of anxiety in the breast; in which case their nerves are no longer excited, and those muscles cease to act; they remain flaccid, and even, according to the observation of Dr. Stokes, they gradually become paralytic from inaction; a phenomenon, as I conceive, almost exactly similar to the loss of power in some of the muscles of the eyeball in cases of amaurosis affecting one eye.

II. Again, another important application of the information acquired by study of the nerves of the eyeball, is to explain the use of the plexuses or analogous contrivances, through which all the nerves, sensitive and motor, pass both to the upper and lower extremities, very generally in the animal kingdom.

In regard to the use of this very remarkable piece of structure, found in those nerves, by which the most forcible and the most nicely regulated muscular movements are effected, there have been various opinions. Several authors, among others Sir Charles Bell, have supposed it to be intended to facilitate the combinations of different muscles for particular actions, proceeding on the plausible supposition that, when the will acts simultaneously on several muscles, its influence proceeds from a single point, and is diffused from thence to those different muscles.

“The principal cause of the irregularity and seeming intricacy in the distribution of nerves, is the necessity of arranging and combining a great many muscles in the different offices. Wherever we trace nerves of motion; we find that before entering the muscles they interchange branches, and form an intricate leash of nerves, or what is called a plexus. This plexus is intricate in

proportion to the number of muscles to be moved, and the *variety of combinations* into which the muscles enter; while the filaments of nerves which go to the skin regularly diverge to their destination. From the fin of a fish to the arm of a man, the plexus increases in complexity in proportion to the variety or extent of motions to be performed by the extremity. By the interchange of filaments, the combination among the muscles is formed; not only are the classes of extensors and flexors constituted in the plexus, but all the varieties of combinations are there formed, and the curious relations established which exist between opposing muscles, or rather between the contractions of one class and the relaxation of another." In short, it appears to be his idea, that a plexus is necessary to enable a single effort of the mind to throw into action a combination of muscular contractions, and a succession of efforts to excite such a succession of these combinations as exists in every complex movement.

But the case of the muscles of the eyeball seems quite sufficient to set aside this opinion. None of these nerves on the opposite sides of the body are connected by plexuses, yet no nerves can combine their actions more perfectly or more surely. There is no more perfect consentience in the living body than that between the sixth nerve of the right eye, and the inner portion of the third of the left, and both are often exerted in varied combinations with many other nerves and muscles; but no nerves in the body can have less connection, so far as anatomy informs us, either at their origin or in their course.

In fact, when we reflect on what passes within us when we throw into action any two muscles at the same moment, we shall see that when such a voluntary effort is made, it is just as easy for us to excite simultaneously the most widely distant or the most closely contiguous muscles; and again, when we attend to the necessary *selection* of so many different and distant muscles, in any of the requisite combinations which are apparently under the influence of sensation, as in coughing, sneezing, vomiting, &c. we shall perceive that, in the entire state of our faculties, any intense sensation may be said to have at its command all the muscles of the body; and although, as I have stated, I believe all mental acts to be guided by sensations in the selections which they make, yet I think it quite plain that neither proximity of origin, nor connection in their course, can be assigned as the cause of any of these selections.

I believe that Dr. Monro made a nearer approach to the true statement of the use of a plexus, and put it in a simpler view, when he said, that "the chief intention of nature in this very solicitous intermixture of the

nervous fibrils, is to lessen the danger by which accidents or diseases affecting the trunks of the nerves would, without these contrivances, have been attended. Thus let us suppose that two nerves are sufficient to supply the flexors and extensors of the forearm, it is evidently better for us that the one-half of each nerve should go to the flexors, and the other half of each to the extensors, than the whole of the first nerve should have gone to the flexors, and the whole of the second to the extensors. For if by accident or disease one of these nerves should be cut across, or lose its powers, we should, on the first supposition, preserve one-half of the powers, both of flexion and extension, which would surely be preferable to our possessing fully the power of flexion without any power of extension. And thus, in the arm, where five trunks are found, there would on this supposition, as to the use of a plexus, be only one-fifth of the power lost, of performing any motion, by division of any one of these nerves." (Obs. p. 45.)

That this is really the effect of this arrangement in regard to the effects of injury, appears to be sufficiently established by the experiments of Panizza on frogs, in which animals the plexus supplying the inferior extremities is much less intricate than in the mammalia. "If," he says, "one anterior root of the three last spinal muscles be cut, the motions of the corresponding extremity are as perfect as if the motiferous nervous system of the part had not been injured. Even if two roots be divided, although for a moment the motions are not so energetic as at first, yet they are speedily renewed, and the frog springs as if it had suffered no injury. Yet, by this operation, more than two-thirds of the nervous matter which presides over the motion of the extremity is destroyed; and if the third filament is divided, all motion immediately ceases in the limb." "Whence, if I am not mistaken, appears the use of the nervous plexuses, which, by the intermixture of the filaments of different roots having a common function, establish among them, as it were, such a concentrated force, that each is adequate to preserve the integrity of the function, when, by means of any harm, the continuity of the other filaments is interrupted." (Edin. Med. and Surg. Journal, No. 126, p. 89.)

I am aware of experiments by Cronenberg and by Müller, who found that by cutting one of the nerves entering the crural plexus in the frog, they could paralyze or greatly enfeeble certain movements of a limb, and leave others unimpaired; and of the elaborate investigations of Müller and others in Germany, which lead to this conclusion, that every nervous fibril, whether passing through a plexus or not, remains perfectly distinct

from its origin to its termination. Notwithstanding these observations, it is distinctly admitted by Müller, that "plexuses convey to each muscle of a limb fibres from different parts of the brain and spinal cord."

It seems to me, however, hardly possible to suppose, that this very carefully adjusted piece of structure is intended merely as a guard against injury, and therefore is of no use in any person or animal on whom such an injury as the section of one of the nerves of an extremity has never been inflicted. But if we advert to what has been said already of the evidence that any voluntary effort, which excites a muscle to contraction, extends its influence over *a considerable portion* of the cerebro-spinal axis, and at the same time to the evidence, in the experiments above quoted, that every muscle supplied from a plexus has part of its motor nerves, and may be excited to contraction, from each of the nerves entering that plexus, we can hardly miss the conclusion, that this contrivance not merely provides against injury, but *multiplies the power* which acts on each of these muscles, and enables the mind to *vary* the degree of energy which it can expend on each, in a degree much greater than in any case where it can act on a muscle only from a single point of the spinal cord.

Then, if we remember farther, that, by means of the plexus, each *sensitive* nerve which supplies any muscle of the extremities, consists of fibrils coming from different points of the cord, we can easily perceive that, by this arrangement, the sensations resulting from each portion of the muscle may be more distinct, and more easily discriminated from each other, than those which are excited by nervous fibrils bound in the same sheath throughout their course, and originating beside each other in the cord.

Thus the effect and use of a plexus will be, to make the muscular sensations more precise and distinct, and to make the power which the will can exert over the muscles greater, and capable of greater increase at pleasure, than where such arrangement does not exist; and therefore, to increase the force and precision with which the efforts of volition may be directed and insulated on the muscles which are thus supplied with nerves. And I think that any one who attends to the subject may observe that he is actually conscious of these differences, when he compares the effects of his voluntary exertions in his extremities with the motions of his head and trunk.

I think, therefore, that Sir Charles Bell was right in asserting that the plexus enables the acts of the will to form combinations of muscular motions for definite ends, in greater variety and with greater precision than they otherwise could; but I apprehend

the reason to be, not that each combination is effected by an impulse emanating from a single point, nor that the different combinations are *formed* in the plexus, but that the plexus, rendering the muscular sensations more distinct, and the acts of the will more energetic, enables the mind to act on all the muscles thus supplied with more power and precision, and to recollect and resume the action at any subsequent time with more certainty and uniformity, and thus facilitates combinations.

[To be continued.]

PLURALITY OF CHILDREN AT BIRTH.

To the Editor of the Medical Gazette.

SIR,

SEEING in your valuable journal of last week a case reported by Dr. Henry Davies (whose pupil I formerly had the honour of being) of a plurality of children at a birth, I am induced to forward you an account of two similar cases that occurred in the practice of a medical gentleman (Mr. Woods) of this town, who has kindly presented me with the placenta of one, and the particulars of both, should you think them worthy of an insertion.—I am, sir,

Your obedient servant,

CHARLES DODD, Surgeon.

Northampton, May 17, 1841.

Mrs. Manton, æt. 32, mother of seven children, with none of which did she go to the full period of utero-gestation, was taken in labour at 4 P.M., Nov. 20, 1840, having been pregnant between six and seven months. She did not send for the medical gentleman till 1 A.M., Nov. 21st; and on his entering the room, the nurse said, "Sir, the child was born with the pain you heard as you came up stairs." On examination, one child, a male, was found in the usual situation; another, a female, between the thighs; and the third, a female, against the abdomen: the woman described them as all being born with one pain: the placenta was expelled naturally, and the woman recovered without a bad symptom. The male child lived half an hour, one of the females 36 hours, and the other a month. The placenta, which I have now in my possession, is in one mass; between two portions there is no membranous septum, but there is a very distinct membrane between these and the

third portion. I injected the placental vessels of each child with different coloured wax, and there is evidently no communication between them.

In the other case, which occurred to the same gentleman about ten years since, the first child was expelled naturally, but the other two were footling presentations: the placenta came away in the usual time, and was also in one mass, but it was not injected. Here, however, there were three distinct membranes: the woman did well, but the children all died in a few days.

ON STAMMERING,

AND THE METHODS PROPOSED FOR ITS
REMOVAL.

By EDWIN LEE, Esq.

[Continued from p. 352.]

[*For the Medical Gazette.*]

I now proceed to the consideration of the operations which have been performed for the cure of stammering, and of their respective applicability to particular cases.

The first operation for stammering was performed by Professor Dieffenbach, of Berlin, in January last, who, in a letter addressed to the French Institute, thus states the manner in which he contemplated the removal of the complaint might be effected:—"As I conceived that the disorder in the mechanism of speech produced by stammering was referrible to a dynamic cause, which I considered to depend upon a spasmodic state of the air-passages, especially of the glottis, which was communicated to the tongue, muscles of the face, and even of the neck, I was led to think that by interrupting the innervation in the muscular organs which participate in this abnormal state, I should succeed in modifying or preventing it altogether." Accordingly he proposed the transverse section of the muscles of the tongue by three different methods; first, transverse horizontal section of the root of the tongue; second, transverse subcutaneous section of the root of the tongue, or section of the muscles without division of the mucous membrane; and, third, the horizontal section of the root of the tongue, with the excision of a triangular portion throughout its

whole width and thickness. In performing these operations, the tongue is seized with forceps terminating in double hooks to each blade (*pincers de Museaux*), and drawn out of the mouth. A bistoury is then passed through it at its thickest part from one side to the other, and its substance divided by cutting upwards. The posterior part is immediately seized with forceps, and held by an assistant, while the operator passes through it from behind forwards three short curved needles, armed with strong ligatures, which are carried to the bottom of the wound, and brought out on the upper surface of the anterior portion, when they are tied together and approximate the cut surfaces. The second operation is performed in a similar manner, with the exception that the mucous membrane of the dorsum of the tongue is not cut through. In the third, a piece of the tongue is cut through, somewhat resembling in shape a slice of melon, and the ligatures applied as in the first method. This last operation has entirely superseded the others, which were only performed at the outset.

The first operation was performed in four cases; in one, where there was a paralytic state of the tongue, it was unsuccessful; in the other three it was attended with success. The patient first operated upon was a boy whose stammer was very bad, and who had been considered to be incurable. He experienced more especially a difficulty upon the gutturals g, k, ch, z, and other consonants. At times he could not speak; and the presence of strangers always produced considerable agitation. The tongue was rigid to the feel; and the action of the muscles of the face and neck was considerable. On attempting to speak, the first word always caused the greatest difficulty: after this was overcome he could speak easier, and the words issued out in quick succession, till, on his becoming confused, the stammer returned. There was a good deal of hæmorrhage during the operation; but it ceased after the ligatures were tied. The stammering was removed, but the contractions of the face continued when he spoke. Considerable swelling of the tongue, and fever, supervened, which, however, subsided, and on the seventh day from the operation no remains of stammering were perceptible; the convulsive

movements of the face and lips had disappeared; his pronunciation was easy; the presence of strangers or mental emotion did not occasion any recurrence of the stammer. In the case where the second method of operating was adopted, on the introduction of the bistoury the blood gushed out with violence on both sides, and the tongue became very considerably swollen from the extravasation of blood. The third operation was performed on a boy, æt. 16, who had great difficulty in pronouncing words beginning with certain consonants, and also occasionally upon the vowels, attended with considerable spasmodic action of the muscles. When with his brother and sister he could frequently speak very well; but the least agitation, or the presence of strangers, caused a recurrence of the stammering. A slice of the tongue was cut out, and a good deal of bleeding ensued. The stammer was, however, removed, and, on the eighth day, the cure was complete.

The Paris papers having mentioned that a letter had been received, on the 9th of February, from Professor Dieffenbach, stating that he had operated successfully upon the muscles of the tongue for stammering, though the nature of the operation was not mentioned, several surgeons began to operate. M. Phillips first performed the operation of dividing the genio-glossi muscles below the tongue in private, and sent a sealed packet to the Académie des Sciences descriptive of the method he had adopted, and the result. I saw the patient at his house, a few days afterwards: he could speak and read with considerable facility. M. Velpeau, however, first operated in public, after describing the operation, on the 14th February. M. Amussat, who had directed his attention to the subject (before notice of Dieffenbach's operation had arrived) in consequence of a patient stammering who presented himself to have the operation for squinting performed (which was likewise the case with Professor Dieffenbach), after having examined the muscles beneath the tongue in this patient, stated to several persons present his opinion that their division would be likely to relieve the stammer. He subsequently performed the operation on several patients, some of whom were presented to the Académie de Médecine,

M. Baudens and others likewise operated with success in many cases.

M. Phillips' operation is as follows:—The patient seated on a chair, his head resting on the breast of an assistant, the operator seizes the frenum with a small hook near its angle of reflexion with the tongue. Another hook is inserted a little lower down, and an incision with scissors made between the two, and the membrane divided to some extent on either side; a sharp-pointed curved bistoury is then introduced, and, carried from one side of the symphysis of the jaw to the other, dividing the attachments of the muscles. The bleeding is sometimes abundant, but is salutary. A piece of sponge dipped in vinegar is placed in the wound to arrest the bleeding.

M. Baudens plunges a pair of finely-pointed scissors, curved almost to an acute angle, with their blades moderately separated, beneath the tongue, immediately behind the symphysis of the jaw, and with one cut divides the attachment of the genio-glossi muscles. There is generally little bleeding, as the incision is made close to the bone. A piece of sponge soaked in vinegar is put into the wound, and retained for two or three days, which practice has, in some instances, given rise to inflammation and suppuration. M. Baudens likewise divides the genio-hyoidei in those cases where the muscles of the neck and throat are implicated in the spasmodic action. One of the worst cases of M. Baudens was that of Vincent Zousset, a baker (*boulangier*) who was unable to articulate his name or profession. Immediately after the operation he could pronounce these words with great facility.

M. Amussat divides his operation into two periods or parts. The mouth being opened wide, and the tongue turned back, the mucous membrane of the lower part of the frenum between the Whartonian ducts is cut with small scissors, and the incision is extended on either side, the tongue being at the same time drawn outwards, so as to expose the genio-glossi muscles, which are then divided with large scissors curved on the flat. "At the point," says M. Amussat, "where I practise the section of the genio-glossi muscles, there is less difficulty and danger than at any other. You act on a double fibrous band (*faisceau*) or on

the summit of the triangle, whilst higher up, as is well known, the muscles spread out in a fan-like form, and are surrounded by vessels and nerves."

In some individuals it is found that the mere section of the frenum and sublingual membrane on either side of the roof of the mouth is sufficient to remedy the infirmity. Thus, out of fifty-five persons who were operated upon, eighteen were relieved in this manner. There is not in general much bleeding, and in none of the above cases has it occasioned uneasiness; when more than usually abundant, it is stopped by iced water and small lumps of ice beneath the tongue.

One of the first patients operated on was Alexis Beyeuval, a man, *æt.* 48, who had stammered from his childhood, and had had the frenum divided at two different periods without any advantage. The stammer was of the worst kind. The movements of the tongue were very limited. It was turned towards the right side, and the patient could not protrude it over the upper lip. After the division of the frenum and the sublingual membrane on either side, he could speak some words plainer than before, but experienced great difficulty upon others. The *genio-glossi* muscles were then divided, which was followed by considerable amelioration, though the stammer was not altogether removed. Some muscular fibres which were felt to be still attached to the bone were cut through, and pronunciation became free and easy, with scarcely any remains of the stammering. The deviation of the tongue was likewise rectified. Two months afterwards (18th April) there had been no relapse; the patient spoke as well as immediately after the operation.

Victoire Courgeot, *æt.* 16, has stammered since her childhood; her elder sister likewise stammered till she attained the age of fourteen, when it ceased. The father of these girls was a stammerer, which strongly corroborates the opinion I have already advanced; viz. that the majority of cases of stammering in females would be found to arise from imitation. We see, in this instance, that the sister, when approaching to puberty, being probably less under the control of her father, and associating less with him, lost her stammer; whereas, in the majority of

cases, the complaint increases at this period of life. In Victoire's case the division of the membrane was sufficient to cure her. Two months afterwards there had been no relapse.

An operation of a different kind was performed by M. Velpeau a few days before I left Paris. In this patient the tongue was larger than natural. He could touch the lower part of his nose and chin. A small triangular portion was cut out of the apex of the tongue, without dividing the mucous membrane beneath. The edges of the wound were brought together with points of suture. This was attended with some degree of amelioration at the time; but I hear that as the wound healed the stammering returned, though not so bad as before.

Another method which I saw described a few days ago in the *Gazette des Hôpitaux* has lately been tried, as it would appear with success, by the same eminent surgeon. The patient was a young man from whom scarcely a word could be obtained, insomuch that he would have been considered dumb, were it not that the movements of the lips, and the violent efforts which he made to articulate, indicated his actual condition. The tongue in this case was also very long, and he could touch with it the lower part of his nose. Previous to operating, M. Velpeau tried the experiment of raising the tongue with a pair of ordinary forceps, when the patient was able to pronounce some words; from which it was anticipated the operation would be attended with success. The operator then drew the tongue out of the mouth with his left hand, furnished with a piece of linen rag, and with the right passed transversely through its substance, at the junction of the posterior and middle thirds, a needle armed with four ligatures. Two of the ligatures were tied as far back as possible; the other two were tied anteriorly, thus circumscribing a portion of the tongue, which, after it had sloughed away, would entail a similar loss of substance as in Dieffenbach's operation. The patient did not appear to suffer much, and, immediately after the operation, was able to pronounce some words distinctly. He would not remain in the hospital, but promised to return every second day. During the first four days the size of the tongue was considerably

enlarged, and the neighbouring parts were swollen, but no serious accident ensued: a few leeches, with the use of gargles, sufficed to allay the above symptoms. A week after the operation there was only some slight inconvenience felt in the throat. The stammer was in great part removed, so that he could speak with tolerable facility.

About the same time M. Velpeau tied the temporal and facial arteries on a man, æt. 36, affected with epilepsy during the last seven years. The disease was first induced by a fright, and the attacks had continued eight or ten times a month; but within the last three months he had attacks almost daily. The temporal arteries were first tied: the patient had only a slight attack on the same day, and the following day passed without any. On the 5th of April the external maxillary arteries were tied at the point where they pass over the jaws, and the attacks did not recur.

The operation on the *genio-glossi*, as performed by Mr. Lucas, does not differ materially from the other methods which have been already mentioned, except that a portion of the muscle is removed; but a different mode of dividing these muscles, viz. the subcutaneous, has more recently been practised by M. Bonnet, of Lyons. A puncture is made in the skin, a few lines behind the symphysis of the jaw: a blunt-pointed tenotome is then introduced, with its sharp edge turned towards the bone, and passed upwards till its point is felt by the finger through the mucous membrane beneath the tongue. The attachments of the muscles are then divided right and left; the cutting edge of the tenotome being kept close to the bone, and acting only on the superior part of its concavity, the *genio-glossi* are alone cut, and the *genio-hyoidei* avoided. Scarcely any bleeding ensued. M. Bonnet had performed this operation in five cases: the two last patients, though labouring under a bad form of stammering, and who even stammered in reciting verses, were immediately relieved without preserving the least trace of their infirmity. On the other three patients the relief was not so decided, though a considerable degree of amelioration resulted; which circumstance M. Bonnet ascribed to the mode of operating not being so perfect in the first cases as in the last;

though I should think it depended on the former being more strictly cases of nervous stammer than the latter, and that the operation would not be so likely to be permanently successful.

The following cases will serve to illustrate the two varieties of stammering.

Purely nervous stammer.—A young man who has stammered since his childhood, states himself to be very nervous. When alone, or when with persons with whom he is very intimate, he can talk very well, but almost always stammers before strangers; sometimes more than at others; especially if he thinks the infirmity is remarked, or if he be questioned respecting it. The stammer is not, however, very bad. There is no particular tension of the muscles of the tongue, which can be turned upwards towards the nose, nor is there anything abnormal perceptible about the mouth or throat. He has two cousins similarly affected.

Physical stammer.—This man always experiences difficulty in pronouncing certain words, especially those beginning with a q or n, as *quatorze*, *navet*; considerable spasmodic action of the muscles of the face and lips being induced by the attempts to articulate these or other words upon which the difficulty exists. He has as much difficulty when alone as when in company or before strangers: cannot protrude the tongue far, or turn it upwards over the lip. After the division of the sublingual membrane, and cellular texture on the floor of the mouth, he was able to protrude the tongue, and could speak the above and other words with facility.

Mr. Yearsley's operation consists in the excision of the entire uvula, and also of the tonsils, when these bodies are enlarged or diseased. The part to be removed is seized with an Assalini's tenaculum, and cut off, scissors being employed for the uvula, and a knife for the tonsils, which, being frequently hardened, present considerable resistance, and could not, in all probability, be cut through by the new instrument, or guillotine as it has been termed, recently invented for this purpose, on the supposition of the likelihood of hæmorrhage from this operation; which occurrence, though it might happen when the tonsils are enlarged and soft, is not likely to take place, and I believe never has happened, when they are indurated. The extirpation of these parts

is attended with scarcely any pain in most instances, and the pain from the excision of the uvula is but slight. Nor has any particular inconvenience, as far as I know, attended the loss of this part; though it is said that singers and others, who have had this operation performed, were more liable to sore-throat and coughs. Mr. Yearsley, however, states that this is not the case when the entire uvula is removed; but that when only a portion of it is snipped off the pain is much greater, and subsequent irritation is not uncommon, which may be very easily conceived. In some cases, when the palatine arch descends lower than natural, it is snipped on either side previous to the removal of the uvula.

One of the most successful cases of this operation which has fallen under my observation is that of Crawley (the seventh case in Mr. Yearsley's pamphlet), who was affected with one of the worst kinds of stammer; in whom the tonsils and uvula were enlarged, and who derived immediate relief, which has continued to the present time (two months from the operation). He can now speak with facility, and without stammering.

In the boy Wright the stammer came on when he was between three and four years of age: there were occasionally remissions of a few weeks at a time; and as he had not stammered for a long time, his mother supposed that he had got rid of it altogether. Three months ago, however, his father died, which occasioned a recurrence of the stammer. Nothing anormal was perceptible about the mouth or throat. On the uvula being removed he immediately spoke freely, and without impediment. I am unable to say whether, in this case, the relief was permanent.

A man, æt. 40, sailmaker, when alone speaks "as well as any body;" and when with his comrades does not stammer so much as when with strangers, when the inconvenience is sometimes very great. Does not stammer when he speaks in a singing voice. The uvula was removed, but he still stammered, though not so much as before.

A man named Partridge has a difficulty, especially upon the p's, and can scarcely articulate his own name; but if he says it without forethought, or if

his attention be diverted, can speak it very well.

Snelling, a young man who was in the room when the boy Wright underwent the operation, on seeing its effect; and that the boy spoke without difficulty, was likewise able to speak without stammering. At my suggestion he was sent back to wait a little, and on returning into the room, at the expiration of about an hour, stammered a good deal while being questioned respecting it. There was a warty excrescence on the left tonsil: while he was asked about its origin he spoke fluently, and without the least stammer. This was also the case when questioned about other things which diverted his attention from the complaint. After the wart was removed he thought he spoke better, but still stammered. After removal of the uvula he spoke better, and on reading experienced every now and then some difficulty.

A boy, who had more especially a difficulty on letters beginning with p and l, experienced considerable hesitation when told to say *love-child* or *lollypop*; but after a full inspiration he could pronounce these words tolerably well during the act of expiration. After the removal of the uvula he could articulate them with facility, and without drawing in his breath.

A young woman, whose uvula was removed without any perceptible advantage.—The muscles beneath the tongue were a good deal developed, and this organ appeared to be limited in its movements. She is not free from stammer even when alone, but is always much worse when with strangers.

A man, in whom the palate had been extensively divided by another surgeon without any advantage resulting.—The uvula was excised by Mr. Yearsley, but did not produce any amelioration at the time.

Henry Gee, a bad stammerer.—The palate was divided on one side, after which he spoke better. Desired to return in a week. At the expiration of this period he still stammered, though not so badly as before. His mother says that he spoke better the two days succeeding the operation than the three subsequent days. After the uvula was removed, he was able to articulate much easier than before.

Wilkinson (mentioned in Mr. Yearsley's pamphlet), a man in whom the tongue wa

of large size.—Uvula removed without any advantage.

Henry Smith, a boy who stammered.—The tonsils were enlarged. After their excision was able to speak without much difficulty. The uvula was next removed, but it did not appear that he spoke any better than after the operation on the tonsils.

The above are a few of the cases of which I made a memorandum, and to which I shall have to allude. It now remains that I should endeavour to explain in what manner these various methods and operations were successful in removing stammering, and the causes of their failure in some instances; and to what cases the one or the other are more particularly applicable.

[To be continued.]

CONGENITAL APERTURE IN DIAPHRAGM.

To the Editor of the Medical Gazette.

SIR,

A FEW days ago a case of congenital diaphragmatic hernia occurred to me, a brief account of which may not be uninteresting to your readers.

A lady, thirty years of age, was confined with her second child at the full period of gestation, after a short and easy labour. The respiration of the infant was very imperfect, and it died an hour after its birth. As nothing existed in the circumstances of the labour to account for this result, I expected some malformation or disease in the chest, and requested permission to examine the body. There was found a large opening in the central tendon of the diaphragm (from arrest of development) through which the whole of the small intestines, and a considerable portion of the liver, had passed into the right side of the chest, occupying the greater part of that cavity, and unconfined by adhesions, so that the whole could be withdrawn through the same opening. The right lung was a mere vestige, and lay close against the spine, in texture resembling the carnified lung in empyema. The left lung was very small also, but permeated by air throughout, which rendered every portion of it buoyant, even after forcible compression. The heart and primitive vessels were of normal size and struc-

ture. The child was a male, of average size, and the presentation was natural.

I believe the cases on record of a similar nature are rare; perhaps they would be less so if medical men made a point of opening the bodies of all still-born children. The examination in the present instance was very satisfactory to the parents as well as myself.

I am, sir,

Your obedient servant,

W. F. MORGAN,

Surgeon to the Bristol Infirmary.

15, Park Street, Bristol,

May 18, 1841.

A CASE OF POISONING BY OPIUM,

IN WHICH ELECTRICITY WAS EMPLOYED
WITH SUCCESS.

By JOHN ERICHSEN, Esq.,

Formerly House-Surgeon to University College Hospital.

(For the Medical Gazette.)

CASE.—Sarah Mitchell, ætatis 22, was brought to University College Hospital about 2 P.M. on the 6th of August, 1840, labouring under the ordinary symptoms of poisoning by opium. The friends accompanying her said that they knew but little concerning the particulars of the transaction; that she was a married woman, and appeared to live on good terms with her husband; that on visiting her rooms about two hours ago, they found her in a very "stupid" state, and scarcely able to answer any question put to her: she however informed them, that having procured laudanum at different shops to the amount of an ounce, on the plea of its being required as an application to an aching tooth, she had swallowed it with a view of putting an end to her existence, and that she was then labouring under the effects of the poison. A medical man who was called in gave her an emetic, which induced vomiting, and advised her being sent to a hospital, which was accordingly done.

On admission, four hours after taking the poison, she was perfectly insensible; her countenance was flushed, or rather had a blotched appearance; the surface of the body and the hands were cold and clammy; her eyes were closed, and the pupils were firmly contracted; her limbs were flaccid; she was not able to

walk, and answered no questions, even when put in the loudest voice; her pulse was slow and small, and her breathing less frequent than natural, but without any stertor.

I immediately introduced the stomach-pump, and drew off a small quantity of fluid impregnated with the odour of opium, and then washed out her stomach with warm water, until it came away quite clear. In order to rouse her, hot strong coffee, with brandy and ammonia, was given; and large quantities of cold water were repeatedly and forcibly dashed in her face. She was also, without intermission, dragged about a cool room between two men; and was occasionally rather roughly shaken and slapped on the back: *dolichos pruriens* was applied to her hands and feet.

The cold affusion roused her for a time, to a certain degree, but she almost immediately relapsed into her drowsy condition: none of the other means that were employed had any effect upon her.

About three hours were spent in this way, when, instead of getting better, she appeared to become more comatose, and it was only with very great difficulty indeed that she could be roused at all.

Being fearful of losing her, and thinking that a stronger stimulus than any that had as yet been employed was required to overcome the state of torpor in which she was, I determined upon trying the effect of electricity. For this purpose a rather powerful electro-magnetic apparatus that was in the hospital was set in action; one of the cylinders, which was attached to the conducting wires, being applied to the forehead, and the other to the upper part of the spine. After a few shocks had in this way been passed through the head, she began to recover from the state of coma in which she had previously been, and attempted to remove the wires with her hands, begging that the shocks might be discontinued, as they gave her pain. They were, however, persevered in for about twenty minutes, or half an hour, when she was sufficiently recovered to be able to answer questions with comparative facility, and to walk a little. She was then removed to the ward, and allowed to lie down, with strict injunctions to the nurse, however, to prevent her sleeping, by continuing the admi-

nistration of the coffee, and by shaking her occasionally. Towards the evening she became quite sensible, cried frequently, and deplored her present condition.

7th.—Has passed a good night; complains of some pain at the pit of the stomach, but has not vomited. Has a nervous tremor about her. Pupils not so much contracted, but as yet not fully dilated. She feels cold and weak; pulse sharp, but not quick. Her bowels have been opened by a dose of castor oil. Dr. Thomson saw her in the course of the afternoon, and ordered a saline draught every four hours.

8th.—Much the same as yesterday; bowels confined.

Infusion of Senna with Salines ordered.

10th.—Complains of pain in the lumbar region; the nervous tremor exists to a great degree; pulse is quick and irritable; bowels confined; she has not passed any water since yesterday afternoon; there is great tenderness on pressure at the epigastrium. Dr. Thomson ordered

C. C. Lumbis ad ℥xij .

℞ Pil. Hydrarg.; Ext. Aconiti aa. gr. j.; Ext. Gentian, gr. iij.; Ft. Pilula 6ta qq. horâ Sumenda.

℞ Decocti Aloes, ℥iv .; Tr. Valerianæ Vol. ℥ij .; Misturæ Camphoræ, ℥ij .; Sumat. 4tam partem inter singulas doses pilularum.

12th.—Has much pain in making water, which is scanty; she still complains of some pain in the loins, which has, however, been relieved by the cupping; pulse very tremulous, and quick; general tremor over the body: all the symptoms of poisoning have disappeared, and she is now labouring under hysteria produced by great agitation of mind. Surface clammy and perspiring; does not sleep well; bowels somewhat confined.

C. C. Lumbis ad ℥xij .

℞ Pil. Hydr. gr. v.; Ext. Conii, gr. iij.; Ft. Pilula; quamprimum sumenda; Haustus Niger horâ unâ post pilulam.

14th.—Patient is very much better. She has entirely recovered from the effects of the poison, and has nearly lost all the hysterical symptoms.

16th.—Discharged cured.

In another case of poisoning by opium which was brought to University College Hospital about six weeks ago, my friends, Dr. Lankester, the resident

medical officer, and Mr. Henry Taylor, one of the house-surgeons, employed electricity with success after all other means had failed. I have not the particulars of this case; but I believe they will shortly be published in one of the journals.

REMARKS.—My object in publishing these cases has been to direct the attention of the profession to the employment of electricity as a powerful means of rousing the energies of the nervous system in cases of poisoning by opium; more especially as it is a mode of treatment that, I believe, has not been hitherto recommended by any writer on toxicology; at least I have not found any mention made of it in any work on that subject which I have had an opportunity of consulting.

The chief indication to be fulfilled in the treatment of poisoning by opium is, after the stomach has been emptied by means of the stomach-pump, to keep the nervous system roused until the narcotic effects of the drug wear off, which happens after a longer or shorter time, according to the size of the dose taken and other modifying circumstances, just as the excitant effects of a stimulant, such as alcohol, gradually pass off. Now we are certainly in possession of no stimulus which is so immediate in its action, so powerful, and so easily applied, as electricity. In all these respects it is far superior to dashing cold water on the body and face. Moreover, the cold affusion, if continued too long, may be productive of some injurious effects; for, if the patient be allowed to remain any length of time with his clothes saturated with water, the cold, by depressing still more the energies of the system, will tend to aid rather than to counteract the sedative influence of the opium. It should therefore be employed with great caution in those cases in which the surface is cold, the pulse slow and weak, and the respiration feeble. It is quite possible for pneumonia to follow the use of the cold affusion: this actually happened in the second case which I have briefly mentioned above; the disease, no doubt, being predisposed to by that congested condition of the lungs which is so common a consequence of narcotism. Notwithstanding these objections to the employment of the cold affusion, it is, if properly used, a most valuable means in the treatment of

poisoning by opium, and one that, in the great majority of cases, is fully sufficient to overcome the narcotic effects of the poison, after the exciting cause has been removed from the stomach. In some instances, however, of which the case detailed above is a good example, the shock produced by the sudden and forcible dashing of cold water is not sufficiently powerful; and it is in this case that I would strongly recommend the employment of electricity after the cold affusion had been persevered in for a moderate length of time, from half to three quarters of an hour, and had failed in rousing the powers of the patient.

The electricity should not supersede the use of the cold affusion, or of those other means that are ordinarily resorted to in cases of poisoning by opium; but that it may be employed with the greatest advantage in addition to these, I think we are fully warranted in concluding from the case that I have detailed, and which, there can be little doubt, would have had a fatal termination had it not been for the employment of this very powerful stimulus, which produced a most marked and almost immediately beneficial effect; the patient, who was quite speechless, insensible and motionless, with flaccid, cold, clammy extremities, and a small weak pulse, attempted, after a few shocks had been passed through her head, to remove the wires, complaining of the pain the application of the electricity gave her; and was in half an hour sufficiently recovered to answer questions that were put to her, to walk, and to be allowed to lie down on her bed.

LIGATURE OF COMMON CAROTID.

To the Editor of the Medical Gazette.

SIR,

I BEG to inclose you the notes of a case of aneurism on the side of the face, cured by tying the common carotid artery. The case was extremely interesting to me at the time of its occurrence, and I think may probably be so to the profession at large.—I am, sir,

Your obedient servant,

JAMES SURRAGE,
M.D. M.R.C.S.

Wincanton, Somersetshire,
April 20th, 1841.

Monday, October 26th, 1840.—John Churchouse, a tailor, aged nineteen,

called on me this day to consult me respecting a large tumor on the left side of the face. About a year since, when cutting one of the dentes sapientiæ, he perceived a hard swelling behind the jaw, but, as he associated its existence with the state of the tooth, he did not at the time attach much importance to it. From the above period, however, it gradually enlarged, and, during the last four months, has increased very rapidly, but without much pain. Since the case has been in progress the patient has consulted several surgeons, who have given him different opinions as to its nature; some considering it a chronic abscess, whilst others thought it a disease of the bone, and recommended him to place himself in some hospital. The tumor, when I first saw it, was of an immense size, extending from below the angle of the jaw upwards, until it was lost behind the malar bone, and from the ear forwards to within an inch of the mouth; it was œdematous over the whole surface, and firm beneath, but neither pulsation nor fluctuation could be detected. The patient could not breathe through the left nostril, and the right felt greatly obstructed. On examining the mouth, which it was difficult to do, in consequence of the confined state of the parts, I found the cheek pressing inwardly against, and much indented by, the teeth; the molar and first bicuspid of the upper jaw were, indeed, nearly hidden by the gums, and the mucous membrane on the left side of the palate projected so much as to resemble very strongly the pointing of an abscess. After carefully reflecting on the history of the case, and under the hope that by doing it I should gain an insight into the nature of the swelling, I determined on extracting the tooth supposed by the patient to be the original cause of the mischief. It was not decayed, or unnatural in appearance; nor was its extraction followed by a discharge of matter, or more bleeding than usual. About twenty-four hours after the tooth was removed, I was summoned to the patient in consequence of considerable hæmorrhage from the socket: this was easily stopped by a plug of lint, but recurred in the course of eight hours to a very considerable extent, and produced alarming exhaustion. The tension of the tumor being now much lessened, an indistinct pulsation

could be felt, synchronous with the pulse at the wrist, and removeable by pressure on the carotid, so that the nature of the case was no longer doubtful. The patient now, for the first time, told me he had been subject lately to bleeding from the left side of the nose, and that twice within the fortnight epistaxis had occurred to an alarming extent; so much so, indeed, that, on one of the occasions, severe syncope came on, and continued for a considerable time. There could now be no doubt the case was one of aneurism, either of the external carotid, or one of its terminal branches; most probably, I think, of the internal maxillary. I therefore determined on tying the common carotid. My friends, Mr. Eastment and Mr. Bruorton, whom I now consulted, coinciding with me in opinion as to the nature of the case, I availed myself of their ready assistance, and immediately performed the operation, on the morning of the 28th, two days after I first saw the patient. The vessel was secured in the usual manner, and without much difficulty, about a quarter of an inch below the tendon of the omohyoid muscle.

Vespere.—The tumor considerably diminished in size, no pulsation, and the patient comfortable; there was some oozing of bloody saliva from the mouth, which continued, at intervals, until the evening of the 30th, when several coagula came away, and there was a discharge of arterial blood to the amount of four ounces. The swelling was now materially lessened, being not more than half of its original size; but, as I was fearful of hæmorrhage, I plugged the opening in the mouth with lint, kept the head supported, and the cheek constantly moist with evaporating lotion.

Nov. 3d.—The tumor has been gradually increasing since my last report; there is now considerable pain and throbbing; the cheek is hot, and wears a hectic appearance. As it was evident the sac was suppurating, I removed my plugs, and gave exit to about two ounces of grumous blood, mixed with pus; the swelling now subsided, the pain ceased, and the patient fell asleep.

5th.—As the swelling and pain had again increased, I removed the plugs, and discharged a small quantity of pus, which was but slightly tinged with

blood. From this period the discharge of matter gradually ceased; the gum healed, and gave me no more trouble. At the end of three weeks the incision in the neck was nearly healed; but the ligature was undetached. My patient was now allowed to leave his bed, and was put on full diet. The case proceeded so satisfactorily from this time, that I lost sight of it until December 25th, when my patient reported himself as proceeding fast to recovery. The ligature had come away spontaneously, the neck was quite healed, and the swelling fast subsiding.

April 20th. — The patient is now healthy and well, the tumor entirely gone, the passage of the left nostril perfect, and he can open his mouth as widely as usual, but some deformity remains in consequence of the projection outwards of the malar bone. On examining the mouth, a distinct fissure can be felt on the palatine process of the palate and maxillary bones.

That this was a case of aneurism I have not the slightest doubt; but I am not quite certain as to the individual artery involved. From the history the patient gave of the tumor, I am inclined to think it was an aneurism of the internal maxillary, which commenced deeply, and posteriorly to the ramus of the jaw, extended downwards to the neck, and upwards behind the malar bone: causing a passage by absorption into the antrum, finding its way thence into the nose, and finally producing absorption of the floor of the nostril, it made its appearance under the mucous membrane of the palate.

IODINE IN PHTHISIS PULMONALIS.

To the Editor of the Medical Gazette.

SIR,

THE local application of iodine is of acknowledged use in healing external ulcers; therefore, by analogy, it is concluded to be of similar use in pulmonary abscesses communicating with the atmosphere: experience also corroborates the above conclusion.

Though physicians agree in the principle of action, yet they differ in the mode of application. Sir C. Scudamore recommends the inhaler; Dr. Corrigan impregnates the atmosphere of the patient's room with iodine.

Both methods being inconvenient in practice, I direct the patient to apply a sufficient quantity of iodine ointment on the ribs, under both axillæ, and to cover the head with the bed-clothes, to breathe the iodine volatilized by the heat of the axillæ; the ointment produces counter-irritation on the skin where it is applied, and is to be repeated according to circumstances.. This method has appeared to me to arrest the progress of the disease; and as I should wish to hear the result of more extensive trial, I beg you will place the above remarks (if you think them worth inserting) in the MEDICAL GAZETTE.—I remain, sir,

Your obedient servant,

ALEXANDER LEIGH,
A.B. M.B.

3, Olympic Place, Jersey,
May 14th, 1841.

ANALYSES AND NOTICES OF BOOKS.

“L'Auteur se tue à alonger ce que le lecteur se tue à abréger.”—D'ALEMBERT.

Handbuch der Geschichte der Europäischen Medicin, Chirurgie, Pharmacie und Naturkunde in philosophisch practischer entwicklung. Von HOFRATH Dr. ISENSEE, Universitätslehrer, practischem Arzte und Operateur in Berlin, &c. Berlin, 1839-41.

Hand-book of the History of European Medicine, Surgery, Pharmacy, and Natural History, in their philosophico-practical development. By Dr. ISENSEE, Court-councillor, Teacher at the University, Practising Physician and Operating Surgeon at Berlin, &c. &c.

THIS is one of the most comprehensive, complete, learned, and at the same time succinct histories of the medical sciences with which we are acquainted. It satisfactorily fulfils the promise held out by its title, and considering the wide scope of the latter, this is no little praise. From the number of authors whom it notices, and from the care with which the titles of their works are quoted, it is invaluable as a book of reference. It gives a complete list of the writings of Aristotle, and also of those of Hippocrates, with brief critical remarks on each of the latter. We cannot better convey to our readers an idea of its merits, and of the system on which it

is arranged, than by following the author through a stage of one of the periods of the history of medicine. We will choose, for the sake of example, the "Roman epoch" (p. 103). This period is divided into three principal sections; the first treating of the progress of medicine in Rome before Galen; the second, of the same by means of Galen; and the third, of that which was effected after Galen. It is to the commencement of the first section that we must confine ourselves. The author first gives us a page of general reflections on the peculiar character of Roman intellect and mental cultivation. The next two pages are entitled, "A retrospective glance on medicine in Rome in its earliest ages" (with references to the ancient historians, and to Gans and Niebuhr amongst the moderns). Then follows a sketch of Asclepiades: 1. of the circumstances of his life; 2. of his writings; 3. his manners and style as a physician; 4. his medical system, viz. *a*, his therapeutics; *b*, materia medica; *c*, pathology; *d*, anatomy and physiology; *e*, general views. Finally, half a page is devoted to the enumeration of his scholars. The whole sketch of Asclepiades and his school occupies seven pages, and displays the most careful research, and also mature judgment.

Two parts of this work have already been published: the second and larger one brings down the history of medicine to the seventeenth century.

The Surgical Anatomy of Inguinal Hernia, the Testis, and its Coverings. By THOMAS MORTON, one of the Demonstrators in University College, London; and formerly House Surgeon to the Hospital of the same College. Illustrated with lithographic plates and wood engravings. 8vo. Taylor and Walton.

WE had much pleasure, some time since, in expressing a very favourable opinion of Mr. Morton's work on the Perinæum, the first of a series which will embrace all the most important regions of the body. The subjects discussed in the present volume are treated of in the same clear and useful style which was the great recommendation of the former. The engravings are well executed, from admirable drawings,

and the work altogether is one of those which, without the pretence of originality, where novelty is nothing, has the great merit of being eminently practical where practice is every thing.

MEDICAL GAZETTE.

Friday, May 28, 1841.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

THE ANATOMY BILL.

A CONVERSATION took place in the House of Commons last week respecting the working of this measure. Mr. Maclean and Mr. Duncombe gave an account of the "great dissatisfaction" which prevailed as to the manner in which the Bill has been carried out. Now had they stated the grievances at this time last year, the necessity of some change or of some energetic inquiry, would, we believe, have been generally admitted; but it must be confessed that during the season just past the Anatomy Bill has been acted upon in such a manner as to have produced a very satisfactory result. This at least has been the case in London, and, as far as we have been able to learn, the management for the provincial schools has been equally good; and we state this with the more pleasure, from the belief that the regularity with which, whenever they were growing lax, we stimulated the energies of those on whom the working of the Bill mainly depended, has contributed in no small degree to bring about this full accomplishment of the purposes of the Act.

The exertions made by the London Inspector during the past season have been, we are bound to say, very creditable; and the only reason why we, who have always so readily told him

his faults, have not sooner accorded to him this praise, has been that we feared he might think energy enough had been employed, and might too soon again retire to that life of ease and calm repose, which he seemed at first to regard as quite compatible with the duties of a somewhat laborious office. But we do hope that now, when he has seen how much may be effected by exertion, and how amply the powers under his control are sufficient to accomplish all that the anatomical schools can reasonably expect, he will not henceforth relax in his endeavours, but will obtain each year, as he has during the past, a supply that shall be even something greater than the demand.

The abundance of subjects during the past year has been chiefly drawn, we believe, from the metropolitan workhouses, from several of which the body that would formerly have been buried at the parish expense have been sent to the anatomical schools. One reason of this has undoubtedly been the withdrawal of Mr. Roberts, the gentleman who, being the inventor of a system for preserving bodies from decomposition, was naturally anxious to embarrass, as far as possible, the scheme by which a sufficient supply of fresh subjects could be obtained, and who therefore annually got up an excitement by hand-bills and placards, in the neighbourhood of the chief workhouses, to lead the poor to feel horror at the very name of an anatomical school. We long ago exposed the absurdity of attempting to apply any of the modes of preservation yet discovered to the subjects for dissection. We feel assured that neither Mr. Roberts's, nor M. Gannal's, nor any other yet invented, can be advantageously adopted for this purpose; and we now are happy to believe that the only circumstance that could render them worthy of consideration, namely, a de-

ficient supply of subjects in the recent state, is removed.

But we believe that a cause yet more efficient than this, in securing the full supply, has been the rational indifference with which the dissection of bodies, when decently performed under authorized superintendence, is generally regarded. There is really no public feeling whatever upon the subject now; and men of sense being left to the right exercise of their judgment upon it, unmoved by prejudice or the clamour of ignorance, of course decide in the manner most favourable to the anatomical schools. Much of this has depended on the repeal of that enactment which consigned murderers' bodies to dissection, and so conferred on all who shared the same fate a portion of their dishonour. But much more has resulted from the quietude in which the bill has been allowed to work. We fully accord with those members of the House who deprecated the public introduction and discussion of the subject, for the purpose of any slight alterations that might be made; it is peculiarly one of those measures in which a full discretionary power should be allowed to authorities, for the purpose of introducing, from time to time, such modifications as, without affecting its general principle, may adapt it to changing circumstances, and to cases the occurrence of which was not, and perhaps could not be, calculated on. Perhaps there is no measure of recent years in which, at its first enactment, so much of the detail was undetermined, and left to be filled up at discretion by the Home Secretary of State; in fact, the whole working of the bill has been the subject of arrangements made subsequently to its being passed; and the result of thus keeping the subject from being perpetually brought under public notice has been most beneficial; for

the evils of occasional delay and frequent inactivity have been more than counterbalanced by the absence of excitement, and its necessary consequent, public prejudice.

As an instance in proof of this, we will mention but one important addition made to the bill during the last season. Under the original enactment there was nothing to provide for or against the dissection of still-born children; and these were therefore commonly brought to anatomical schools by nurses, or at last even by men who made a sort of trade of them, and there left without further inquiry. Such a plan was probably harmless, but it admitted of abuse; and last year a case happened in which there was some suspicion (which, however, we believe was subsequently proved to have been unfounded) that a child murdered by its mother had been disposed of at an anatomical school. The immediate consequence was an order from the Secretary of State, that in future no foetus should be received at a school without sending to the inspector a certificate of its birth, and its death before or directly after birth, signed by the medical attendant, or some other person present at the time, and by the teacher receiving the body. The result has been, indeed, that the number of foetus brought to the schools has been greatly diminished, the parents not liking such facts to be generally known; but this will probably be but a temporary evil, and even if it continue it will yet be far better than the great amount of injury that would have been done had the public been excited by the same arrangement being made the subject of parliamentary legislation.

On such a subject, and on all of the like kind, when they are prominently brought before them, even the higher classes of the laity are scarcely capable of forming a calm and correct judg-

ment; and there is little doubt that the mode in which dissections are carried on, even with the most perfect observance that is possible of all decency, would appear to them very disgusting, and perhaps deserving of legislative interference for its improvement. The wisest course that they can pursue in such matters is to turn their thoughts away from them; just as, in their ordinary walk through the world, they turn away their eyes from any object that is offensive, but if they believe that it performs some useful office, or is essential to their own or others' comfort, leave it unmolested in its offensiveness. Dissecting and dissecting-rooms are signally useful objects of disgust; they are sometimes loathsome even to those who are most familiar with them, and most obliged to keep down their aversion; but in this respect they are not capable of much improvement, and the public, who at last reap the greatest benefits from them, must be content to leave their management in the hands of those who can alone judge dispassionately where, in such cases, necessary indifference ends, and where indecency begins, and on whom they can, in some measure, rely that the limit will not be overpassed.

We sincerely hope, therefore, that Mr. Maclean will, when he has read the report, be induced to think it satisfactory, and bring forward no measure founded upon it. The only subjects of any general importance to which it will relate, are the contraventions of the Act by teachers or students since its enactment, and the proceedings, if any, which have been taken thereon. We can ourselves, we believe, give the answer; which is, that the contraventions (at least such as on a liberal interpretation of the bill can be so called) have been very few and unimportant; and that the proceedings upon them have not amounted to more than re-

proofs from the inspector, and threats of punishment more heavy if the offence were repeated. But, really, neither this nor any of the questions that will be answered in the returns, would at all merit the trouble of legislation, even if it could be carried on without evil. The errors in the working of the Bill are not, or rather were not, such as Acts of Parliament can remedy; the want of energy in its management has been found curable by an occasional sharp stimulus; and it is to be hoped that, this being effected, there will henceforth be no great cause for complaint.

With respect to the distribution of the subjects, which was introduced into the conversation, and which is another arrangement added to the Bill by the consent of the Home-office, to whom it was recommended by the inspector (we believe, at the request of the majority of the anatomical teachers); this, if it has been carried out fairly, (and we know of no facts to prove the contrary) is in nowise objectionable, so long as the supply of subjects is not suffered to fall below the demand for them. When the number obtained by the inspector is deficient, through his lack of zeal or of influence, an equal distribution is mischievous, because, as we have very often said when the question has been under discussion, there is no inducement for the more influential among the anatomical teachers to use any exertion to obtain subjects, which will probably be taken, not to their own schools, but to those of their opponents. And in past years this has commonly happened; the inspector, taking the office of distributor, has of necessity become purveyor also, and, unless he discharged thoroughly well the duties of this last office, the power which he exerted in the second was very mischievous, and the first would soon have become a sinecure.

But the case is different when there are plenty of subjects in the common stock to distribute; then an equal partition of them is as much the most convenient as it is obviously the fairest method. Indeed, in such a case the chief purpose which the system accomplishes is not so much the preventing one school from obtaining an excess while another has a deficiency (for none would necessarily incur the annoyance and expense of an excess), as the distributing the subjects to all the schools equally, so that none of them may be at one time burdened, and at another empty. During nearly the whole season just past this purpose has been well and steadily attained; at the very beginning there was some deficiency, and at Christmas (when the number of pupils was diminished by the fortnight's vacation) there was some, and even a considerable excess; but, on the whole, there was such a supply that most of the teachers may congratulate themselves if the Bill be as well worked in all future years. And all this we may add was effected in a season during which the amount of illness among that class of the poor from whom the dissecting rooms are chiefly supplied was less than the average; for we had neither severe influenza nor fever, nor any other destructive disease in an epidemic form. We repeat, therefore, our sincere hope that the Bill will be left undisturbed: so long as energy is employed in working, it is amply sufficient for its purpose.

DEATH OF DR. HOPE*.

WE regret to announce the decease of Dr. Hope, which took place at Hampstead, on the 13th instant.

Dr. Hope was in the 41st year of his age, and the complaint which proved fatal was pulmonary consumption. He was the author

* This notice was accidentally omitted last week.

of a work on the heart, which has attracted a large share of attention; he also published a set of plates illustrative of morbid changes of structure in the human body. Some years ago he held the office of Physician to the St. Marylebone Infirmary, and afterwards became one of the Physicians to St. George's Hospital.

DEATH OF THOMAS HODSON, ESQ.,
SURGEON AT LEWES, IN
SUSSEX.

(From a Correspondent.)

THE subject of this memoir was the son of the Rev. Henry Hodson, Rector of Sandhurst, Kent. He prosecuted his medical studies at the hospitals of Guy's and St. Thomas's, London, where he had for one of his teachers Mr. Cline, and as a contemporary the late Sir Astley Cooper, with both of whom he maintained a friendly intercourse till they died.

Mr. H. received an honorary certificate of proficiency in his studies from his medical school, and commenced his practice, as a general practitioner, at Lewes, in Sussex, in 1786. Soon after he became a member of the College of Surgeons of London.

He early distinguished himself by the operation of lithotomy, which during the course of his practice he performed upwards of forty times with the loss only of three patients—a degree of success scarcely surpassed by that of the most eminent surgeons of the metropolis. The zeal which he shewed, and the knowledge which he possessed of his profession, and the liberal and kind manner in which he practised it, are well known to the physicians and surgeons of Lewes and its neighbourhood, as well as to many of the most eminent surgeons of London, with whom he had frequent intercourse and consultations.

About twenty years before his death he formed the plan of transferring his residence to Brighton, where the frequent resort of the Court, and the prospect of a county hospital (soon after established there) seemed to offer a more extensive field of practice; but this project he never carried into effect, though he often regretted he had not. He closed an active and useful life, April 13th, 1841, in the seventy-ninth year of his age*.

Of three sons and a daughter, the last only survived him, now married to the Rev.

Edward Bull, Rector of Pentlow, Essex. Mr. Hodson was a man of great integrity and simplicity of character, retaining a taste even to his later years for the manly exercise of his youth, hunting, shooting, and cricket—amusements in which professional business rarely permitted him to indulge.

For several years of his life he expressed to some friends doubts on religious subjects; but there is reason to believe that in his later years these doubts were removed, and that he died in the faith and hope of a Christian.

CLIMATE OF TEXAS.

To the swarms of medical practitioners that yearly issue from the universities and colleges of Europe, Texas offers little encouragement as a field of professional speculation. There is no malady that can be properly called endemic; and the febrile diseases which usually afflict early settlers, especially in southern latitudes, are of a mild type, completely within the control of medicine, and generally to be avoided by the observance of a few simple rules of living. Emigrants accustomed to northern habits, should, at least until they are thoroughly "acclimated," shun undue exposure to the noonday sun; exercise caution in the use of fruit and salted food, abstain from ardent spirits, and refrain as much as possible from drinking, save at meals. To persons new to the climate, occasional cathartics, followed by the use of bark, are beneficial. Very frequently, the injurious effects attributed to climate are caused by intemperance, or the neglect of some of those precautions of which the climate of every latitude demands its peculiar share. If any part of Texas can be termed sickly, it is the narrow strip of country running parallel to the gulf, where in the low timbered bottoms the rivers deposit the accumulations of their annual overflows. In this section, to which Providence has granted exuberant fertility, in compensation of its comparative insalubrity, settlers are liable to be attacked by bilious and intermittent fevers; but after receding some distance from the coast, no part of the globe is more friendly to the regular action of the human frame. The towns immediately on the coast, within the direct range of the trade winds, are healthy, although rather trying to temporary sojourners from cold climes, during the months of June, July, and August. Pulmonary consumption, so destructive in England and the Northern States of the American Union, is almost unknown in Texas. Rheumatisms and chronic diseases are not prevalent; and nine-tenths of the Republic are considered healthier than the most healthy parts of the United States. In the opinion of respecta-

* The disorder of which Mr. Hodson died was apoplexy. The first threatening of it took place about three weeks before his death, by a sudden involuntary dropping of his left arm. This was very transient, the usual power and feeling returning immediately. It occurred on six or eight different days. On the 10th of April there was slight distortion of the mouth; on the 12th hemiplegia of the left side; and on the 13th he died.

ble medical men, a residence in this country would be as favourable to persons of a consumptive tendency as the south of Europe, or Madeira. As a general fact, it may be stated that the farther from the lands bordering on the coast, the more salubrious is the locality; and persons who arrive in summer will be quite safe by retiring fifty or sixty miles inland. Western Texas is best adapted to a nothern constitution; and above the falls of the Brazos, or in the region lying about seventy miles above the mouths of the rivers westward of the Brazos, natives of Great Britain may settle with at least as fair a prospect of longevity as they had at home." — *Kennedy's Rise, Progress, and Prospects of Texas.*

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Friday, May 21.

David Mahony. — Daniel Wilson. — Robert Horlock. — Samuel Newham. — William Stoker. — Thomas Hodson. — Samuel Phillips. — Henry Brooking Square. — Edward Heath. — Robert Boyle Travers. — Henry Hodgson Ogle Hay.

Monday, May 24.

James Cooper Pigg. — George Rodwell. — Thomas Wetherall Sproule. — John Fewster Dawson. — Henry Manning — George Butler. — John Jobson. — John Scott. — Charles Meeres.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 15th May, 1841.

Small Pox	20
Measles	13
Scarlatina	6
Hooping Cough	45
Croup	6
Thrush	1
Diarrhoea	4
Dysentery	1
Cholera	0
Influenza	4
Typhus	19
Erysipelas	8
Syphilis	0
Hydrophobia	1
Diseases of the Brain, Nerves, and Senses ..	141
Diseases of the Lungs, and other Organs of Respiration	236
Diseases of the Heart and Blood-vessels	20
Diseases of the Stomach, Liver, and other Organs of Digestion	57
Diseases of the Kidneys, &c.....	2
Childbed	3
Ovarian Dropsy	0
Diseases of Uterus, &c.....	3
Rheumatism	0
Diseases of Joints, &c.....	4
Ulcer	1
Fistula	0
Diseases of Skin, &c.....	0
Diseases of Uncertain Seat	87
Old Age or Natural Decay.....	64
Deaths by Violence, Privation, or Intemperance	15
Causes not specified	0
Deaths from all Causes	761

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N. Longitude 0° 3' 51" W. of Greenwich.

May.	THERMOMETER.		BAROMETER.	
Wednesday 19	from	46 to 57	29·39 to	29·21
Thursday . 20		48 56	29·21	29·47
Friday . . . 21		38 62	29·61	29·60
Saturday . 22		54 67	29·59	29·98
Sunday . . 23		44 70	30·00	30·11
Monday . . 24		44 70	30·11	Stat.
Tuesday . 25		51 73	30·12	30·09

Winds variable, S.W. prevailing.
On the 19th, overcast, raining frequently during the day; wind boisterous. The 20th, generally clear, rain at times. The 21st, evening overcast, with rain; otherwise clear. The 22d, cloudy, rain fell in the afternoon. The 23d, and following day, clear; vivid lightning in N.E. from about 10 p.m. till after midnight of the 24th. The 25th, evening overcast, otherwise clear; lightning during the evening.
Rain fallen, .365 of an inch.
CHARLES HENRY ADAMS.

UNIVERSITY OF LONDON.

ON Wednesday last the Senate of the University of London passed a resolution to the effect that all students who commenced their medical studies, either as apprentices, or in any other way, in or before January 1839, will be exempted from the Matriculation Examination, whenever they may present themselves for the examination for bachelor of medicine.

LECTURES AT THE COLLEGE OF SURGEONS.

THE Surgical Lectures for the present season are to be delivered by Mr. Guthrie; and the subjects he has chosen are Injuries of the Head, Chest, and Abdomen. They commence Tuesday June 1st, at 4 o'clock P.M.

DR. CARPENTER'S AND DR. CORRIGAN'S LECTURES.

Dr. Carpenter's Lectures will be resumed in the next number but one, and afterwards continued without farther interruption.
Dr. Corrigan's Clinical Lectures will also be resumed in an early number.

NOTICES.

We should not feel justified in giving up so much of our space as Mr. M.'s (of Leeds) would occupy; but if he can state the circumstances within a page, we shall publish them for him.
The discussions contained in the letters of Mr. F., of Liverpool, and Mr. C., of Glasgow, can only be continued in the *extru limites* department.
M.R.C.S. — The party is entitled to charge for his visits; but we cannot speak as to the amount without knowing more of the circumstances.
WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

FRIDAY, JUNE 4, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXXVII.

Chorea. Symptoms; pathology; complications; causes; treatment. Chronic Chorea. Other Nervous Disorders to which the same name has been applied.

Chorea.—Another disease of a spasmodic kind, and essentially belonging to the nervous system, is *Chorea—St. Vitus' dance*. This is far less serious than the complaints which we have recently been considering; but it is a very unpleasant disorder to suffer, and it has several points of analogy with the other nervous and spasmodic ailments. Its prominent symptom is an irregular and involuntary clonic contraction of some of the voluntary muscles, which, however, are not wholly or constantly withdrawn from the government of the will. In tetanus we had rigid spasm, while the mind was clear and free; volition was unaffected, but the muscles which should have obeyed the effort of the will were seized upon by some stronger overruling power. In epilepsy, with convulsive spasm, there was suspension of the mental functions; a temporary interruption of consciousness, and therefore of volition. But in chorea we have a different state from either of these: there is no loss of consciousness; no defect of volition; the ordinary movements of the body can be performed in some degree, or sometimes, under the direction of the will; but it would seem as if some other power wantonly interfered to excite them when they are not needed, to

render them unsteady and imperfect, to arrest the natural action, and give a new direction to the limbs, and to cause the patient to gesticulate and grimace like a Merry-Andrew. Moreover, these apparently absurd movements do not occur in paroxysms, but continue throughout the day, sometimes for weeks together; but they generally cease during sleep: for the most part, but not always, the agitated limbs are still, while the senses are shut up in slumber. The complaint is not attended with fever.

Symptoms.—This disorder was first distinctly described by Sydenham, whose account of it is very graphic and excellent, and has been copied by most subsequent writers. Without reference, however, to the portrait which he has left us, I will sketch the disease as it has occurred under my own observation. It usually begins with slight twitches of a few muscles in the face, or one of the extremities; and by degrees the spasmodic action becomes more decided and more general. All the voluntary muscles are liable to be affected by it. Those of the face seldom escape. The features are twisted into all sorts of ridiculous forms; you might suppose that the patient was what is called pulling a face, or making mouths at you; but there is neither mirth nor mockery in the contortion; it is a convulsion: it is succeeded by a vacant look, and then it begins afresh. The disease occurs much oftener in young girls than in any other persons. If you ask the patient to put out her tongue, she makes sundry attempts to do so before she can accomplish it; and then the tongue is suddenly thrust out, and as suddenly withdrawn, and the jaws snap together as if she were resolved that you should have as short a glimpse of it as possible. She writhes and contorts her shoulders; she cannot keep her hand or arm half a minute in the same position; when, at meals, she desires to carry her hand to her

mouth; it is arrested midway, and suddenly pulled back again, or pushed out in some other direction; and it is only after many deviations and fruitless efforts that she succeeds. The lower extremities are equally affected: when the patient intends to sit or stand still, her feet scrape and shuffle on the floor, or one is thrown over the other; and if she endeavours to walk, her progress is most uncertain; she halts, and drags her leg rather than lifts it up, and advances in a jumping manner, and by fits and starts. In short, the voluntary muscles are moved in that capricious and fantastic way in which we might fancy they would be moved if some invisible mischievous being, some Puck or Robin Goodfellow, were behind the patient, and prompted the discordant gestures. With all this the articulation is impeded; there is the same perverse interference with some of the muscles concerned in the utterance of the voice. By a strong figure of speech, the disorder has been called "insanity of the muscles."

Such is a picture of the main symptoms of this strange malady, as they have presented themselves to me; and such, I venture to say, you will often see in your future practice. You will find, moreover, that the irregular jactitations are usually more marked and general on one side of the body than on the other; and sometimes they are confined to the muscles of one side. Here, therefore, we have a trait of resemblance to epilepsy and to hemiplegia. If you take hold of the only limb which happens to be thus agitated, and keep it still by main force, some other limb or part will take on the convulsive action. The persons who are subject to chorea are always inordinately sensitive, and what is popularly called "nervous." They are easily moved by new ideas and sudden feelings, and pass readily, and upon slight occasion, from one mood of mind to another. The mind is affected, as Dr. Cullen remarks, in the same way, and often shews the same varied, desultory, and causeless emotions, as in hysteria. You see the indication of this nervousness in the fact that the fidgetty catching of the muscles increases when the patient is spoken to, especially by a stranger—by the physician, for example. The nurses of the hospital constantly tell me that such and such a patient, who has chorea, is much more composed at other times than she is during my visit, when she is surrounded by students, and made the object of their attention. In most cases the jactitations are partly and in some degree under the influence of the will: sometimes the patient seems to give way to them, indulges in or exaggerates them; at other times she can, by making an effort, control them. Many of the patients, espe-

cially such as are old and intelligent enough to understand the directions given them, and to make the trial fairly, can suspend for some seconds the convulsive movements, by taking a deep inspiration, and resting upon it, without expiring, for a little while. Like other spasmodic diseases occurring in moveable constitutions, chorea is liable to be propagated also by a species of contagion, or rather of involuntary imitation. These diseases constantly approximate and touch each other in some of their characters.

Chorea, in this its standard form, is essentially a disease of youth. Sydenham, and Cullen who closely copies him, state that for the most part it attacks boys and girls who have not reached the period of puberty; between the tenth and fourteenth years of their age. These limits are, however, too scanty. It is very common between the eighth and sixteenth years; it sometimes comes on as early as five or six; and now and then it begins in adult life, or in old age. I have already intimated that it is much more frequent in girls than in boys. Dr. Heberden says the proportion is as 3 to 1. Dr. Elliotson, out of 30 patients, had 22 females and 8 males. Of 84 cases reported by Dr. Reeves, of Norwich, 57 were females, and 27 males. Of 72 occurring in Dr. Manson's practice at Nottingham, 53 were females, 19 males. Of 18 cases in the Hampshire County Hospital, 12 were girls, and the rest boys. Now taking all these numbers together, we have 204 cases, of which there were 144 females and 60 males; the proportion is as 12 : 5, or a little more than 2 : 1; and leaving out Dr. Reeves' list—which differs considerably from the others in containing a larger number of males—we have 120 cases, of which 87 were females, and 33 males: this ratio is as 29 : 11, or nearly, but not quite, 3 : 1. I have also observed that the disease occurs much more frequently in children having dark hair and eyes, than in those of a light complexion; and I think I have seen the same remark in some author, but I forget where.

When the disease is strongly marked, or lasts long, there is usually some imbecility of mind manifested; a slight degree of fatuity, and a foolish expression of the features. But this goes off with the other symptoms. The child generally recovers, but the disease is apt to recur, and that more than once. In this respect we may trace a distant resemblance to epilepsy: if we regard each attack as a long and mild paroxysm, then these paroxysms are liable to repetition. No doubt the duration of the disorder is often abbreviated by proper treatment: there are *cures* in this disease as well as *recoveries*. It is a very rare thing for

chorea to prove fatal; and the few fatal cases that have occurred have thrown no light on its pathology. Dr. Elliotson saw a strong girl affected with it die of apoplexy; but perhaps she would have died of apoplexy whether she had had chorea or no. Chorea offers no protection against the invasion of other diseases. My colleague at the Middlesex Hospital, Dr. Hawkins, had a fatal case. He found great vascularity of the uterus, earthy concretions in the pancreas, omentum, and mesentery, and tubercles in the lungs. But these appearances had no connexion probably with the chorea. In an instance that proved fatal under Dr. Bright's observation, there was considerable disease in the uterus and its appendages. I am afraid that we shall seek in vain in the dead body to discern the *nature* of chorea. When we find organic disease accompanying it, we must look upon such organic disease, if it have any connexion with the chorea at all, as being a predisposing cause; as producing or increasing that irritability and mobility of the nervous system which fits it for submitting to the exciting causes of various nervous diseases.

Pathology.—There is a speculation of some of the French writers respecting the seat and nature of chorea so ingenious, that I cannot refrain from mentioning it.

It is affirmed by certain modern physiologists, as you may perhaps know, that one of the functions, the principal office indeed, of the *cerebellum*, is to preside over and regulate the faculty of locomotion; to keep the muscles in due subordination, as it were, to the will. No voluntary movement, almost, can be executed without the combined and consenting action of many muscles: it is the business of the cerebellum, they say, to maintain this consent and community of purpose; to prevent any mutiny of individual muscles, and to make them unanimously co-operate in producing a given movement. How far this doctrine is true I do not intend to enquire; but, supposing it well founded, then they very ingeniously assign the cerebellum as the seat of that change, whatever it is, which gives rise to the phenomena of chorea. And it is most certain that the irregular movements by which chorea is characterized can neither be considered as the effects of imperfect paralysis, as some have stated, nor of convulsion, in the proper sense of that word, as others have asserted; but rather as consequences of the want of due harmony and agreement between the various muscles, which should combine to produce the desired state either of rest or of motion. There is a defect of the requisite association in the action of the different muscles; and it is in this sense that chorea has been denominated *insanity* of the muscles. There is a certain

portion of the brain which ministers to the intellectual functions; there are certain altered states of that portion, which lead to mental aberration; the persons so affected form false judgments, cannot associate their ideas aright. So also there is a certain portion of the encephalon which presides over the locomotive functions; and there are altered states of *that* portion, which lead to a loss of the due association of the muscular contractions. That portion is the cerebellum. Such is their theory: and it is a very plausible and pleasant, but withal an unsatisfying theory. The disorder really belongs, I apprehend, to the excito-motory division of the nervous system. From some unhealthy or unnatural state, either of the cord or of the incident nerves that convey impressions to it, its reflex function is called into irregular play, and voluntary muscles contract independently of volition. Sometimes at the same instant the patient *wills* certain definite movements through the instrumentality of the very same muscles. The consequence is, that the same muscles, receiving at the same time contradictory orders from these two sources, obey neither mandate completely, but give rise, by their discordant action, to the grotesque and seemingly antic gestures which these patients exhibit.

But to leave these seductive theories, and to return to duller matters of fact. Chorea is a complaint that is seldom attended with any bodily pain. I have in several instances, however, known it to be accompanied by pain of the head; and in some of them, with pain on that side *only* of the head which was opposite to the agitated limbs. I mention this as being of some practical importance; for I have found the disease to become sensibly less severe, and very soon to cease, upon drawing blood by leeches, or cupping, from the painful side of the head. In a greater number of cases, however, no such pain is experienced. Sometimes you will find that in all respects, excepting the nervousness, and the irregular movements, the patient is in the enjoyment of perfect health. But neither is this very common: generally there is something manifestly wrong in the state of the stomach and bowels, either before or during the complaint; a capricious appetite, costiveness, a tumid abdomen, offensive breath, a foul tongue.

Complications.—Chorea is sometimes complicated with other disorders, and above all with hysteria: and no wonder, since they both occur chiefly in persons of the same sex, of the same temperament, and at nearly the same period of life. It is said also to happen in conjunction with acute rheumatism and rheumatic pericarditis; and with certain affections of the skin. Its coincidence with cutaneous complaints I sug-

pect to be accidental; and I should not say, judging from my own experience alone, that the disease was often associated with acute rheumatism. Dr. Copland and Dr. Bright have both, however, noted that connexion, and therefore I cannot doubt that it does sometimes exist. I certainly have seen jactitations like those of chorea in a few instances of rheumatic carditis. Very lately a boy, affected with chorea, became my patient in the hospital. We soon detected a strong bellows-sound of his heart; and tracing his history back a little, we found that he had suffered acute articular rheumatism. In a recent volume of the *Medico-Chirurgical Transactions*, there is a paper by Dr. Bright, detailing "cases of spasmodic disease accompanying affections of the pericardium." Now we do not perceive any obvious or direct connexion between the cardiac disorder and the nervous disorder. There are just two conjectures which occur to me upon the subject. Rheumatism (as we shall see by and by) is especially a disease of fibrous structures, and it usually affects various fibrous parts at the same time. It is not improbable therefore, that, in the cases in question, some morbid condition of the membranes of the spinal canal may have arisen, simultaneously with the inflammation of the pericardium; or the cardiac disease may perhaps operate, by some ill-understood influence, upon afferent nerves of the cord, as an eccentric cause of the irregular movements.

Causes.—Probably any thing which makes a strong impression on the nervous system may act as an exciting cause of chorea. Strong mental emotion, or a sudden mental shock, is very likely to bring it on in those of a moveable constitution who are predisposed to it. Of its ascertained or alleged exciting causes, *fright* is beyond all comparison the commonest. It has been known to follow a blow or fall on the head; but even in these cases the alarm may have had more to do with the disorder than the blow itself. It sometimes seems to depend upon irritation of the stomach or bowels, by improper diet, by accumulated fæces, or by worms; and it is found to be connected, in not a few cases, with difficult and painful menstruation. It frequently begins about the period of the second dentition: the late Dr. Gregory, of Edinburgh, was in the habit of relating instances of that kind. In one case, the old teeth were remaining while the new ones were appearing by their sides. The old teeth were drawn, and the removal of the chorea was complete. This Dr. M. Hall would justly call eccentric chorea. But even in such cases the state of the gums cannot be regarded as the sole cause of the chorea: there must be the predisposition, as well as the accidental exciting cause; for the com-

plaint is apt to recur under the agency of some new irritation, and may then be removed by other means.

Chorea, such as I have been describing it, may last from a week or two to some months. In those eighty-four cases which I have already mentioned as having been reported by Dr. Reeves, the shortest period of medical treatment was two weeks; the longest eight months; and the common average seven weeks. This appears to me a long average. The disorder often terminates—at any rate much more often than epilepsy does—at the period of puberty; especially upon the first coming on of the menstrual discharge in the female.

I had occasion, in the last lecture, to remark, that when a vast number of different drugs are recommended as specifics in any given disease, we may sometimes infer from that very circumstance that the disease is difficult of cure, and generally intractable under all plans of management. But there is another class of diseases which a variety of drugs are supposed capable of curing,—those, namely, which tend to terminate in health. I believe that many cases of chorea—most cases—would at length get well without any aid from physic: I believe also that many of the boasted specifics have been quite innocent of any share in the recovery of the patients to whom they were administered; at the same time I am quite certain that treatment has a great influence over the disease.

Treatment.—It was Sydenham's practice first to bleed and purge his patients, and then to administer bitters, aromatics, and antispasmodics, with the object of strengthening the nerves. After his time the blood-letting and purgatives fell into disuse, until the publication of Dr. Hamilton's well-known work brought the latter deservedly into favour again.

A main part, however, of the treatment, consists in the employment of such measures as are calculated to give stability to the moveable nervous system. I can confidently recommend you to abstract blood locally in those cases in which there is a fixed pain in the head; but with this exception, blood-letting is neither useful nor even, in my opinion, justifiable.

I shall not attempt to distract your attention by discussing the various remedies that have been vaunted against chorea; but shall take the liberty of referring you to books (to Dr. Copland's *Dictionary*, for example) for farther information on that subject, and content myself with telling you what modes of treatment I have been in the habit of employing, with very satisfactory results. I think, then, that, setting aside the complication with headache just mentioned, you will be able to deal successfully with most of

the cases of chorea which you may have to treat, if you have at your command purgative medicines, the shower bath, preparations of iron and of arsenic, and oil of turpentine.

It will be right, in all cases, to begin by clearing out the bowels by calomel and jalap, or some active aperient; and you should persist in the regulated use of purgative medicines, if they continue to bring away much fecal matter. We are to be guided less by the amount of the doses than by the effects they produce; at any rate one full evacuation of the bowels should take place every day. But though purgatives are good auxiliaries, we cannot trust to them alone for the cure of the complaint.

One of the most effectual of the tonic remedies is the cold shower bath. If the patient be of a feeble constitution, the water may at first be used tepid; by degrees it should be used cold. This remedy should be employed every morning, or every other morning, early, as soon as the patient gets out of bed. Of the best indications of the propriety of its continuance I spoke in the last lecture only; I need not tire you, therefore, by repeating the observations I then made.

With this external tonic it will be right to combine some internal one; and for the most part the best for the purpose is some preparation of iron. The carbonate of iron is an exceedingly good form, and it may be given in the way recommended by Dr. Elliotson, one of whose *pets* it is,—namely, mixed with twice its weight of treacle, so as to form an electuary. You may begin with it in half-drachm doses, and presently increase the quantity to a drachm, or a drachm and a half, or two drachms. Much larger quantities than this have been given, and that for a long time together; but I am not in the habit of *so* pushing this drug. Patients do not like to swallow from half an ounce to an ounce of the powder and twice as much treacle three or four times a day; and some of them cannot get so much down. And I mentioned on a former occasion that the iron is apt to accumulate in the large intestines, and to be expelled at last, often with difficulty and pain, in large, hard, red masses, like what is called, I fancy, slag, or the dross of iron ore from a furnace. When one or at most two drachms given three or four times a day make no impression on the disease, you had better (in my humble judgment) change the form of the medicine. Give a grain or two of the sulphate of iron for a dose, or frequent draughts of Griffith's mixture (*mistura ferri composita*), or twenty or thirty minims of the *tinctura ferri muriatis*. Dr. Bright says he has found the sulphate of zinc answer when

the carbonate of iron had failed, and the iron succeed when the zinc had done no good. One most severe case, about which I was consulted, and which had resisted other remedies, got well under the use of the sulphate of zinc; the dose of which was gradually increased to ten grains, given three times a day. Whenever the medicine was pushed beyond this point it became emetic. Certainly the disease is often very obedient to arsenic; but, for plain reasons, it is better to effect a cure, when we can, by less hazardous substances. The severest case I ever had to treat occurred, not long since, in one of my hospital patients. I tried the carbonate of iron in vain. The shower bath so terrified and agitated the girl that I could not persist with it. I then gave her arsenic, under which she improved at first, but it ultimately was very injurious; her bowels were greatly irritated by it, she became paralytic in her lower extremities, and sunk into a typhoid state; and I really was afraid that I should lose her. But she recovered from this condition, which I could not but ascribe to the arsenic; and as soon as I dared venture, I began to give her the muriated tincture of iron, twenty drops at a time, every six hours. Under this treatment she steadily and rapidly improved, and was soon quite well.

The oil of turpentine also is certainly a valuable medicine in this disease; whether there be worms at the bottom of it or not. When the bowels are torpid, and the girl is of that age that menstruation may be conjectured to be at hand, its arrival seems sometimes to be accelerated, and great relief to be produced, by the turpentine. The best way of exhibiting it in such cases is in combination with an equal quantity of castor oil: two drachms or half an ounce of the *mixture* may be given every morning, or every other morning, according to its effect upon the bowels: and when they are very sluggish, or the stools are unnatural, it will often be serviceable to give a couple of grains of calomel also, twice or thrice a week, at bedtime.

It is scarcely necessary for me to say that in all cases due attention must be paid to the diet. This ought to be plain and simple, but at the same time nourishing, or even generous. And exercise, short of that which produces fatigue, in the open air, in fine and dry weather, will greatly conduce to the patient's recovery.

Chronic chorea.—There is an affection (it scarcely deserves to be spoken of as a disease) which is sometimes called chorea, or a chronic nature, and resembling the disorder I have just been speaking of inasmuch as it commonly is met with in nervous persons,

and consists in the irregular, unmeaning, and involuntary contraction of certain muscles, especially of the limbs, neck, or face: but differing from it in this—that the same muscles are always affected, and in the same way; that it lasts long, almost always for life, and implies no accompanying derangement of the general health. In its slighter form the irregular movements are rather *awkward tricks* than spasms: a repeated shake of the head, or knitting of the eyebrows, or corrugation of the integuments of the nose, or shrugging of the shoulders—which the person seems scarcely conscious of. At other times, however, the motions are more extensive; a limb starts out, or the head is turned awry; and the individual who performs these evolutions is quite aware that he does so, and vexed and annoyed at the ridiculous figure he makes, but he cannot help performing them; or if he can prevent it, the necessary effort is worse than the disease. One young man who was subject to this infirmity told a friend that he could stop the movement by a strong exertion of the will; but that *that exertion* was extremely painful, and was followed by languor and much discomfort. In some instances I make no doubt that the continuance of the affection is the result of a long-established *habit*. It occurs more frequently in men than in women. I had for a long time, as an out-patient at the hospital, a girl about seventeen years old, in all other respects the picture of health, but who was annoyed by an involuntary shake of the head, which took place two or three times in a minute. She received no benefit from medicine. A lad in my own service was affected in a similar manner. He seemed to be giving me, and my friends, from time to time, a familiar nod: and I was obliged to part with him. Others are subject to twitchings of the face. I am acquainted with one gentleman who is perpetually wrinkling his nose; and he has assured me that he was subject, when young, to an involuntary shake of the head, like the two persons just mentioned; but a blister having been once applied to his throat for some disorder in his air-passages, the shaking of the head was thereby rendered painful and difficult, and the movement there ceased: but (as he expressed it) it broke out in his nose, where it triumphs to this day. This *chronic chorea*, as it has been called, I merely mention to prevent your confounding together two affections which, though they have received the same name, and are in some respects analogous, yet differ in still more points, and those points of more importance. I believe that medicine has no power over any of these tricks. They are distressing and unsightly; but in no way dangerous.

The word *chorea*, which you know signifies a *dance*—and the trivial term of St. Vitus's *dance*—are not very appropriate to *either* of the modifications of the nervous affection which I have been noticing. In fact that term was originally applied, and much more suitably, to another set of symptoms of a most singular kind, concerning the real occurrence of which we might well be sceptical, if we had not authentic narratives of many instances of such disorder from different persons of credit, as well in this country as in others. What has happened, many times, before, may happen again; and you ought not to be in ignorance of the histories to which I allude. They relate to an affection characterized by movements that cannot be called spasmodic, but were rather owing to an irresistible propensity to muscular action, increased sometimes to a sort of mania by the force of imitation, or by the sound of music. It is the *volition* that, in these cases, is morbid and perverse. You might fancy the patient to be possessed and coerced by an evil spirit, like the *δαιμονιζόμενοι* of the Gospel history.

Some of the subjects of these extraordinary affections, impelled by a strange unintelligible necessity, execute measured and regular movements with surprising energy, rapidity, and perseverance. When music is performed in their hearing, the movements become an actual dance; and where crowds are collected together, the dancing mania is apt to spread from person to person by a sort of imitative infection; realizing the fable of Orpheus, and giving origin (it may be presumed) to those romantic legends, met with in the literature of most ages and countries, of universal, involuntary and unceasing saltation, at the sound of a magic pipe. To these feats the term *chorea* is apposite enough. Indeed I have seen it somewhere suggested that the phrase *chorea Sancti Viti* is but a vulgar corruption of *chorea Sancti inviti*; and took its rise in the misfortune of some holy person who chanced to be afflicted with one of these unwilling but invincible impulses to caper. The common explanation makes this holy person to have been a certain German Saint Weit, to whom a chapel is said (I know not with how much truth) to be dedicated at Ulm, in Suabia.

Sometimes, instead of dancing on their feet, these patients drum and beat with their hands, either upon their own knees, or upon the objects near them. This variety has received the bombastic title of “malleation.” Sometimes they circumsolve with great rapidity; or they turn their heads repeatedly from side to side with great velocity: this is “rotation.” When they are irresistibly impelled to move in a given direction, the term “propulsion” is employed. The very

invention of these names attests the reality of the disorder.

You will find one of these singular cases related by Mr. Kinder Wood, in the seventh volume of the *Medico-Chirurgical Transactions*.

The patient was a young married woman. After having suffered severe pain in one side of her face, she began to be troubled with involuntary movements. They commenced in the eyelids, which were opened and shut with excessive rapidity. Then the muscles of the extremities became affected. The palms of the hands were beat rapidly upon the thighs, and the feet upon the floor. The motions soon extended to the trunk and pelvis. The patient was suddenly half raised from her chair, and instantly resealed. This was repeated as quickly as one action could possibly succeed another. Sometimes she had a propensity to leap upwards, and strike the ceiling with the palm of her hand; or to touch little spots or holes in the furniture of the room. Or she would dance on one leg, holding the other in her hand. These attacks were accompanied by headache, sickness, and vomiting. At last she took to making steps about the room, regulated by an air, or by a series of strokes on the furniture as she passed: her lips moving as if words were articulated, but no sound escaping them. A person thinking he recognised the tune which she beat on the furniture, began to sing it; and she danced directly up to him, and continued dancing till he was out of breath. A drum and a fife were now procured, and the same air played upon them. She immediately danced up to the drum, and as close to it as possible, till she missed the step, when the motions instantly ceased; and this was found always to be the case. The motions stopped also when the measure was changed; or increased in rapidity beyond her power to keep pace with it. A continued roll on the drum had also the effect of putting an end to her movements. And this being discovered their approach was watched; and by always rolling the drum as soon as they threatened to begin, the chain of association which seemed to constitute the disease was at length broken. The bowels were in an unnatural state during the complaint; and the menstrual discharge appeared on the evening of the day on which it ceased. One might conceive that the conduct here described was an indication of folly or of insanity; but Mr. Wood declares that the patient's spirits were good, and her perception and judgment accurate and just: that during the absence of the paroxysms she went about her household affairs as usual; and that she had a correct knowledge of her situation, and of the advantage she derived from the drum, with an anxious desire to continue its use. She stated "that

there always was a tune dwelling upon her mind, which at times becoming more pressing, irresistibly compelled her to commence the involuntary motions."

In a lady, whom Dr. Abercrombie saw, the following symptoms, among others, occurred:—After she had been ill with various nervous affections for two years, she began to suffer convulsive action of the muscles of the back, and involuntary twitches of the legs and arms, producing a variety of movements of the whole body very difficult to describe. These were much increased by touching her, especially on any part of her back. This is a symptom quite in conformity with Dr. Hall's doctrine of eccentric irritation. At one time there was difficulty of deglutition, so that attempts to swallow produced spasms resembling those of tetanus. At other times, after lying a considerable time quiet, she would in an instant throw her whole body into a kind of convulsive spring, by which she was jerked entirely out of bed: and in the same manner, while sitting or lying on the floor, she would throw herself into bed, or would leap, as a fish might do, on the top of a wardrobe fully five feet high. These are feats that surpass the powers of a person in health: and I say we should hesitate to believe them if they were not related by a physician of such sober judgment and unquestionable veracity as Dr. Abercrombie. He tells us that during the whole of these symptoms her mind continued entire; and the only account she could give of her extravagance was, a secret impulse which she could not resist.

But after a time motions still more wonderful commenced, affecting the muscles of the upper part of the back and neck, and producing a constant semi-rotatory motion of the head. This sometimes continued without interruption night and day for several weeks together; and if the head or neck were touched, the motion was increased to a most extraordinary degree of rapidity. These paroxysms were relieved by nothing but cupping on the temples to the amount of ten or twelve ounces; when the affection ceased in an instant with a general convulsive start of the whole body. She was then immediately well, got up, and was able to walk about in good health for several weeks; when the same symptoms returned, and required a repetition of the same treatment. All this went on, at intervals, for four years; the menstruation during that time being irregular and scanty, and the bowels torpid. She was pale and bloodless from the frequent bleedings, but not reduced in flesh. At last, in the spring of 1829, she had a severe paroxysm of the rotatory motion of the head; and it was then determined to allow the attack to take its course, and to direct the treatment entirely to the menstruation.

Sulphate of iron, and Barbadoes aloes, were prescribed. She went on for three weeks, the convulsive motion of the head continuing without intermission night and day. At length, in the middle of the night, the paroxysm ceased in an instant, with the same kind of convulsive start of the whole body with which it used to cease after cupping. At the same instant menstruation took place in a more full and healthy manner than it had done for many years. From that time she remained well; at least up to the period when Dr. Abercrombie wrote the account.

This alternating rotatory motion of the head is by no means an uncommon feature of these singular cases. It occurred in a patient of Dr. Conolly's; in whom the menstruation was irregular, and about to cease altogether. It came on in paroxysms which were repeated many times a day, and was attended with inordinate loquacity. The head was turned from side to side about eight times in a second, and each paroxysm lasted three or four minutes. The patient got well after being cupped and leeches, and thoroughly purged. I have seen precisely the same thing in a hospital patient. Dr. Crawford met with an instance of involuntary rotation of the head, without pain, but attended with intolerance of light. And there is a striking example of it described in the twenty-third volume of the *Edinburgh Medical and Surgical Journal*, by Mr. Hunter, of Glasgow, who speaks of it under the name of "rotatio or chorea." The motions are said to have been furious and alarming: they were executed with such immense rapidity, that it was difficult even for the eye to follow them. She appeared, Mr. Hunter says, absolutely to be looking backwards and forwards, and in every direction, at the same moment. This woman had sometimes fifty paroxysms of this kind in a day, which greatly exhausted her: but she was perfectly rational in the intervals. A modification of the same kind of affection took place in a most extraordinary case recorded by Dr. Watt, of Glasgow, in the fifth volume of the *Medico-Chirurgical Transactions*. His patient was a girl ten years old. First she had headache, accompanied by vomiting, and increased by the slightest deviation of the body from the erect posture, either backwards or forwards, or to one side. These symptoms lasted about a month; and during that time she lost the power of speech and of walking. At the end of that period she was seized with a propensity to turn round on her feet, like a top, with great velocity, always in one direction; and was pleased when those about her assisted in increasing the rapidity of her movements. After continuing nearly a month, these motions ceased, the headache returned,

and she became unable to move her neck, or support her head. Soon after she was seized with a new kind of motion; she would lay herself across the bed, and turning round like a roller, move rapidly from one end of it to the other. At first the fits of this kind lasted two hours; but they gradually increased to six or seven hours every day. On being carried into the garden she rolled rapidly from one end of a walk to the other: and even when laid in the shallow part of a river, though on the point of being drowned, she began to turn round as usual. The rotations were about sixty in a minute. She made little or no use of her arms in revolving. In about another month or six weeks an entirely new set of movements began. She lay upon her back, and, by drawing her head and heels together, bent herself like a bow, and then allowing her head and heels to separate, her buttocks fell with considerable force upon the bed. She repeated these movements ten or twelve times in a minute, first for six hours daily, and at length for fourteen. After another space of about five weeks had elapsed, the most singular freak of all ensued; she was seized with a propensity to stand upon her head with her feet perpendicularly upwards. As soon as the feet were elevated in this manner, all muscular exertion seemed to be withdrawn, and the body fell down as if dead; her knees first striking the bed. This was no sooner done than she instantly mounted up as before; and continued to do so from twelve to fifteen times in a minute, for fifteen hours a day. After a variety of fruitless treatment, a spontaneous diarrhoea came on, and she recovered.

The spinning motions observed during a part of this case have been observed in other instances.

In Magendie's *Journal de Physiologie*, the two following singular forms of disease are referred to. A man, after some other symptoms of cerebral affection, was seized with an irresistible propensity to move forwards, stopping only when exhausted. He would sally forth into the streets, and continue walking straight forward until he dropped down, and was obliged to be brought home in some conveyance. This man at length died, and several tubercles were found in the anterior hemispheres of the brain. Dr. Laurent, of Versailles, exhibited to the Academy of Medicine a young girl, labouring under the exactly opposite necessity. In the attacks of a nervous disease she was irresistibly propelled backwards, and with some rapidity: being unable to avoid obstacles or hollows, she received many falls and bruises in her course.

I say that histories such as I have been giving you some samples of, and those mostly in an abridged form, would sound

very like romances, if they were met with in the older authors alone, or if they were not attested by unimpeachable authority. They resemble chorea in this respect, that they are examples of muscular actions performed by persons in possession of consciousness, and performed in spite of themselves. But in most other respects they differ from what we now-a-days mean when we speak of chorea. Perhaps they may rank among *hysterical* vagaries. It is remarkable that the majority of them occur in young women, in whom the menstrual function is suspended or irregularly performed. Some persons may consider them as varieties of insanity. The patients certainly did not *feign* to be ill, for the feats of strength and agility which many of them performed were much beyond their natural power or endurance. The truth seems to be, that there are innumerable modifications of the nervous functions, and that some of them are more common and more capable of being arranged into groups than others; but that they all offer points of resemblance, like (as I observed before) the different members of a large family, of which the individuals have the same general cast of features, and yet preserve each his particular identity.

I advert to these odd forms of disease with the view of directing your attention to such of them as may come in your way. We are yet terribly in the dark about morbid affections of the nerves, both organic and functional. Hereafter a medical Newton will arise, and reduce all these apparently complicated phenomena to some simple law. At present all that we can do is to collect and, as far as we may, to arrange facts, in the hope that at length some better light will be shed upon the subject. And it must be observed that some of the modern researches into physiology do throw a little glimmering of illumination into these dark corners of pathology.

In certain of M. Magendie's experiments on animals the following curious facts were ascertained:—When a vertical section of the cerebellum of a rabbit was made, leaving one-fourth of the whole adhering to the crus of the right side, and three-fourths to the left, the animal rolled over and over incessantly, turning itself towards the injured side. The same phenomenon occurred upon the division of the crus cerebelli. The animal lived for eight days, and continued during the whole of that time to revolve upon its long axis, unless stopped by coming in contact with some obstacle. How like is this to the symptoms exhibited at one period in the girl whose case is related by Dr. Watts! Nor is Dr. Watts' case a singular one: M. Serres has described another much like it. A shoemaker, 68

years old, of intemperate habits, after one of his debauches exhibited a kind of drunkenness which surprised his friends. Instead of seeing objects turn round him, as a drunken person is apt to do, he thought he was himself turning, and soon began to revolve; and this lasted till he died: and when his head was examined, extensive mischief was found in one of the peduncles of the cerebellum.

Again, M. Magendie noticed that when the upper part of the cerebrum is gently removed in birds and mammalia, they become blind; but no affection of the locomotive powers is produced. No farther result is occasioned by the removal of a portion of the grey matter of the corpus striatum: but when the striated part is cut away, the animal immediately darts forward with rapidity, and continues to advance as if impelled by some irresistible force, until stopped by an obstacle; and even then it retains the attitude of one advancing. The experiment was tried with the same result upon various species of animals—dogs, cats, hedgehogs, rabbits, Guinea pigs, and squirrels. It seems that there are horses that cannot *back*; although they make good progress enough in a straight-forward direction. Now Magendie says that he has opened the heads of such horses; and has always found, in the lateral ventricles of their brain, a collection of water, which must have compressed and even disorganized the corpora striata. It has further been found, by the same experimenter and by others, that certain injuries of the cerebellum cause animals to move backwards contrary to their will; if the tail of the animal so mutilated be pinched, he still persists in his retrograde course. Injuries of the medulla oblongata had the same effect: pigeons into which he forced a pin through that part, constantly receded for more than a month, and even *flew* backwards. A section of the medulla oblongata, where it approaches the anterior pyramid, gives rise to a movement in a circle, like that of a horse in a mill: the animal, in its walk or its flight, bearing round continually to the injured side. Surely we have, in these facts, supplied by experiments on living animals, and by observation of the phenomena of disease in the living human body, some of the *materials* for a more exact knowledge, both of the physiology and of the pathology of the nervous system, than we have yet reached. M. Magendie supposes that different portions of the encephalon are endowed with energies which tend to cause motion in various directions; that in the healthy state these balance each other, and that a preponderating impulse can be given to any one of these forces by the will; but that when the equilibrium is destroyed by disease, the will is not sufficient to counteract the tendencies which are then

brought into play. Mr. Mayo offers a different explanation of the phænomena. He supposes that the injuries inflicted on the nervous matter produce a sensation analogous to vertigo; and that the animal conceives itself either to be hurried forward, and makes an exertion to repel the imaginary force; or to be moving backward, or turning round in one direction, and endeavours to correct this by moving the corresponding muscles. Whatever may be the true explanation, the facts themselves are abundantly curious and interesting, and I recommend them to your attention.

Some of the affections that I have been describing, fall, perhaps, under the category of those to which the appellation of the *leaping ague* has been given in some parts of Scotland. There is a class also of convulsive spasmodic affections which resemble epilepsy on the one hand, and chorea on the other, or rather form a link of alliance between the two, and which are especially remarkable for this, that they are capable of being propagated by that kind of imitative contagion of which I have several times spoken. This point might be well illustrated by the history of various sects of religious enthusiasts. One or two of those enthusiasts have apparently at first worked themselves up into a state approaching to epilepsy, accompanied even by insensibility sometimes; and then this state has been communicated by sympathy to the more susceptible of their auditors. I must not however go into any farther details on this subject; and perhaps I have prosecuted it too far already: those among you who are inclined to pursue it farther may find some curious accounts of an epidemic which occurred in Lanarkshire, under the head of the "Conversions of Cambuslang," in Sir John Sinclair's *Statistical Account of Scotland*; and in one of the early volumes of the *Edinburgh Medical and Surgical Journal*, Dr. Robertson has described, in an inaugural dissertation *De Choræa Sancti Viti*, a similar epidemic, which occurred in the states of Tennessee and Kentucky, in the western districts of America. This also is referred to in the same volume of the journal. Among other things Dr. Robertson says, that while extravagant sounds, and actions, and gesticulations, were in the first instance *wilful*, the actors "at length, to their own astonishment, and the diversion of many of the spectators, continued to act from *necessity* the curious character which they had commenced from *choice*." I will only remark farther of such forms of nervous disease, that as they spring often from moral causes, so they admit, in a great degree, of moral remedies. The pranks played by the Scotch enthusiasts were brought to an end by threatening to duck every one who should thereafter be at-

tacked; and, I believe, a few of them were horse-pounded, by way of example. With respect to the solitary instances of perverted locomotion, our business must be to correct whatever is wrong in the state of the bowels; in women, to amend the disordered uterine functions; to strengthen and confirm the system generally; and, in addition to the measures proper to effect these objects, I suspect that the cold sousing would in many cases be found of most material service.

ON

CERTAIN PHYSIOLOGICAL INFERENCES WHICH MAY BE DRAWN FROM THE STUDY OF THE NERVES OF THE EYEBALL.

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of Edinburgh

[Concluded from page 384.]

III. Let us next attend to the information given by the study of the nerves of the eye, as to the influence and use of the ganglia of the sympathetic nerve, of which it is generally admitted that the ciliary ganglion, furnishing the ciliary nerves, and through which the iris is moved, is a specimen and representative.

On this subject there has been much discussion at different times, which may be set aside as irrelevant or hypothetical, because proceeding on the supposition, that part of the office of the sympathetic, as of other nerves, is to *give* the vital power or energy to the muscles it supplies. It has always seemed to me extremely improbable, that any one of the solid textures of the living body should have for its office to *give to any other* the power of taking on any vital action; and that the only doctrine on this subject which involves no hypothesis, is that of Haller, who regarded every part of the body which is endowed with irritability, as possessing that property in itself, but subject to excitement and to control, of one kind or another, from the nervous system; and the nervous system as exercising that control chiefly, and in the natural and healthy state probably only, in so far as it is the seat and the instrument of mental acts.

This doctrine, excluding the larger masses of the nervous system from all share in bestowing the property of irritability or vital energy on muscles, has received, as it seems to me, the only confirmation of which, in the present state of our knowledge, it stood in need, from the experiments of Dr. Reid,

which were laid before the British Association in 1834, and have since been repeated on warm-blooded as well as cold-blooded animals. These experiments prove, that after the irritability of muscles has been, as nearly as possible, extinguished by irritation, it is perfectly recovered by rest, notwithstanding that all their connections with the brain and spinal cord have been cut.

There is, however, nothing hypothetical or visionary in the assertion as to the nerves, that “*Soli in corpore, mentis sunt ministri* ;” and, therefore, when we observe that all the great organs of involuntary motion, and among others the iris, have nerves which have passed through ganglia, and when we remember that all those organs are beyond the power of the *will*, but are peculiarly liable to control from certain *involuntary* acts of mind, particularly from sensations and emotions, our business is to inquire whether there is any thing in the structure of those parts of the nervous system which can be supposed to unfit them for the one of those offices, and fit them for the other. And if we keep steadily in mind this precise object of our inquiries, we shall find the subject less obscure and intricate than it has often been thought.

When it is stated that the nerves which pass through the ciliary ganglion supply the only muscle in the eyeball, the actions of which are truly involuntary,—that all the truly involuntary muscles of the body have in like manner nerves which pass through ganglia,—and, farther, that all these ganglia appear, from the most recent and careful examination, to be, like the ciliary ganglion, formed of filaments both from the motor and sensitive nerves, it is impossible to doubt that much of what can be ascertained as to the office of this ganglion in the eye must be truly applicable to the other ganglia supplying involuntary muscles in the body.

If we were to assert, however, that all nerves which excite involuntary movements in the body, in obedience to sensation or emotion, are ganglionic nerves, or that it is through ganglia only that these involuntary acts of mind affect the body, we shall be immediately met by various examples of sensations (or the nervous actions which attend sensations) certainly exciting movements through motor nerves destitute of ganglia. Of this the portio dura and phrenic nerve furnish sufficient examples.

But setting aside the supposition that the ganglia are necessary to enable the involuntary affections of mind to act on the muscles, let us inquire how far the opinion long ago stated by Dr. Johnston and others is correct,—that the ganglia intercept the influence of the will,—prevent the voluntary acts of

mind from acting on the muscles which have their nerves only through them.

A decided opinion is given against this supposition, both by Müller and by his very intelligent translator Dr. Baly. The reason given by Müller is this, that as we know from the experiment formerly mentioned of forcibly acting on the muscles of the eyeball, and thereby causing contraction of the iris, that a *motor* influence can traverse the ciliary ganglion, there is no reason to suppose that a *voluntary motor* influence should be arrested in it, if really brought to it. He considers it, therefore, more probable, that the fibres of the “sympathetic, at their origin in the spinal cord and brain, are not in communication with the source of the voluntary influence ;” *i. e.* that they are not set on the fibres by which the will acts downwards from the source of voluntary power ; to which Dr. Baly adds, that to suppose the admixtures of other fibres in the sympathetic to have the effect of removing the motor cerebro-spinal nerves from the action of the will, is in opposition to one of the fundamental principles in physiology, that of the course and influence of nerves in their “peripheral part,” *i. e.* at a distance from the brain and spinal cord, being *insulated*,—*i. e.* admitting of no admixture or transference of power from one filament to another. These authors, therefore, regard the ciliary nerves as beyond the influence of the will, by reason of the mode of their origin, not of their passing through the ciliary ganglion.

But, on the other hand, if we attend to the experiment insisted on by Müller, we shall see that its result is not correctly stated by his expression, that it shews that a *motor* influence can be transmitted through a ganglion, and therefore gives us reason to presume that an effort of volition could traverse the ganglion also, if really carried to it. When the third nerve transmits an effort of volition to the muscles of the eyeball, and at the same time causes contraction of the pupil, it is plain that the influence which affects the iris has originated in the “source of *voluntary* influence” in the brain,—that is, not only a motor influence, but one consequent on a voluntary effort, which has traversed the ciliary ganglion. The ganglion has not prevented the influence of volition from acting on the nerves and muscular fibres which it supplies, although the will has no power of regulating the movement of these fibres ; and this being so, I do not see how it can be denied that it has modified, in one way or other, the endowments of the nerves entering it ; rendering them incapable, not of transmitting the influence of the volition, but of obeying any specific efforts of the will.

In fact, if it were in consequence of their roots having no connection with the motor portion of the brain and spinal cord, that the ganglionic nerves in the eye or elsewhere are not obedient to the will, and if the nerves underwent no change of endowment in the ganglia, we do not see why the motor nerves of the involuntary muscles (*e. g.* the motor filaments of the ciliary nerves) should pass through ganglia at all; they would be fitted for their function merely by their mode of origin.

Nor does it seem to me difficult to define a little more precisely the modes in which, in this as in other instances, by the connection established in every one of the ganglia of the sympathetic between motor filaments from the anterior, and sensitive filaments from the posterior, column of the spinal cord, the involuntary muscles, although we believe them to be supplied with motor nerves through the ganglia, are withdrawn from the power of the will.

1. Even if we implicitly rely on the experiments of Valentin and others in Germany, tending to correct the previous statements of Haller, Bichat, Wilson Philip, Mayo, and many others, and to shew that all the involuntary muscles may, under certain circumstances, be excited by physical irritations applied to their nerves*,—yet I think it cannot be doubted (from the negative result of so many experiments made previously by so many experienced physiologists) that the power of the motor nerves to excite muscular contraction is *greatly diminished* by passing through ganglia. The contractions, so excited in involuntary muscles in these experiments, have followed irritation *above* the ganglia, or even in the central masses, much more surely than in the nerves *below* the ganglia; and their force, and the certainty with which they can be produced, are certainly much inferior to those of the contractions excited by similar means through nerves not ganglionic, *i. e.* voluntary muscles.

2. The vital agency of the *sensitive* nerves passing through the ganglia seems also to be much modified; they certainly do not show on irritation, when in the natural state, nearly as much sensibility as other nerves; and their grand peculiarity seems to be, that although supplying the muscular fibres, they are incapable of transmitting those muscular sensations by which, in the case of the voluntary muscles, we are continually informed of the contractions we excite. Although the study of the eye teaches us that the influence of volition can traverse a ganglion, yet in no one instance in the body is this influence *felt to be exerted* on muscles placed beyond ganglia. And when we reflect

on what has been said of the importance of the resulting and guiding sensations, in *insulating and directing* the efforts of the will, we shall easily perceive that the want of any such sensations in the present case, is sufficient to explain the inefficiency of voluntary efforts over those muscles. These seem to be results of the degree of intermixture of the motor and sensitive filaments (with the interposition of grey matter), which takes place in the ganglia, instead of taking place at the extremities of the nervous filaments in the muscular fibres themselves.

It is very well worthy of notice that there is one action of the eye, in which the ciliary nerves are essentially concerned, and in which there is a distinct *resulting sensation* consequent on their action, and in that action the ciliary nerves and the iris may be said to act in obedience to the will: I mean that still mysterious effort, whereby the eye increases its own refracting power, and so enables the rays from an object brought gradually nearer it, to form a distinct image on the retina, and excite a distinct sensation in the mind; which effort is uniformly coincident with a gradual contraction of the pupil. Here an effort of volition is made in the direction of the eye, and the continued gratification of the sense, resulting from that effort, in so far as it affects the refractive power, seems to act the same part there, as the gratification of the sensations in the chest, in regulating the contractions of the muscles of respiration.

However, I am aware that objections may be stated to these speculations; and probably it is wiser to rest at present on the general inference, deducible from a comparison of the ganglionic nerves of the eye and of other parts, that when the sensitive and motor filaments which connect a muscle with the spinal cord meet in a ganglion before reaching the cord, their endowments are so far modified that the sensations thence resulting are rendered less precise; that the efforts of the will cannot be insulated on such a muscle, and, therefore, although capable of being influenced by the will, it is truly involuntary.

But it is obviously part of the design of nature, in the construction of the ganglionic nerves, not only that they should withdraw the muscles they supply from the dominion of the will, but likewise that they should facilitate and increase upon them the power of what I have elsewhere called sensorial influence, *i. e.* the influence attending or resulting from sensations and emotions of mind, which we know to originate, or to be excited exclusively, in the larger masses of the nervous system, and to act with peculiar power on muscles and other organs which have their nerves through the ganglia. Here also the study of the eye gives us important information.

* See Valentin De Functionibus Nervorum, &c. p. 62.

The ordinary action of the iris, in obedience to the stimulus of light, is certainly effected by a reflex action, in which the optic nerve, the corpora quadrigemina, and the third nerve, are concerned, and which has been fully illustrated by the experiments of Mayo, Flourens, Valentin, and others. That the peculiar sensation of light, excited by the impression on the corpora quadrigemina, not only attends the action but regulates its degree, is at least highly probable; although it is right to admit, that the action occurs occasionally in cases of amaurosis, where the patient expresses himself as conscious of no sensation; and I do not think that there is so good evidence of the necessary interposition of mental changes in this action, performed by an involuntary muscle, as in the cases where selected and regulated contractions of voluntary muscles are excited by the reflex function of the cord, as, *e. g.* in the contraction of the orbicularis oculi and of this muscle only, effected through the seventh nerve, on the same sensation being felt.

As the third nerve appears to have roots in the posterior as well as anterior portion of the crus cerebri, it is certainly quite possible that those of its filaments which enter the lenticular ganglion are set on sensitive, not on motor portions of the cerebro-spinal axis; but if so, the observations already made show that they are capable of being excited by an influence acting downwards from the strictly motor portions.

The indirect and probably modified influence, resulting from volition, and transmitted through the ganglia to the involuntary muscles, and of which we have this unequivocal example in the eye, is in itself in all probability an important part of the design of nature in the construction of the sympathetic nerve and its ganglia. I perfectly agree with Müller that it is in this way only that the effect of muscular exercise on the action of the heart, and much of the beneficial strengthening effect of exercise, is to be explained; and this indirect influence of voluntary muscular exertion on the heart is obviously important, as keeping its actions in unison with any occasionally required increase of voluntary muscular exertion; and so enabling us to keep up exertions which must otherwise have failed. And a slighter degree of the same indirect influence of exercise is seen in the movements of the stomach and intestines, which become to a certain degree torpid from inactivity of the voluntary muscles. For this slighter agency of voluntary exertion on the moving organs supplied by the splanchnic nerves, there is probably provision made, in these nerves passing through a greater number of ganglia, before they reach the moving fibres, than the nerves of the heart, and therefore having

the indirect influence of the voluntary efforts transmitted through them in a less degree of intensity.

But it is very important, in reference to the use of the ganglionic nerves, to observe, that the movement of the iris is capable of being effected, not only through the third nerve, but likewise through the fifth nerve and the sympathetic, *i. e.* by all the filaments which form part of the composition of the ciliary ganglion. I shall not enter on the observations which have been made on the differences observed in different muscles in this respect; nor on the speculations of some German physiologists as to the mode of action, particularly of the sympathetic, on the iris; but only observe that the effect chiefly observed from the section of both these nerves on the iris, is a gradual and permanent contraction of the pupil. The influence of both these nerves on the iris is therefore strictly analogous to the kind of influence observed in experiments on animals, from injury of different parts of the nervous system, or the sympathetic nerve, on other involuntary muscles, consisting, as Müller states, "either in enduring contractions, or in a long-continued modification of the ordinary rhythmic action of the organ;" a change, *e. g.* in the number and rapidity of the beats of the heart, or of the peristaltic movements of the intestines; in short, as Haller long ago expressed it, a change of the property of irritability itself, as resident in these muscular organs.

Now, when we apply these observations generally, to the living actions of those muscles which have their nerves from the sympathetic, I think we can be at no loss as to the use of great part, at least, of the structure of this part of the nervous system. These nerves place the organs which they supply in connexion with the whole extent of the cerebro-spinal axis: we know, from the observations now stated as to the iris, that an influence may be transmitted to these organs through any of the nerves entering any one of the ganglia; we know, from such experiments as those of Le Gallois and Dr. Wilson Philip, as well as from the effects of injuries on the human body, that injuries acting on any large portions of the brain or spinal cord, affect the heart at least, if not other of these organs, nearly alike; we know that, in the natural state, all these organs are peculiarly under the control of what I have called *sensorial influence*, *i. e.* an influence resulting from those changes in the nervous system which attend intense sensations and emotions of mind; we know, from various facts, some of which I have elsewhere collected*, that this sensorial influence, although often originating from an

* Outlines of Physiology, p. 398.

impression made on a single point, extends itself rapidly in different directions through the nervous matter, and that it can cross from the sensitive portions of the nervous matter to the motor portions, probably at any part of the spinal cord. The effect of any arrangement which brings a particular muscle into communication with many points of the cord, must be still more decided in regard to this sensorial influence, than as to the influence of volition as affected by a plexus. The purpose of the multiplied origins of the spinal accessory nerve, which appears, from the experiments of Valentin and others, to transmit an influence to a greater number of nerves, connected with the cervical plexus, than had been formerly suspected, and therefore to be essentially concerned in many complex actions consequent on sensation and emotion, is thus easily understood. Some observations already published by Dr. Reid, shew more precisely that in the case of the heart, just as in the case of the iris, the sensorial influence, or one exactly similar to it, affecting the contractile power of the muscle, may be transmitted through different nerves entering the ganglia, and so passing to the muscles; for he found that a violent blow on the head influenced the actions of the heart much less, when the sympathetic and par vagum were cut in the neck, than when these nerves were entire, shewing that a part of that influence passes through these nerves; and on the other hand, he found that when an animal in which these nerves had been cut was under the impression of fear, its heart's actions were quickened nearly in the usual way; shewing that another part of that influence must pass through other nerves. It seems impossible to miss the conclusion, that the arrangements and the communications to those ganglionic nerves are designed and adapted, according to the laws of nervous action,—while they intercept the direct influence of the will,—to multiply and concentrate, on all the organs they supply, that equally certain, equally important, and more varied and extended influence which results from sensations and emotions of mind. And I think it appears clearly, from what has been said, that these are objects which the arrangements of this part of the nervous system must necessarily be so disposed as to secure.

IV. The last question which I shall here consider as elucidated by what we observe in the eye, relates to the mode of transmission of that sensorial influence, resulting, in the natural state, from mental sensations and emotions, which affect the organic functions of nutrition and secretion, and, in all probability, the vital properties and composition of the blood itself, in all parts of the body.

It has been long known that the lacrymal gland is supplied so completely by the fifth nerve, that it must be through a branch of this nerve, almost exclusively, that the passions of the mind, or the sensation of pain excited in other parts of the body, must produce their effects on the flow of tears; and the experiments of Magendie, in which inflammation and ulceration of the conjunctiva and cornea, and ultimate collapse of the eye, followed section of this nerve, and some cases presenting the same series of phenomena in the human body (of which I have myself seen two), have shewn that the nutrition of the whole eyeball, and especially the secretion of mucus on the conjunctiva, are under the control of this nerve. It is hardly necessary to say, that the common expression of this nerve “presiding over these functions,” is vague and unsatisfactory; but that it is the nerve destined to affect these functions, in the way in which nature intends them to be affected by changes in the nervous system, is sufficiently obvious, and is another general principle derived from observations on the eye, and manifestly applicable to the nerves of common sensation all over the body. I have formerly stated a conjecture, which I still think the most probable explanation of the inflammation excited by disease or section of this nerve, viz. that the sensitive nerve, which Sir C. Bell has well denominated the “guard of the organ,” having thus lost its power, the irritations which, in the natural state, are applied to the mucous membrane, and by an action there, attended with sensation, determine a sufficient flow of the natural protecting mucus, now lose their effect, and the membrane is reduced nearly to the condition of a serous membrane, and inflames (as all serous membranes do) merely from the contact of the air.

This influence of sensitive nerves and of sensations, and this consequence of the want of such influence, I take to be an important point in the physiology of other mucous membranes as well as this; but we are more immediately concerned with the question, in what manner the fifth nerve is qualified for transmitting downwards the effect which sensations, even in distant parts of the body, and emotions or passions of the mind, have on the circulation through the eye, and on all its secretions.

The instance of the lacrymal gland, and of the mamma, (which, according to the directions of Müller, has its nerves merely from the intercostals, to the exclusion of the sympathetic), are enough to shew, that the most intense agency of mental emotion may take place through the nerves of common sensation.

I think Dr. Marshall Hall has good reason for the opinion which he has stated, that as

the nerves which supply most of the internal organs of secretion, and of organic life in general, are ganglionic, and as the circulation in the eye itself is liable to influence from section of the sympathetic nerve as well as of the fifth, it is probable that the Gasserian ganglion, and the ganglia on the sensitive roots of the spinal nerves generally, must be designed for the influence of these nerves on secretion and nutrition, not for their functions in regard to sensation; but it seems to me much more doubtful whether Müller is right in his conjecture that the *grey matter* of the ganglia, and the grey fibres passing from them along the nerves, are the parts of the nervous system designed exclusively to affect the organic functions of secretion and nutrition. There are no experiments to shew any such peculiar power in the grey matter of the nervous system; and I can state one fact which shews unequivocally that if it is, as Müller supposes, through the grey matter in the Gasserian ganglion, and of the branches of the sympathetic which communicate, beyond that ganglion, with the fifth nerve, that any emotions or sensations affect the secretions of the eye, that grey matter must itself be acted on by the substance of the fifth nerve behind the ganglion. For in one of the cases of palsy, affecting the fifth nerve on one side, which was long under observation in the clinical ward, it was quite obvious that neither emotions of mind, nor sensations excited in the sound nostril, or in other parts of the body, affected the eye of the palsied side, which, although inflamed, remained always dry when the other was suffused on such occasions. Now, in this case it was ultimately ascertained by dissection, that the diseased (and ultimately wasted) portion of the nerve was behind the Gasserian ganglion, between it and the origin on the crus cerebelli, from which it appears quite certain that the influence of mental sensation and emotion must pass downwards through this portion of the nerve (which I believe hardly contains any grey fibres) on its way from the sensorium commune to the eyeball.

Whatever may be the use of the grey matter in the ganglia, or in other parts of the nervous system, I think we cannot doubt there is here a grand exception to the principle which has been laid down by several authors, that the same nerve is never employed to convey impressions upwards to the sensorium and downwards to the extremities of the nerves. At least, if there be a set of nerves destined solely to convey the influence of sensation and emotion downwards to the organ of organic life, these nerves are every where bound up in the same sheath with the nerves of common sensation, by which impressions are carried upwards to the brain.

Thus the study of the nerves of the eyeball enables us, I think, to give a decided opinion as to the following points;—

1. That all strictly animal muscular movement is not only excited, directly or indirectly, by sensations producing it, but is continually guided and regulated by sensations which succeed and result from it.

2. That it is the province of these resulting sensations, *commencing or anticipated*, to determine on individual muscles the influence of the will; and where distinct animal movements are always consentient, it is because the sensations thus guiding them are the same.

3. That neither the connections of nerves at their roots (so far as anatomy has detected them), nor the plexuses which they form in their course, can be assigned as the cause of consentience of their movements, or of any combinations of their actions; but that the plexuses of nerves, placing both the sentient and motor nerves of the muscles of the extremities in connection with a large surface of the spinal cord, seem to be designed and fitted to render the muscular sensations more distinct, and the acts of the will more energetic, than they otherwise would have been, and thereby to give power, facility, and precision, to the combinations and successions of muscular contractions in all movements of the limbs.

4. That the action in nervous matter, which is excited by an act of the will, can traverse a ganglion, but is never felt to be exercised, and therefore cannot be applied to any specific object, beyond it, apparently because of a modification of the endowments, both of sensitive and motor filaments of nerves, where they are subdivided and intermixed with the grey matter of a ganglion.

5. That the motor filaments of nerves which have passed through ganglia may be affected by changes in the sensitive as well as the motor filaments which enter the ganglia; and that in this way, probably, the influence of sensations and emotions of mind (which must be transmitted through the ganglia, because it affects especially muscles which have only ganglionic nerves), is conveyed from many parts of the spinal cord, and concentrated on the muscles of organic life.

6. That the influence of changes in the nervous system, and especially of such as accompany sensations and emotions of mind, on the capillary circulation, on the functions of nutrition and secretion, and on the properties of the blood, may be transmitted downwards by the nerves of common sensation, and that it is probably with a view to this influence that the ganglia are formed on the roots of those nerves.

ON STAMMERING,

AND THE METHODS PROPOSED FOR ITS
REMOVAL.

BY EDWIN LEE, Esq.

[Concluded from p. 390.]

[For the Medical Gazette.]

WITH regard to the methods of education, the object of which is to regularise the respiration and the action of the different parts concerned in speech, and thus bring them to act in harmony together, there is no doubt that they are calculated to effect the removal of the complaint in a large proportion of cases; but when we consider the length of time required, the subsequent attention necessary to prevent a recurrence, and the power of habit, we cannot feel surprised that many persons should be unwilling to have recourse to them, and that relapses should frequently occur; for the statistical accounts of teachers and practitioners of these methods cannot always be relied upon, on account of the difficulty which must exist in keeping the patients in view for a sufficient time after the termination of the treatment; and if the opinions which I have expressed of the varieties of stammering be correct, there are obviously cases in which these means would be attended with no advantageous result, though all are undertaken indiscriminately. This mode of treatment I should conceive most likely to be attended with success in the nervous kind of stammer, particularly when it has arisen from imitation, and in those cases where there exists little or no physical alterations of the tongue or throat: but even in these it will fail in a certain proportion, and relapses must not unfrequently occur: so that, as stammerers have now other means within their reach by which they might frequently be relieved, it becomes a matter of choice for them to which they would prefer having recourse; but "*l'un n'empêche pas l'autre*," and many individuals whom treatment has failed to relieve may still be cured by operations which are not particularly serious, and in the vast majority of cases entail no unpleasant consequences. And again, in some of those who have undergone operations, educational methods may be had recourse to with advantage when the operation has not

succeeded in removing the infirmity, or has been attended with only a partial success. Having just said that the operations are not generally serious, or followed by unpleasant consequences, I of course do not include that of Professor Dieffenbach, which I consider to be attended with considerable danger, and only to be thought of in extreme cases, when the patient is willing to run the risk; and I have no doubt that many, if not the great majority, of the patients so operated upon would have been relieved by the other operations. It has, I believe, only been performed once in this country, by Mr. Lucas—after the failure of the other operations—with success; and we have seen that the operation by the ligature, by M. Velpeau, appeared to be equally efficacious; though this also is a proceeding by no means to be recommended. Indeed, the hæmorrhage is almost always profuse, and has occasioned the death of one patient, a medical student. Even Professor Dieffenbach speaks of it in such a manner as not to recommend its adoption, unless by very skilful operators. He says, "The importance of so serious an operation, the dangers which may result from it, the loss of the tongue by mortification, a too abundant suppuration, or even the unskilfulness of an assistant, which may cause it to be torn, are so many considerations which require to be fully taken into the account, and which, joined to the difficulty of its performance, will prevent its being undertaken by unskilful operators." The cases in which it might be admissible, and most likely to be attended with relief, are those in which the tongue is of very large size, which impedes the harmonious action of this organ with the other parts concerned in speech.

The division of the genio-glossi muscles or of the frenum and sublingual mucous membrane, on either side, is the operation which I have seen most successful; and in which the success has been the most permanent in bad cases of stammering. I consider that the method of performing it by the mouth is preferable to the subcutaneous operation, not only because it is more easy, and because it may be ascertained with the finger whether all the fibres are divided, but likewise because in a certain number of cases the division of the membrane is sufficient, and the

patient is spared the pain and inconvenience of the second part of the operation. It may be that the three first patients on whom M. Bonnet operated were not completely relieved of the stammer, because the muscular fibres were not entirely divided, as in the last two patients; or it may be, that being of the purely nervous kind, this division of the muscle is not so well calculated to remove the stammer as in the physical kind, where the movements of the tongue are limited, or this organ is more fixed than natural to the floor of the mouth, so as to prevent its being duly protruded. The section of the membrane or muscles of the mouth I conceive to be also best adapted to the third kind of stammer, when there is a combination of the nervous and physical varieties. The chief objections to this operation are the bleeding and inflammation sometimes occasioned by it. In the great majority of cases these accidents do not occur to any prejudicial extent; and by keeping the edge of the knife or the blades of the scissors near to the bone, the risk of bleeding is greatly obviated, as there are only small vessels at this part, though they may in some cases become anormally enlarged, in proportion as the muscles themselves are enlarged. Several of the patients at M. Amussat's lost a few ounces of blood before the bleeding ceased by the application of ice. Hæmorrhage to a considerable extent occurred in one case of Mr. Phillips, but was not followed by any bad consequences. A case likewise happened to myself, in which at least ten or twelve ounces of blood were lost before the bleeding ceased. The muscles of the tongue, and beneath it, were very much developed in this case. The patient's pulse was quick and full, and the bleeding did not induce faintness, so that I consider it was rather beneficial than otherwise. This was a bad case of mixed stammer; the patient being of a very nervous temperament. The stammer was a good deal relieved, but he still stammers much when excited.

A case of alarming hæmorrhage recently occurred to M. Guersant, but it is not attributable entirely to the operation, as there existed in the boy a hæmorrhagic diathesis, of which the operator was not previously aware. He was subject to repeated bleedings from

the nose, and, only a few days before, the extraction of a tooth had given rise to troublesome bleeding. There was very little loss of blood at the time of the operation, but it took place subsequently, and the greatest quantity of blood was lost three days after the operation, so that it did not occur from the division of any particular artery. The actual cautery was twice applied, ice and other means having been previously found ineffectual; but the hæmorrhage ultimately ceased, and the boy's pulse was not particularly depressed. M. Guersant had performed the operation on nine other individuals, who considered themselves relieved by it, but the amelioration was not so apparent to M. Guersant. In one of the patients, who experienced the greatest benefit immediately after the operation, the stammer gradually returned as bad as ever. In that case also, where M. Velpeau excised a triangular portion from the apex of the tongue, the advantage resulting from the operation was not permanent; so that in these cases mental impression must be considered as the cause of the immediate advantage, as it is evident from M. Velpeau's account that there was no physical alteration of the tongue itself; and I purposely mentioned the case of epilepsy, in which he tied the facial and temporal arteries, (as well as those of aphonia cured by mental impressions, in the beginning of this paper) in order to illustrate the power of similar agencies over nervous diseases. Almost every practitioner is aware that the attack of epilepsy may frequently be suspended for a longer or shorter period by any means in which the patient can be induced to have confidence: hence the histories of cures of this disease from animal magnetism, as well as from the host of drugs which have in turns been cried up as specifics, but which have only enjoyed an ephemeral reputation. Any thing tending to break the chain of habitual recurrence will also very frequently produce a similar effect; as a burn or other accident. And there is not the slightest doubt but that in a great many of the cases of stammering which have been cured by operation, the advantage may be satisfactorily accounted for in this manner; but, on the other hand, there is no doubt that the operations act in

many cases by removing physical impediments to the associated and harmonious action of the parts concerned in speech, as in many of the individuals whom I have observed, especially in Paris, where in almost all those persons whom I saw, some degree of shortening, thickening, or other anormal condition of the tongue existed, (which has led M. Amussat to conclude that stammering depends invariably upon the anormal states, but which, I have no doubt, are, in the majority of cases, consecutive on the nervous kind of stammer, as I have already said), and almost all of whom were either cured, or derived more or less permanent advantage from the operation. In one patient, whom I examined, where it failed to afford any relief, the tonsils were considerably enlarged, which shews the necessity of discrimination. The nine cases operated by M. Guer-sant, in which he did not think any very marked benefit was produced, were likewise in all probability not so well suited to this kind of operation. The circumstance, which I witnessed several times, of the stammer being partially remedied on the section of the sublingual membrane, still more so when the muscles were partially divided, and altogether removed after the division of the muscular fibres which remained attached to the bone, suffices to show the material action of the operation in some cases where the movements of the tongue are not particularly limited; yet, in a large proportion of cases, its muscles are unduly developed, and contract energetically on the introduction of the finger, and consequently, on attempting to articulate particular letters or words which require their active concurrence: here, I conceive, the operation produces a beneficial effect by removing this state of anormal contraction and rigidity, producing a degree of relaxation, (to which likewise the loss of blood is favourable), which enables this organ the better to act in unison with the muscles of the throat and of respiration. Dieffenbach's operation tends to produce a similar result. Hence we may perceive why the spasmodic actions of the face and other parts should so speedily cease after these operations, why the excision of the uvula and tonsils should fail when there is considerable

enlargement of the tongue, or when the motions of this organ are particularly restricted.

With respect to this latter mode of operating, I am convinced that it likewise acts in many cases, though not in all, by the moral impression produced upon the patient: this will be evident on referring to two or three of the cases which I have noted, and also to those reported by Mr. Yearsley. We see in the case of Henry Gee, that he spoke much better when the palate was incised on one side; that he continued to speak tolerably well the first two days, but afterwards he began to lose the advantage, till the uvula was removed, when he again experienced a decided improvement. On the other hand, the man on whom extensive division of the palate had been made without any benefit resulting, had also his uvula removed without any advantage. In some patients the excision of a portion of the tonsil relieved the stammer, as in the case of Henry Smith. I think it not improbable that the presence of these bodies when enlarged may in some cases produce stammering (not, as Mr. Yearsley imagines, by narrowing the throat, or by preventing the free egress of air, but in a similar manner as when the muscles of the tongue are enlarged, and consequently limited in their movements, or when this organ is more bound down than natural), by interfering with the harmonious and associated action of the muscles of the palate with other parts concerned in speech. But these cases are rare when compared with others. The presence of enlarged tonsils must then be looked upon as an accidental coincidence, though when existing this enlargement doubtless tends to aggravate the stammer. In the case I have mentioned, at M. Amussat's, when the section of the genio-glossi failed to afford relief, and the tonsils were a good deal enlarged, it is very likely their removal would have been productive of benefit. This I suggested to M. Amussat, and he promised to perform the operation; but as I left Paris on the next day, I am not aware whether or not it was done. In Mr. Yearsley's case, to which I have already alluded (Crawley), it may be questioned whether the extirpation of the tonsils would not have been attended with considerable relief to the stammer, even if

the uvula had not been excised. I say this, not to call in question the propriety of excision of the uvula in this case, but as a hint for future investigations which may serve to elucidate the complications of stammering. When, therefore, in stammerers, the tonsils are enlarged, the removal of these bodies may relieve the stammer, by allowing a freer play to the muscles of the throat, by which means their action can be better associated with that of the respiratory and lingual muscles. In corroboration of what I have now stated, I may refer to the cases of Charles Geyer, (in Mr. Yearsley's pamphlet), in which the tonsils were enlarged, and the mere excision of the uvula produced no advantage; of J. Topliss, whom the excision of the tonsils sufficed to cure; and also of James Carter, æt. 12, a bad case of stammer from early childhood, in which the tonsils only were excised, with relief at the time, and progressive subsequent improvement, terminating in complete cure. I think, therefore, that in stammerers, when the tonsils are enlarged, there will often be found, in addition to the nervous stammer, a difficulty upon particular letters (the gutturals), and that the removal of the enlarged tonsils would generally be productive of as much benefit as when the uvula is also excised. The excision of this substance, or the division of the palate, may likewise act in some cases by diminishing the state of tension, or tendency to anormal contraction of the palatine arch, which occasions the impediment in articulating, when the person is in a nervous or excited state; but in many, if not in the majority of these instances, the moral impression from the operation is principally instrumental in producing the amelioration, which may be only temporary, or it may be of a permanent nature; but in whichever way it acts it is unquestionable that several persons have been cured, and many have experienced decided relief; so that in certain cases, when the division of the genio-glossi muscles, or of the membrane covering them, does not appear to be more especially indicated, I see no reason why the uvula should not be removed; for, even supposing the effect to be merely mental, that is no reason against its adoption, if the complaint can be by this means removed or mitigated, particularly if it be clear that no

inconvenience results from the loss of the uvula in those cases where it is not attended with success. In cases of purely nervous stammer it is perhaps as likely to be successful as the more severe operation of the section of the genio-glossi, which, however, I consider better adapted to the physical or mixed cases (unless when there is enlargement of the tonsils), and to those of bad stammerers, where a difficulty exists upon particular letters or words. This operation, as well as the others, no doubt also acts in some cases where nothing anormal is perceptible purely by the mental effect which is produced.

No one can have a greater abhorrence of charlatanism, or be less likely to countenance it, than myself: I therefore think it due to Mr. Yearsley (though not agreeing with him as to the nature of stammering, or the manner in which excision of the tonsils and uvula afford relief) to state my belief that he has acted with good faith in giving the correct results of his cases, as far as he was able to ascertain them; and I think that if his operation were performed with more discrimination, he would be enabled to record a greater amount of success.

ON THE DUBIOUS EFFICACY, AND NEGLIGENT PREPARATION, OF DIGITALIS.

To the Editor of the Medical Gazette.

SIR,

SHOULD the accompanying remarks on the dubious efficacy and negligent preparation of digitalis be thought of general interest, you will oblige me by publishing them in your journal.

I am, sir,

Your obedient servant,

HENRY BURTON, M.D.

41, Jermyn Street, May 14, 1841.

Believing that the efforts of physicians to cure disease have been often foiled by the use of medicines negligently prepared, I feel desirous, through the medium of your widely-circulated journal, of calling the attention of the medical profession to the dubious efficacy, and unequal strength, of the tinctures made with the leaves of digitalis purpurea, and dispensed by many druggists.

The leaves of this plant have been frequently employed, during the last

sixty years, in the treatment of various maladies, and many physicians of reputed experience have attested their curative utility. There has, however, been much difference of opinion existing between practitioners on this point; and I believe digitalis is now less frequently prescribed than it was at the commencement of the present century; but whether the disuse of this remedy is a consequence of its inefficacy under all circumstances is open to discussion, and I shall endeavour to point out one or two circumstances referrible to the collecting and preparing the leaves of digitalis, which seem to have led to the introduction of a medicine of inferior strength to that which was employed, more generally than now, at the commencement of the present century.

For the instruction of those among your readers not conversant with the facts bearing on the medical uses and preparations of digitalis which have been recorded, and as preliminary to describing my own observations, I beg to point out to them the work of Dr. Withering, published in 1785, as one which has been long considered a standard authority. A similar work was also published by Dr. W. Hamilton in 1807, on the same subject; and in these two volumes a great many authors are named who had prescribed digitalis in the treatment of different diseases with various results. The reader will derive much information from perusing these works, and by also consulting, in addition, the "Treatise on Consumption," by Dr. Young, published in 1815, in which the opinions of many other distinguished authors, for the most part favourable to the use of digitalis in the treatment of that malady, are quoted; and by perusing the valuable observations of Dr. Blackall on Dropsies, first published in 1813, he will meet with a confirmation of Dr. Withering's opinion of the benefit occasionally derived from the use of this plant in dropsies. Lastly, among the most eminent living practitioners who have written on the same topic, is Dr. Holland, whose testimony of the utility of digitalis, in the treatment of cardiac disease, and as a diuretic, is recorded in his valuable book, entitled "Medical Notes and Reflections."

In the face, therefore, of the numerous opinions recorded by these experienced

and learned authorities, due allowances being made for impartial exaggerations or misconceptions in favour of the medical utility of digitalis, it would be evincing unreasonable scepticism to doubt its efficacy in certain maladies, and I should regret to see this plant condemned as an useless medicine until all the probable sources of error attending its preparation and exhibition had been thoroughly investigated, and unquestionable proofs obtained that no reliance should be placed on the medical qualities of its most approved preparations. My own opinion is, that no sufficient evidence has been adduced of its inefficacy, and that a large share of the contrariety of opinions entertained on this point has arisen from the use of preparations which had been made with the leaves of digitalis unfit for the purposes of medicine, and I confess my own expectations of producing a beneficial effect by them have been often disappointed; but my attention has been only recently directed to the cause of this disappointment.

The value of my theoretical knowledge of the uses of this plant has been assayed during the last six years by the exhibition of its tincture to at least three hundred patients, treated by me in St. Thomas's Hospital, and to whom this remedy was given either with the intention of producing the absorption of effused serum, or of promoting the secretion of urine, or of controlling the heart's action. For several years the tincture was prescribed in the medium doses of twelve minims to twenty minims: they were repeated three or four times daily, and very frequently continued for several weeks without intermission; but notwithstanding the length of time the preparation was administered, yet in only a few cases did any necessity occur to discontinue it, in consequence of the accession of those symptoms which have been insisted upon as signals to withhold the use of digitalis. But this seeming inaction was not invariable, and, in a few cases, one or more of the ordinary effects attributed to digitalis were noticed, and in these there was either a diminution or augmentation in the force and number of the pulsations, and irregularity; or sickness, giddiness, and impaired vision; and in a few patients the secretion of urine was notably augmented. What then was the reason, it may be

asked, why, notwithstanding the statements made in favour of the efficacy of small doses of digitalis, and published by the authorities above quoted, some evident effect was not more frequently noticed in three hundred patients? Not, I believe, in consequence of inattention to the directions given by these authorities for the exhibition of digitalis; nor from a neglect to examine my patients: but after taking into account the nature of their respective diseases, their strength, ages, and peculiarity of constitution; the preparation, dose, combination, and repetition of the medicine; and comparing the effects produced under circumstances as nearly similar as the nature of medical inquiries will permit, with those under which digitalis was given by Dr. Withering, Dr. Hamilton, and Dr. Blackall, with manifest benefit, I think the absence of sensible effects in the majority of the patients treated with its tincture by me, may be justly attributed to the indifferent quality of the leaves employed in making this preparation, and to the smallness of the dose in which it was prescribed.

The former source of error was foreseen by Dr. Withering; and Dr. Holland observes, "It is certain that many of the inequalities and seeming anomalies in the effects of digitalis are owing to neglect of the qualities of its preparations." Dr. Blackall also, in speaking of the dosing with digitalis in dropsy, says, "One considerable source of inaccuracy certainly arises from the various forms in which this medicine is prescribed—of infusion, of tincture, of powder, and the extreme difficulty of making a just calculation of the relative strength of these."

Now it is manifest that unless each form is exhibited in equivalent doses, and has the same uniform strength, the effects produced by them respectively, when given in doses nominally the same, will necessarily differ, and probably occasion an erroneous judgment of the efficacy of digitalis. But the same form has not had at all periods of the history of digitalis the same invariable strength; and, with respect to its tincture, it has been made with the same kind of materials, applied in at least three different proportions, since it was first recommended by Dr. Darwin, to whom we are indebted for suggesting the use of the saturated tincture. The

tincture described by Dr. Darwin was made with two ounces of the dried leaves and eight ounces of proof spirit, or in the proportion of one ounce of the leaves to four fluid-ounces of the spirit (*Hamilton*, p. 82): whereas, in the tincture directed to be made in the London Pharmacopœia of 1824, the materials were applied in the proportion of one ounce of the leaves to eight fluid-ounces of the spirit; and in the edition of the same book published in 1836, the proportions there stated are as one ounce of the leaves to ten fluid-ounces of spirit. Now, taking as assumed facts, that all the virtues of the leaves are imparted to the proof spirit employed in making the tinctures, and that ten minims of the tincture directed to be made in the London Pharmacopœia of 1836 is a medium dose, these ten minims will be the equivalent dose of one grain of the powder; whilst ten minims of the tincture directed to be made in 1824 will be the equivalent of one grain and a quarter; and ten minims of Dr. Darwin's tincture, of two grains and a half of the powder. Hence, as equal measures of these several tinctures contain unequal quantities of the active principle, they must *cæteris paribus* produce different results when exhibited in doses nominally equal; and from the use of these dissimilar tinctures has arisen one source of the disagreement among practitioners relative to the medical powers of digitalis. It is often a point of great consequence to the success of the treatment, that the strength of similar preparations should be uniform, and the prevalent opinions respecting the medium doses of each correct. Now the prevailing opinion, relative to the medium dose of digitalis in powder, seems to have originated with Dr. Withering, and requires notice. "I give," he remarks, "from one to three grains of this powder twice a day: in the reduced state in which physicians find dropsical patients four grains a day are sufficient," (p. 181); and Dr. Blackall also observes, "that even these doses are not free from the possibility of injury, and not fit to be continued without the watchful care of the medical attendant" (page 313). Dr. Withering adds, that half a drachm of the powder, given to the extent of four grains daily for five days, may be in general taken before nausea com-

mences." But this quantity of the powder recommended by Dr. Withering, as well as that of the tincture prescribed by myself, has been much exceeded by other practitioners, two only of which it will suffice to name in evidence of the maximum quantity of each preparation which has been swallowed without injury: thus, in the *Med. and Chir. Rev.* July, 1834, notice is taken of the experiments of M. Andral, from which it appears he gave to one patient as many as 206 grains of the powder in twelve days, or at the rate of seventeen grains and a half nearly each day: and in the *MEDICAL GAZETTE*, vol. i. 1828, it is stated by Dr. Williams, that a drunken man swallowed a fluid-ounce of the tincture twice, at short intervals, without any deleterious consequences. From these statements, among many others which might be quoted, one of two inferences may be drawn; either that the doses in which digitalis was given by Dr. Withering were ridiculously small, and his authority unworthy of credit; or that the quality of the digitalis used by M. Andral, and alluded to by Dr. Williams, was comparatively speaking inert. The latter inference seems to be the most correct: for Dr. Withering, as well as other credible authorities, agree in stating, that in a great many cases digitalis has been given in small doses with manifest effects on their patients; and there is also sufficient evidence recorded of the poisonous nature of digitalis: and speaking of the danger of large medicinal doses of it, Dr. Blackall says, "the results have been some unexpected recoveries much talked of, and more failures which tell no tales" (p. 301.)

I can, therefore, only attribute the very different effects produced by the preparations of digitalis, under the direction of different practitioners, to the employment of bad leaves; leaves, for instance, which have been collected at improper periods of the growth of the plant; sometimes from the seedling root; at others, from the second year's root, before their juices have been duly elaborated: such leaves never can furnish a good active remedy, and their characters vary materially from those directed to be employed by Dr. Withering, and all the subsequent writers on materia medica, who have reiterated his instructions. I have pro-

cured the objectionable leaves from several sources for the purpose of experiments, and have compared them with those directed to be employed by Dr. Withering. "The leaves," says this author, "vary greatly in their efficacy at different seasons of the year. They should be gathered after the flowering stem has shot up, and about the time when the blossoms are coming forth." Dr. Hamilton, also, in his book on the Preparations and Uses of Digitalis, says, "The largest and deepest coloured leaves are to be selected and carefully dried."

But let the conscientious practitioner inquire when the leaves are collected, and how they are dried, and he will be much distressed to find that, instead of the leaves being gathered at the period of their growth, as directed by the best authorities, when the stem has shot up for flowering in the second year of the plant's growth, when the leaves have acquired their full development, and their juices have been duly elaborated towards the end of June and July; they are, on the contrary, gathered as Spring shoots, before any vestiges of either stem or flower have become apparent, and before their juices can have had time to be duly matured. At this period of their development the leaf-stalks rise directly from the root, and are cut in tufts consisting of the Spring leaves connected together by the upper portion of the root-stalk. The fact should be also remembered, that digitalis purpurea is a biennial plant, and does not flower in the first year of its growth. It rises from seed, and when its leaves of the first year's growth are fully developed in the autumn, they are gathered and applied to the purposes of pharmacy, in common with the young leaves of the second year's growth.

But the juices of a biennial plant do not arrive at perfection until the second year of its growth; and hence, with reference to digitalis, a double source of error emanates from the use of its leaves before the flower-stem has shot up. It would be useless to extend this communication with descriptions which may be found in every book on Materia Medica, of the mature leaves of digitalis, or of the modes of preparing them. I am, however, desirous of adding a few remarks on the character of the tinctures of digitalis which are dispensed

by the London druggists, with the object of enabling the practitioner to distinguish the genuine preparation from the spurious. I have recently compared together twelve different tinctures of digitalis, eight of which were procured from as many different sources, and four were made by myself; two of the four were made with the mature leaves of the plant, and two with its immature leaves.

The practical results of my observations and experiments are represented in the subjoined tabular view, and they have led to the conclusion, that tinctures made with the mature leaves differ chemically from those made with the immature leaves, and that the greater density and dark reddish-brown colour of tinctures made with the latter description, will, in most instances, point out the kind of leaf employed, and the care bestowed in making the tincture. It may be also stated, that the quantity of extract ob-

tained from equal measures of these several tinctures varied in my experiments, but the quantity obtained from the immature leaves always exceeded that from the mature. The green colouring principle predominated in the mature leaves, and the tinctures made with them. It was also very manifest in the tinctures made by myself with the immature leaves; but in these tinctures it was modified by a large proportion of brown extract, which does not abound to the same extent in the mature leaves, and therefore the green colour of the tinctures made with the latter kind is more distinct and more permanent than the green colour of tinctures made with the immature leaves. The shades of green and brown may be accurately compared by means of bits of white blotting-paper, saturated with the tinctures respectively, and afterwards dried: and in this way minute differences may be observed by reflected light.

Tabular View of the Densities and Colours of Tincture of Digitalis, and of the Weights of Extract obtained from equal measures of eight varieties.

Numbers of the tinctures examined.	Kind of leaves used.	Specific gravity.	Colour.	Weight in grains of extract obtained from one cubic inch of each tincture.	
1.	Immature.	0.9733	Dark reddish-brown; no shade of green.	13.0	Spurious.
2.	Immature.	0.9677	Dark reddish-brown; no shade of green.	12.5	Spurious.
3.	Immature.	0.9646	Dark reddish-brown; no shade of green.	11.8	Spurious.
4.	Probably immature.	0.9610	Light brown, with a very slight shade of green.	10.0	Spurious.
5.	Immature.	0.9583	Lighter brown than No. 4 tincture, but otherwise nearly similar.	10.7	Spurious.
6.	Probably mature.	0.9406	Brownish green.	9.5	Genuine.
7.	Mature.	0.9406	Brownish green, as No. 6 tincture.	Not obtained.	Genuine.
8.	Mature.	0.9375	Light brownish green, and more distinctly green than Nos. 6 and 7.	9.5	Genuine.
9.	Mature.	0.9344	Same as No. 8.	9.4	Genuine.
10.	Immature.	0.9359	When first made same as Nos. 6 and 7, but in a few days exhibited a browner shade.	Not obtained.	Spurious.

In explanation of the results above exhibited of my experiments, it may be briefly stated, that the tinctures numbered 9 and 10 were made by myself as standards of comparison, and supposed to be of the very best quality. They were clear in a few days after being strained, and had a strong bitter taste,

and a smell peculiar to each, which, although not admitting of a description, enabled me to distinguish them from one another.

Tincture No. 9 closely resembled the three tinctures Nos. 6, 7, and 8, which were procured from three of the most respectable London druggists; and

the four tinctures, Nos. 6, 7, 8, and 9, probably possessed all the qualities of the best description of tincture.

Tinctures Nos. 1, 2, 3, 4, and 5, also closely resembled one another in their sensible qualities, but differed from the best kind of tincture in several respects; and although not entirely powerless in large doses, yet so far as a limited number of trials with them entitle me to form an opinion of their therapeutic merits, I think them much less active preparations than the tinctures Nos. 6, 7, 8, and 9. But in order to determine, with an approach to accuracy, their absolute merits as remedies under every condition, and more especially their comparative power in equivalent doses, more numerous observations of their effects on the sick are required than I have hitherto had time to make. They have, however, been commenced, and before the close of the present year will, I trust, be completed.

The Spring shoots of *digitalis*, or its immature leaves, have reappeared in the herbalists' shops, and have been purchased by the druggists, but the season has not yet arrived at which the mature leaves are gathered; and I hope the preceding remarks may have some share in removing an abuse with respect to *digitalis* which has prevailed in London for at least twenty years.

ON THE CONSTRUCTION
AND
APPLICATION OF FORCEPS FOR
EXTRACTING TEETH.

BY JOHN TOMES,
Surgeon-Dentist to King's College Hospital.

[*For the Medical Gazette.*]

UPON having my attention turned to the extraction of teeth, the instruments placed in my hands to effect this small yet necessary operation were—forceps for extracting the front teeth; the key instrument, for the removal of the back teeth; and the elevator, where neither of the former were effective. After using these for some time, I became dissatisfied with forceps as they are commonly made, and with the key, from the direction in which the force is applied.

To supply the place of what seemed to me defective instruments, forceps

variously shaped were constructed, and it is the business of this paper to describe these, with their proposed advantages, as compared with the instruments in general use.

It may be well however, first of all, to state what is required in instruments wherewith to extract teeth most effectively, and why the means commonly used are imperfect.

The indications in removing teeth are—First, to remove the whole of the offending tooth, or part of a tooth; secondly, to remove it with the least possible injury to the contiguous structures, as the gums and alveolar processes; thirdly, to give the patient the least possible amount of pain in the operation.

That method by which a tooth, or the remains of one, can be removed most certainly, quickly, and at the same time with the least amount of injury to the adjoining parts, will also remove it with the least pain.

To fulfil these indications, recourse must be had to an instrument so formed that it shall grasp the tooth alone, and by the required force applied nearly in the axis of the tooth, remove it. Such instruments are forceps; but forceps so constructed that they shall accurately fit the tooth to be extracted, and so fashioned at the jaws, nibs, or blades, that they shall readily separate the gum from the neck of the tooth, to which point they shall arrive by simply placing the extremities of the jaws at the edge of the gum, closing the handles, and at the same time pressing the instrument steadily in the direction of the tooth, till it comes in contact with the free edge of the alveolar process. As the teeth are variously shaped, so will it be necessary to have forceps of different forms; in fact, a pair fitted to each kind of tooth. By forceps so constructed most teeth may be removed in less time than by any other tooth-extracting instrument at present in use; also with less pain to the patient, and without inflicting any farther injury to the gums and alveolar processes than must necessarily result from the forcible separation of a tooth from its natural attachments.

An instrument which in its employment requires that force should be applied to the contiguous parts as well as to the tooth to be extracted, is an imperfect instrument for the required pur-

pose; or an instrument which, by its form and mode of application, requires that greater force should be used than would be necessary for the dislodgment of the tooth, supposing that force to be applied in the most advantageous direction, is also an imperfect instrument, and but ill fitted for the purpose for which it was designed. Such, however, are the imperfections of an instrument in very general use for the extraction of teeth—the “key;” in the application of which the fulcrum is rested on the alveolar process, between which and the fulcrum the interposed gum is subjected to considerable pressure; just as much, indeed, as may be necessary for the dislocation of the tooth. By this treatment the gum is often considerably bruised, sometimes so much so as to lead to suppuration of the injured part, and always to an unpleasant degree of soreness. The force used in extracting a tooth with the key must be much greater than the actual force required to effect the operation, because the power is applied in a lateral direction, a direction in which the tooth offers great resistance, especially in molares of the lower jaw, where the lateral alveoli are strong, and composed of dense bone.

I am quite aware that there are many and eminent surgeon-dentists who use and speak very highly of the *key* instrument; and no doubt it is very much to be preferred to the forceps ordinarily kept by surgical instrument makers: forceps which are applicable to no one tooth in particular, when applied touch only at two or three points, and are quite incapable of being passed down to the neck of the tooth, unless the gum be previously cut away, and even then very imperfectly. Forceps for extracting the molares of the upper and lower jaws are commonly kept for sale: the jaws of these are simply curved at their extremities, and each jaw alike, so that one instrument is equally applicable to molares of either side of the mouth, whether right or left. In many instances teeth may be removed by such means, but if the destined tooth be much decayed, the operation will be difficult and uncertain in its results; and whatever state the tooth may be in, there is great risk of its being crushed, from the circumstance of the pressure made through the instrument not being

diffused over a large surface of the circumference of the tooth, but confined to two or three points. It is no wonder that such instruments should fall into comparative disuse, and be spoken of as suited only to break off sound teeth, or to extract loose ones. These observations apply principally to forceps for extracting the bicuspid and molares. The incisores are generally, though not always, removed with forceps, or with the elevator. The wisdom teeth are sometimes removed by the latter instrument.

Mr. Bell recommends for the removal of the bicuspid and the anterior molares of the lower jaw, the use of the *hawk's-bill* forceps: this instrument, when well made, must be considered as amongst the best of forceps. For my own use, however, I prefer those that have straight blades or jaws, with the curve, if any be necessary, in the handles.

Dr. Flagg, of Boston, has contrived forceps for the extraction of every kind of tooth. I have not been able to obtain any of his instruments, but, from the description and plates given of them in Fitch's “System of Dental Surgery,” I believe they are unlike those I have myself devised.

A work entitled “Operations on the Teeth,” by Mr. Snell, contains a description, with plates, of a form of forceps used by the author for the extraction of the molares. The principal aim of the inventor was to have two points of metal coming out from the edge of each jaw of the instrument; the object of these points being to be passed between the fangs of the teeth. These forceps admit of but very limited use, as the fangs of the molares, especially the second and third, are liable to be agglutinated; which condition, in each particular instance, is known only after the tooth has been extracted: but besides this, it is a matter of some difficulty, in any case, to get these two points between the fangs; neither is the attempt unattended with suffering to the patient.

Forceps for extracting the anterior teeth are to be found every where, though I have not yet met with any but such as were so clumsily constructed, that to remove the stump of an incisor with them would be difficult indeed. Neither will the so-called “Sheppard's safety forceps” be an exception to this

observation. What forceps Mr. Shepard might have had himself I do not know; but those sold as his are next to useless: for although the principle upon which they are proposed to be made is a correct one, yet it is not carried out in the formation of the instruments.

Different shaped teeth requiring forceps shaped to them, it will be necessary to describe partially the teeth, and then the forceps individually, that the peculiar shape of each instrument may be understood. Before doing so, however, I should state that it is quite impossible for any person to extract teeth properly, whatever instrument may be used, especially if the forceps be chosen, unless the operator is perfectly acquainted with the form of each tooth, with the relative position and size of the fangs, with their direction in the alveoli, with the general form of the alveoli themselves, and the positions at which they offer the greatest and the least resistance.

I am not prepared to say that the forms of forceps which I am about to describe are possessed by myself alone, or by those who have chosen to take mine as a copy. Other practitioners, feeling the want of such, may have been led to construct similar instruments. They have not, however, to the best of my knowledge, described them; neither, as far as I can learn, are their patterns in the possession of any surgical instrument maker. Of this I am certain, that the vast majority of the practitioners who extract teeth neither have knowledge or possession of similar instruments. I therefore offer no apology for printing an account of tooth-forceps which, to my view, seem much superior to those commonly employed. Medical men, engaged in general, and especially parochial practice, are very frequently called upon to extract teeth, and by them the operation, for want of better instruments, is generally performed with the key; and not, in all instances, with the most favourable results. I have seen many cases of sloughing of the gums; and now and then exfoliation of the alveolar process. I do not mean to argue that these are the common results of using the key, but they are not very unfrequent consequences when the instrument is in the hands of an unpractised operator.

With well-made forceps no such acci-

dent can occur, if ordinary care be taken; and it is more easy to learn to use forceps with good effect than the key. I believe it requires more practice and care to use the key to good purpose, than any other instrument used to extract teeth; yet, when in the hands of a skilful operator, it is no doubt a useful instrument, and as such should not be neglected, since cases may occur in which it would be of value; though I believe such cases to be rare.

After stating some general conditions which I think inseparable from good forceps, whatever may be their particular form, I will describe those that have been planned or modified by myself; and I do so without wishing to claim any merit as an inventor, but merely as having carried out a principle admitting of general application, which has been neglected, or at least neglected to have been made public.

All forceps whatever should embrace the tooth they are used to extract at its neck; the neck being that part which is between the termination of the enamel and the free edge of the alveoli, and which is covered by gum. In order to arrive at this part of the tooth without difficulty, or unnecessary pain to the patient, the jaws must present an inclined plane, terminating in an edge. The external surface of the jaws of forceps, when closed, should present something like a cone, or parts of several cones, with the apex or apices cut off; and a perpendicular section should present an inclined plane, terminating in an edge, but more or less curved, as may be suitable to the particular instrument. The length from the joint to the edge of the jaws should on no account be greater than will be necessary to allow sufficient space for the reception of the crown and neck of the tooth, so that no strength may be lost.

The fangs of all teeth having a general conical form, forceps, when well made and applied, should be but as a lengthening of the cone towards its base. For removing teeth which are not decayed down to the gums, the ends of the jaws should be square; but when nothing but the fang remains of a tooth, rounded ends are the more convenient, as with that shape they are readily introduced between the fang and inclosing alveolus. Instruments

for extracting stumps should be made altogether lighter, the jaws thin and sharp at their convex ends, so that they may be made to cut rather than tear the membrane connecting the fang with the adjoining tissues.

Forceps should be constructed and used upon the principle of lengthening the tooth for the extraction of which they are intended; thus enabling the operator to move it from side to side, or rotate it if the fang be single, and of a shape admitting of such motion.

After these lateral movements have been produced, the tooth may, unless the fangs have some peculiar position or shape, be raised in a perpendicular direction, leaving as little injury from its removal as the operation can admit.

Forceps for the extraction of the incisores, cuspidati, and bicuspides.

A section through the neck of an incisor of the upper jaw will show that the anterior is larger, and forms part of a greater circle, than the posterior surface. Now the end to be attained in the application of forceps is to apply them over as large a surface as possible, thereby avoiding the chance of fracturing the tooth by the pressure of the instrument. To extract these teeth, therefore, the jaw to be applied to the posterior surface must have a smaller curve than that for the anterior. An average tooth, in this as well as in other cases, when new instruments are required, should be selected and given to the forceps-maker, and he should be instructed to make the jaws to fit the neck of the tooth exactly, leaving sufficient room for the crown of the tooth to be free from pressure, but not more than will be necessary to clear the enamel. When the forceps are closed upon the tooth they should embrace not only the anterior and posterior, but a part of the lateral surface also. The lateral incisores require forceps made upon the same principles, but somewhat less in size. These are liable to greater variation in external dimensions than any other teeth. Sometimes they are very small indeed, at other times they are almost as large as the neighbouring teeth. It will be advantageous therefore to have different sizes of instruments from which to select.

The incisores of the lower are smaller than those of the upper maxilla, and much more compressed laterally. For-

ceps for the extraction of these teeth will require to have the jaw which is to be applied to the posterior smaller than that for the anterior surface of the neck. The jaws of the instrument should be straight; but it will be found convenient to have the handles curved, so as to avoid the upper maxilla, when the mouth cannot be opened wide. The cuspidati require for each a pair of forceps made upon the same plan as those for the removal of the incisores, except that they must be larger and rather stronger. Those for the cuspidati of the lower jaw should, like forceps for the incisores of the lower jaw, have the handles slightly bent. Sometimes these teeth are very small, in which case forceps adapted to the adjoining teeth may serve for their removal.

The bicuspides will be extracted with instruments similar to those already described, except that there will be a little difference in the jaws, which must be accurately fitted to the neck of the tooth. These teeth are not very frequently liable to much variety in size, so that an instrument which is well adapted to an ordinary bicuspid tooth will apply itself to almost all. I have forceps in which the jaws are bent at right angles with the handles, and opening laterally, for the extraction of bicuspides of the inferior maxilla. But they do not answer so well as straight instruments, it being less convenient to apply the necessary force, and more difficult to regulate its direction. In extracting teeth which have their fangs laterally compressed, and are placed in a row with other teeth of like-shaped fangs, the only available movement will be at right angles with the row, and in the direction of the greatest diameter of the fangs. This may be obtained whether the forceps be straight or rectangular; but with an instrument of the latter shape the movement must be an attempt at rotation with a motion upwards. The centre of the rotatory movement will be either at the extremity of the jaw of the instrument, towards which the hand is turned, or else in a line with the handles of the instrument and wrist. Force applied in this manner would seem to be given at great disadvantage, and much expended on the alveolus; there inflicting injury, which, although in the vast majority of cases scarcely complained

of by the patient, prevents the mouth from so speedily recovering from the operation.

Forceps for extracting the molares.

The molares of the superior maxilla have three fangs—two external, one internal. Of the two external fangs, the anterior is the largest, and is placed in a plane external to the posterior fang, which is shorter as well as smaller. The third, the internal fang, is thicker

FIG. 1.

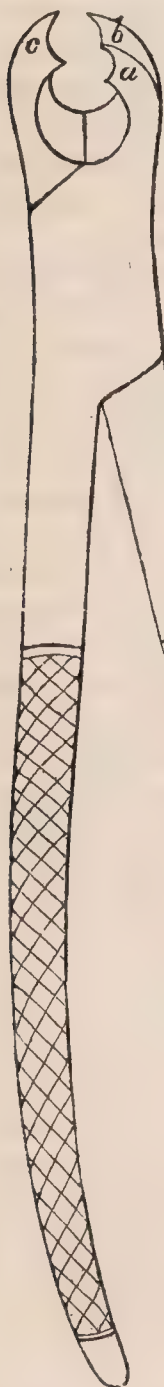


FIG. 2.

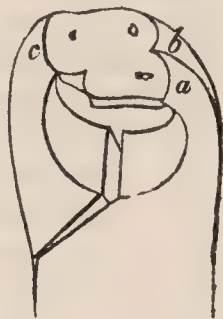


FIG. 1. Forceps for the extraction of molares of the right superior maxilla.

a, the anterior groove, and *b*, the posterior groove, to receive the convexities of the external surface of the neck of the tooth.

c, the groove for the reception of the base of the internal fang.

FIG. 2. The same forceps, with a section of the neck of a molaris from the right superior maxilla in the grasp.

The letters refer to the same parts as in the preceding figure.

FIG. 3.

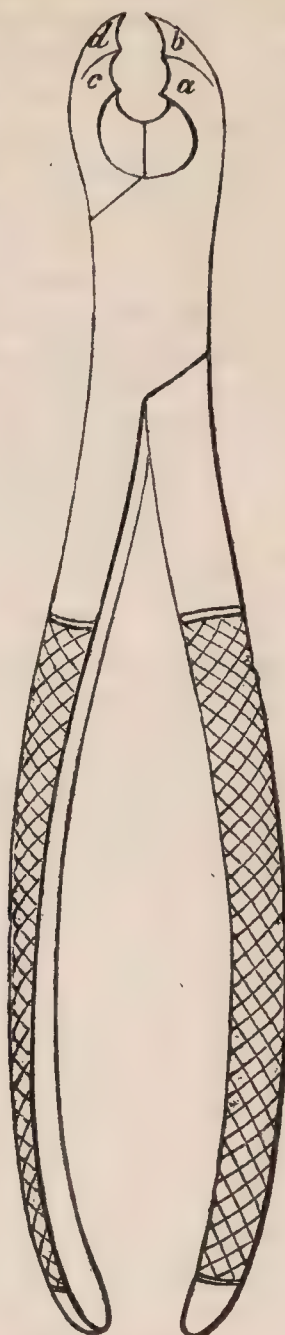


FIG. 4.

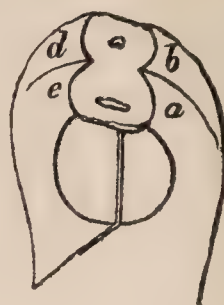


FIG. 3. Forceps for extracting molares of the right side of the inferior maxillæ.

a and *b*, anterior and posterior groove for the external surface of the neck of the tooth.

c and *d*, anterior and posterior groove for the reception of the convexities of the inner surface of the neck of the tooth.

FIG. 4. The same instrument as seen in fig. 3, but with a section through the neck of an inferior molaris from the right side in the grasp. The letters indicate the same parts as in fig. 3.

and of greater length than either of the others, and is situated opposite to the posterior external fang, and the space between that and the anterior external fang. The divergence of the fangs takes place at the point where the tooth becomes concealed in the alveolus, leaving the neck with a form such as would result from the agglutination of the fangs, having the described relative position. At this point the forceps should be applied for the removal of the tooth. Instruments, for it will require two, one for each side,

right and left, must be made upon the same general principles as those already described. The jaw for the external surface of the tooth must have two grooves—the anterior the larger, the posterior smaller, and upon a plane internal to the external groove. The jaw for the interior surface must have but one groove, and that fitted to the base of the internal fang. From the position of the molares of the superior maxillæ, the jaws of the instruments for their extraction must necessarily be bent at an angle with the handles. This angle should not be less than is absolutely necessary, for the more the instrument deviates from straightness the greater is the difficulty of using it. The angle in my own instruments is not less than 135 degrees, and I think they would be better were it even larger. The handles should have a general curve in the opposite direction to the jaws. The molares of the superior maxilla have the two external fangs parallel to each other in their direction in the alveoli. The internal, which is not only the largest but the longest also, diverges from the two preceding fangs, and passes upwards and inwards towards the internal wall of the antrum, and is inclosed in tolerably dense bone. The external alveoli are composed of thin and porous bone. In removing these teeth, then, the tooth being firmly grasped at its neck, the first motion should be slightly inwards to disengage the fangs from the external alveoli. The force should then be directed downwards and outwards in the direction of the internal fang. If these precautions be observed, no difficulty will be found in removing the superior molares, and the operation will be completed not only in less time but with less pain than would have arisen had the key instrument been used. The first and second molares of the superior maxilla are so nearly alike in size and shape, that an instrument well fitted to one will serve equally well for the removal of the other.

The third molaris or *dens sapientiæ* of the upper jaw, though the fangs are often united into one conical mass, yet the shape of the neck of the tooth is so like those of the preceding teeth, that an instrument which is suited for the removal of the anterior molares is often quite well adapted for the removal of the wisdom teeth. The *dentes sapientiæ* are,

however, sometimes much smaller than the other molares; in which case a smaller instrument might be required, but that, when of small size, they are for the most part removed by the application of so slight a force that any instrument, which can apply itself at all, will serve for their removal.

The molares of the inferior maxilla having two fangs, and these being situated, with respect to each other, directly anterior and posterior, give the neck of the tooth formed by their junction a central groove upon the external and internal surfaces; and as these are the surfaces to be grasped by the forceps, the jaws of the instrument must have a corresponding form. Of the fangs, the anterior is in each direction the larger, being both broader and thicker than the posterior fang. These teeth stand rather obliquely; while the external lateral surface, being larger than the internal, gives a line passing through the centre of the neck of the tooth from without inwards; a direction slightly backwards as well as inwards. From this conformation it becomes necessary to have forceps for each side, right and left. The jaw of the instrument to be applied to the inner side of the neck of the tooth must be rather posterior as well as smaller than that for the outer side; since the inner surface of the tooth is on a plane somewhat posterior to the outer surface. The jaws of the instrument must be bent at an angle of not less than 135. The handles straight, or nearly so. I saw a pair of forceps something upon this plan, except that one pair only was made, and that equally applicable for the extraction of molars of the lower jaw of either side; the grooves in the jaws being exactly opposite to each other, and of equal size. The jaws were also bent at a right angle with the handles.

The *dentes sapientiæ* of the inferior maxilla, if situated on a plane with the anterior molares, may be removed with the same instruments: for although the fangs are often united in a mass of conical shape, yet the grooves on the outer and inner surfaces are marked. These teeth are, however, not unfrequently situated in the angle formed by the junction of the lateral and ascending rami of the jaw; in which case the forceps for their extraction should be shaped at the edge so as to fit into the so-formed angle, and made

also with the jaw for the inner side of the tooth longer than that for the outer side, since in such cases the inner sides of the alveoli are on a lower level than the outer.

In removing molares of the lower jaw, the blades of the instrument should be carefully thrust down to the free edge of the alveoli, which part of the operation is easily effected, in consequence of the decreasing size of the teeth from the crown to the fangs; little more is needed, indeed, than, after placing the blades at the edge of the gum, to close the instrument. Having obtained firm hold of the neck of the tooth, the first motion should be inwards, by which the tooth is detached from the external plate of the alveoli: this being done, the tooth should be forced outwards and upwards, and so removed. The fangs of these teeth have, however, not unfrequently, a curve backwards; if, therefore, a tooth of this kind offers considerable resistance when its extraction is attempted, the movement, after the tooth has been forced laterally, should not be perpendicular, but in a curved direction similar to the inclination of the fangs.

Teeth are not exempt from irregularities of form, which, however, are principally found in the fangs; one fang is divided into two at its apex, or is bent at its termination; yet even these varieties do not materially affect the shape of the neck of the teeth, which part is so uniformly the same, that it is rare to find a tooth to the surface of which the described forceps will not apply themselves with tolerable and sufficient exactness. There are teeth, however, which deviate so much from the regular form, that no forceps, unless made for each particular tooth, would be well suited for its extraction, but that such teeth are generally small, and offer little resistance when their removal is attempted.

In writing on the subject of forceps for extracting teeth, I might have quoted more authorities and have given many extracts from works on dental surgery. It has not, however, been my purpose to write a history, but merely to describe certain forms of instruments which seem best adapted for general use.

The foregoing description will be rendered more intelligible by referring to the figures with the explanation, at

least as far as the forceps for the molares are concerned. I have not thought it necessary to give figures of the instruments for the removal of the front teeth.

The instruments themselves may be seen at Mr. Evrard's, of Charles Street, Middlesex Hospital, who has taken much pains and bestowed good workmanship on the making of forceps for extracting teeth.

41, Mortimer-street, Cavendish-sq.
May 19th, 1841.

OPERATION OF PUNCTURING THE MEMBRANA TYMPANI IN DEAFNESS.

To the Editor of the Medical Gazette.

SIR,

WILL you allow me to lay before the profession, through the medium of the MEDICAL GAZETTE, some interesting facts relative to the operation of puncturing the membrana tympani in deafness, and, at the same time, to state those which I consider to be the true indications for its performance.

It is well known that Sir Astley Cooper performed the operation about forty years ago in several cases with such success as to excite hopes of its becoming a grand agent in the restoration of hearing. He was led to propose it from observing that not unfrequently persons with loss of the membrane, congenital or morbid, retain the auditory function in tolerable integrity.

The only cases in which it was first considered applicable were obstruction of the Eustachian tube, and extravasation of blood into the tympanal cavity; but as there were then no means of diagnosing these conditions with certainty, and as the operation, when it came to be performed in a large number of cases, failed extensively, it soon fell into general desuetude.

At the present time obstructions of the tuba Eustachii, and extravasation within the tympanum, can generally be relieved by other means; and the best authorities, Kramer and Itard, recommend the operation only in cases of invincible occlusion of the Eustachian canal, or where there is thickening, insensibility, or cartilaginous hardness of the membrane. After bestowing much

attention on the subject in my own practice, I have ascertained that another important class of ear-affections may be relieved by the operation in question, and which have an advantage to the practitioner over those previously known, of being easy of recognition. It is not at all proved that in complete stricture of the Eustachian passage, the deafness can be relieved by opening the tympanum. Whether this condition be generally accompanied by other pathological changes, is uncertain; but I have performed it in cases where I had proved the tube to be impervious, by careful catheterism and the air-douche, without producing the slightest benefit, so that I feel a doubt if it is in such cases that relief has been obtained; and it must be remembered that Sir Astley Cooper, in his successful cases, had no means of diagnosing the state of the tube further than the sensations of his patients, which are uncertain even in the most intelligent.

The class of cases to which I refer as being those in which puncture or trephining of the tympanum may be performed with promise of success, is that in which pus has formed as a consequence of *otitis interna*, and escaped by the suppurative process through the membrana tympanum, the opening in which has subsequently healed, leaving the ear affected with deafness. On inquiry, loss of hearing will often be found to occur in this manner:—there is first ear-ache and distension of the tympanum, and, unless resolution should ensue, these symptoms are relieved by the discharge of pus, which generally terminates in three or four days, but may continue to be secreted, in a modified form, for weeks, or even years; and if the otorrhœa should cease, and the membrane close by cicatrization, deafness is sure to be caused, or aggravated if it has previously been induced by the disease. In these cases I conceive the deafness depends on the thickening and increased tension necessarily produced in the membrana tympani by cicatrization after loss of substance in it. Savart has shewn by experiment on the membrane prepared for the purpose, that when in a tense state it only vibrates to a limited extent; while when somewhat relaxed, as in the ordinary condition, sounds excited in its neighbourhood occasion vibrations sufficiently powerful to set

in motion sand or small seeds strewn on its surface. The conclusions of Savart have been confirmed by Professor Wheatstone.

Taking the physiology of hearing as a key, we might explain the benefit derived from puncturing the membrana tympani in such cases by supposing that the aperture exposed the membrane of the fenestra rotunda to the impulsion of sound, and that it does not act by restoring the vibratory function of the membrana tympani. There are certain facts which make this latter opinion improbable, and which, indeed, do away with the necessity of entertaining it, by proving that hearing may exist in the absence of the tympanic membrane. Many cases of aural disease have been recorded where there was entire loss of the membrane, with preservation of hearing. There is an interesting circumstance connected with perforation from disease, namely, that it is essential for the stapes to remain *in situ*, or else the auditory function is lost. This has been often observed, and is confirmed by some experiments of Cruickshank, who destroyed *seriatim* the membrana tympani, malleus, incus, and stapes; loss of hearing not ensuing till the destruction of the stapes. In such cases it is difficult to conceive how sonorous vibrations can reach the auditory nerve, unless it be through the medium of the fenestra rotunda. It cannot be held that the waves of sound affect the stapes after it has lost all connection with the membrana tympani, as this would contradict a law in acoustics, that sound excited in the air is not propagated from air to solid bodies with sufficient readiness to subserve the purposes of hearing. We know that bone is an excellent conductor of sound; but from the law stated, a watch cannot be heard through the medium of the teeth unless placed directly in contact with them. A recent writer appears to believe that sound may be transmitted from the air to the stapes; a “much greater influence being required when the impression is made on the stapes or on the fenestra ovalis to throw it into vibration, than when the bone or membrane is influenced by the natural means.” But in another passage he entirely negatives his opinion by stating that “the anatomical arrangement will not permit the

belief that the small bones can be influenced without vibration of the membrane," meaning the *membrana tympani*. The reason why hearing is so much affected by the absence or presence of the stapes is because the loss of the stapes from the connection of its base with the *membrana fenestræ ovalis* would occasion the escape of the fluid of the labyrinth, and thus destroy the function of the *membrana fenestræ rotundæ*.

The opinion was held long ago by Scarpa, that the air of the tympanum and the *fenestra rotunda* was one of the paths by which sound reached the labyrinth. This was opposed on various grounds, but is now adopted by many eminent physiologists. Mr. Tod believed that the use of the round membrane was to allow the motion of the labyrinthic fluid when the impulse was delivered to it by the ossicles and oval membrane. This hypothesis is supported by Prof. Todd. Mr. Tod asserted in corroboration that the ossicula were only present in animals possessed of a cochlea and *fenestra rotunda*; but comparative anatomy shows that frogs have a perfect tympanic cavity, yet want the *fenestra rotunda*: the chain of auditory bones being in them the only means of conducting sound to the labyrinth. On a review of the whole question, I believe the opinion of Scarpa the most correct. Müller has shown, by direct experiment, that though sound is not readily communicated from air to water, yet the communication is easily effected if the air and water are divided by an animal membrane, in the manner the membrane of the *fenestra rotunda* separates the air of the tympanic cavity from the fluid of the labyrinth; so that there seems to be no fact of auditory acoustics opposed to the view of Scarpa, that sound reaches the sensorium by the air of the tympanum and the *fenestræ rotundæ*; in addition to the other path through the ossicula auditus; and, when these bones are destroyed, hearing may still take place by the sound *fenestra* and its membrane.

I am aware that the question has been agitated, whether or not the loss of substance of the *membrana tympani* can be reproduced. It is difficult to ascertain whether the new growth be of similar structure to the original membrane, or factitious, but of this I am

certain, that ulcers extending through its whole substance do heal, so as to restore its continuity. I have seen cases proving this, where patients for years had been able to force air through the tympanum, and then lost the power, after which the tympanic membrane could be seen through a speculum quite entire. I have, even in some cases where a circular piece of membrane was removed artificially, seen the aperture completely close in a fortnight after the operation.

Sir Astley Cooper used a pointed probe, but various improvements in instruments have since been introduced. A stilet, the square perforator of Buchanan, the round punch of Himly, have each been recommended. Sig. Fabrizzi constructed a very beautiful instrument on the principle of the trephine, but with a cutting edge. It is a modification of this instrument on the side of simplicity and celerity I have used for some time, applying the tympanatoire to the inferior quarter of the membrane, anteriorly to the handle of the malleus. Previous to the removal of a circular portion, I invariably puncture the membrane with a cataract needle, which produces no ill effect, and shows beforehand the amount of benefit to be derived from the major operation.

Up to the present time I have trephined the *membrana tympani* in about thirty cases. Some of these have been already published. The day on which I write, a patient called on me for whom I performed the operation two years ago. His object in calling was to assure me of his continued acuteness of hearing. I stated, at the commencement, that nearly all who have had previous discharge, with an open state of the tympanum, followed by suppression and closure, may be relieved of deafness by the tympanatoire; not that I have seen any exceptions, but because the operation cannot be supposed to act as a remedy in cases where there has been disorganization of the ossicles accompanied by the escape of the aquula of the labyrinth, it being absolutely necessary to hearing that the labyrinthic fluid and its membranes shall remain intact.

After the performance of the operation, I have thought it advisable to seclude the patient from sound as much as possible. Slight inflammation has

sometimes ensued, but never to a serious extent.

Apologizing for having extended my remarks to such a length, I remain,

Your very obedient servant,

JAMES YEARSLEY.

29, Sackville Street, May 17, 1841.

A CASE OF LUSCITAS SPASTICA, WITH OBSERVATIONS.

To the Editor of the Medical Gazette.

SIR,

As the following case is one of rare occurrence, and presents some interesting peculiarities, I shall feel greatly obliged by your giving it insertion in your valuable journal, and remain, sir,

Your obedient servant,

AUG. FRANZ, M.D. Leipsic,
M.R.C.S. &c.

19, Golden Square,
May 25th, 1841.

Mrs. W., aged 42, of robust and plethoric habit, has, during several years, been subject to rheumatism. In the beginning of January last she suffered from severe cold in the head, with toothache, and acute lancinating pain over the left half of the face, attended with some degree of fever. With a little care, and the use of some domestic medicines, the cold abated and the febrile symptoms subsided; the face-ache, however, although to a certain extent diminished, was confined to the left orbit, and assumed a remittent character. It appeared principally towards evening, lasted during a part of the night, and became more intense with bad weather. At each of the pains a slight inversion of the left eye was observed, which became in time more decided and more constant. The eye was now no longer straight in the morning and during the absence of the paroxysms, as was the case at the commencement of the complaint, but remained permanently inverted. With this inversion the necessary concomitant of double vision now made its appearance; although slight in the beginning, it increased in the same ratio as the inversion, and was at last so annoying that the patient was obliged to blind one eye or the other in order to follow her pursuits of reading, needlework, &c. When the right eye was closed, the left or squinting eye assumed always its proper position

in the orbit; one morning, however, after the patient had been out to a late hour the night before, she found, on binding up the right eye, that she could no longer direct the left eye towards a book she wished to read, but that the cornea was completely fixed in the inner canthus. The print appeared to her very indistinct, and the inner eyelids, more especially the upper, felt stiff, and its movements were limited. The patient being much alarmed by this circumstance, called upon me a few days afterwards for advice.

On the 8th of March, when I for the first time saw her, nothing abnormal could be distinguished in the right eyeball, neither in its position nor movements; the iris even responded properly to different degrees of light. The left eye, on the contrary, was permanently fixed in an inverted position, so that one quarter of the cornea, which was not only directed inwards, but also a little upwards, was hidden by the inner canthus and the upper lid. When the right eye was closed the left could not be brought into a proper position, but remained immoveably inverted. The pupil, although somewhat shaded by the inner canthus, was diminished in size, and did not answer to different degrees of light, nor contract even when a strong light was concentrated into it by means of a lens. On opening the eyes, the aperture between the lids was of the same extent in both eyes; but the upper lid of the left eye could only with difficulty be moved downwards, and then did not entirely cover the globe: it could, however, be brought in contact with the lower lid when drawn down with the fingers. In the inner canthus the conjunctiva was a little reddened, and the sclerotica presented a fine vascularity; the eyeball felt compressed, and as if pushed forwards, and in the orbit was perceptible a sensation of fulness and tension, with an occasional pricking and lancinating pain. There was no intolerance of light. On closing the right eye, remote objects could not be distinguished, and those near her were seen very indistinctly and faintly. When both eyes were open, the double vision was well marked.

From the history of the case, and the appearance of the affected eye, I was induced to believe that the complaint

was a spasmodic affection of some of the muscles of the eye and upper lid, dependent on a rheumatic or hysteric cause, and consequently ordered the following treatment to be pursued:—Eight leeches to the temple; linseed poultice, with Herb. Hyoscyami, to the eye; a warm bath on two succeeding days; after which perspiration was promoted by means of Pulv. Ipecac. comp.; the bowels were kept gently open. On the third day the affected eye could already be moved from the inner canthus towards the centre of the orbit, when the other was closed, but returned to its inverted position on the cessation of the effort of will. Emplast. Cantharidis was then applied behind the left ear; the eye was fomented with Infus. Anthemides et Hyoscyami; Ung. Hydr. with a little Extr. Belladonnæ rubbed into the lids; irritating foot-baths at bedtime, and Tinct. Colchici, which acted freely on the bowels, were ordered, and the left eye principally to be used. By these means the inversion and other symptoms were gradually lessened, and disappeared entirely a fortnight after the commencement of this treatment; so that at this time the eyeball had its proper position in the orbit, enjoyed freedom of motion in all directions, could be completely covered by the upper lid, and the pupil was of its proper dimensions, expanding and contracting regularly: in fact, no difference existed between both the eyes. As the sclerotica was yet somewhat vascular, and a slight sensation of tension was felt in the orbit on moving the eye, the use of Tinct. Colch. and the ointment was continued for a few days, and the patient advised never to wash the face with cold water. Under this treatment the affection soon vanished, and the eye has ever since been perfect in its position, movements, and function of vision, as the iris also in its actions.

The accomplishment of a cure under this mode of treatment in so short a space of time, proves that the permanent inversion or *luscitas* in this case was not the effect of a disease of the brain, nor of paralysis, or any other affection of the musculus rectus externus, nor of any adventitious growth in the orbit, but was of a spasmodic nature; at first remitting but subsequently permanent, of which rheumatism was

the proximate cause. The spasmodic affection extended to the rectus internus, probably to the rectus superior and inferior, and also to the obliquus inferior and levator palpebræ superioris, as is evident from the direction of the cornea inwards and a little upwards, and from the limited motion of the upper lid downwards. The affected muscles being only those supplied by the third pair of nerves or motores oculorum, it can hardly be doubted that the rheumatism, which at first affected the left half of the face, was in time restricted to the sheaths of the left oculo motorius exclusively. It seems moreover probable, that not only the muscular branches of this nerve were affected by rheumatism, but also the short thick branch given off to the ciliary ganglion, and that thus was occasioned the permanent spasmodic contraction of the pupillary margin of the iris, and consequently the diminished size of the pupil.

In eyes affected with inversion I have sometimes noticed the pupil to be contracted, but at other times, and indeed far more frequently, dilated. The interesting question now presents itself—how far this difference in the state of the pupil may be considered as a diagnostic sign, as regards the primary cause of the inversion? From the above case, and from some other observations I have made in this respect, it appears to me that a contracted pupil in an inverted eye would indicate that the original cause of the inversion was a spasmodic state of one or more of the muscles; for if this state continues some time, it will make the temporary contraction of the pupil and muscles of the permanent eye. Professor Müller*, on the other hand, says, “There is a certain degree of paralysis of the muscles with contraction, or also a contraction with atrophy.” In club-foot, the gastrocnemius sometimes presents either the one or other of these conditions. Now, if the inversion of an eye has been caused by such conditions of the muscle, I think we may fairly expect the state of the pupil to be that of dilatation. The pupil may, however, be dilated by being much shaded by the inner canthus, when the eye is inverted to a great extent, without any reference to the existing muscular defect.

* Physiology, vol. ii. p. 82, German edition.

ANALYSES AND NOTICES OF BOOKS.

“L'Auteur se tue à alonger ce que le lecteur se tue à abréger.”—D'ALEMBERT.

On the Nature and Treatment of Stomach and Urinary Diseases, being an Inquiry into the connexion of Diabetes, Calculus, and other Affections of the Kidneys and Bladder, with Indigestion. By WILLIAM PROUT, M.D. F.R.S. 3d edit. London, Churchill, 1840.

MANY of the doctrines contained in this work will certainly be embodied in the practice of English medical men during the next half century. Instead, therefore, of filling the brief space which we can allot to a review, with any comments of our own, we will present our readers with an outline of the plan which Dr. Prout pursues in tracing the connexion of urinary diseases with indigestion; although this is a title which by no means does full justice to the scope of the work.

In the introduction Dr. Prout gives a short sketch of his opinions on the nature of some of the minuter operations of the animal economy, which he has already made known to the world in his Bridgewater Treatise, and of which the present work exhibits the practical application. He alludes first to the constitution of organised bodies in general, which he shews to be composed of two classes of materials. The first class, comprising hydrogen, carbon, oxygen, may be called *essential elements*; because it is of these that the bulk of all organized matter essentially consists. The other class, which Dr. Prout terms *incidental elements*, comprises a number of principles, such as sulphur, phosphorus, iron, &c. which exist in very minute quantity, but yet exercise a most important influence on the properties of the organized matter in which they are contained. It is to the presence of these incidental elements that the distinctive peculiarities of organized matter are to be attributed; that different kinds of meat, for instance, though alike composed of oily and albuminous matter, yet vary so remarkably in their sensible qualities, and in their effects on the constitution.

In the next place, Dr. Prout proceeds to consider organized matter as aliment; and observes that alimentary

principles may be divided into four great classes; namely, the *aqueous*; the *saccharine*, (including sugar, starch, and vinegar); the *albuminous*; and the *oleaginous*. These are the “four principles by which the higher animals are nourished, and of which their bodies are essentially constituted.” Hence it follows, that a diet to be complete should contain more or less of all four;—a doctrine which is singularly illustrated by the composition of milk; which, being the only material prepared by nature expressly as food, might be expected to exhibit a model of what food ought to be. And accordingly we find that the milk of all animals contains, besides water, a saccharine, an albuminous, and an oleaginous principle.

Having dismissed this subject, he passes in natural order to the process of assimilation, under which term he includes “every process directly or indirectly concerned in the assimilation of alimentary matters into the textures of a living animal body.” And these processes he divides into two classes—the primary and the secondary, of which “the primary assimilating processes comprise the process of digestion, and all the intermediate processes of sanguification inclusive; whilst the secondary assimilating processes comprise those by which the textures of the living body are first formed from the blood, and afterwards re-dissolved and removed from the system.”

The primary assimilating processes must be of at least three kinds. In the first place, the food must be dissolved, and be made to unite with alimentary water—a process styled *reduction*. When an organized substance contains a small quantity of combined water, it is said to be *strong* or *high*; when it contains a larger proportion it is said to be *reduced*, or *weak* or *low*. Thus we read of strong and weak sugars, glues, &c. The reducing operation of the stomach is well exemplified in the conversion of the strong firm albumen of egg or meat to the weak low albumen of the chyle, “the coagulation of which is imperfect, and so wanting in tenacity as to offer a striking contrast with the coagulated albumen of the egg.” Secondly, since the chyle is always the same in its composition, the stomach must possess a *converting power*, by which it can change the simple alimentary principles into each other. Thirdly, there must be an

organizing or *vitalizing* power, by which the crude aliment may become fit to be amalgamated with the living body.

Now it is probable that either of these three processes may be separately deranged. The dissolving or reducing power may be feeble—a defect sufficiently obvious in common indigestion; but, on the other hand, this process may be morbidly active, as in diabetes, in which complaint farinaceous (and perhaps other) matters are reduced to the state of a *low* sugar.

Again, there may be a deficiency of the converting function, of which function the conversion of saccharine into albuminous and oleaginous matters appears to be the most important operation. This defect we also see exemplified in diabetes, in which the stomach is incapable, as in health, of converting vegetable aliment into the elements of chyle or blood. “Moreover, the converting process may be wrongly performed; the saccharine matter, for instance, instead of being converted into chyle, may be converted into oxalic, lactic, and other acid and deleterious matters.

“But even supposing the reducing and converting functions of the stomach to be performed, the third, or *vitalizing* function, may, in some instances, be suspended, or otherwise deranged.” Thus, when too much food is taken, although it may be *dissolved* and *converted*, “yet the *vitalizing* function is withheld, and the superfluous matter eliminated with the bile, or in the form of lithate of ammonia in the urine.” In other cases the imperfectly vitalized chyle may be converted into the tubercle of struma, or the chalkstone of gout.

The secondary assimilating processes are of two kinds—the formative and the destructive. By the first, the tissues are formed out of the blood; by the second, they are, when effete, resolved into it again; and they may be “either converted into principles of a higher order designed for ulterior purposes, or into effete and crystallizable matters designed to be excreted from the system.”

Dr. Prout believes that the *gelatinous tissues*, when effete, are “converted into two classes of principles, of which urea or its equivalent constitutes one principle; and the saccharine principle, in the form of lactic acid, the other:” and these escape by the kidneys and skin.

But both the formative and the destructive assimilation of these tissues may be deranged; and various unnatural compounds, such as sugar, oxalic acid, and carbonate of ammonia, may be produced. Thus, when sugar or oxalic acid is present in the urine, there is frequently visible disorganization of the gelatinous tissues, in the form of carbuncles, cutaneous diseases, &c.” And thus “diabetes very frequently (as far as my personal observation goes, says Dr. Prout, *always*) accompanies carbuncles, and malignant boils or abscesses allied to carbuncles. This is a fact mentioned by several of the older writers, and is of great importance to surgeons.”

With regard to the *albuminous tissues*, Dr. Prout believes that, when effete, they are converted by the secondary destructive assimilating processes into lithic acid, in the form of lithate of ammonia, which is excreted in the urine; and in the same manner he attributes the lithate of ammonia, which is deposited from the urine in ordinary indigestion, to the decomposition of the imperfect albuminous matters during the primary assimilating processes. “But, again, the lithate of ammonia appears in the urine, also when no food has been taken into the stomach, and when therefore its formation can only be referred to secondary mal-assimilation of the albuminous contents of the blood, or albuminous tissues; as, for instance, in certain fevers, and other severe constitutional diseases.”

Having thus spoken of the assimilating processes, and given a general idea of their arrangements, and having next treated of the composition and properties of the blood; of the functions of the liver; and of the relations of the bile to the assimilating processes; of the composition of the bile, and of biliary concretions; of the functions of the kidneys, and of the relations of the urine to the assimilating processes; of the composition of the urine and of urinary calculi; he enters on the grand purpose of his work, which is, the exposition of the *diseases of assimilation and of secretion*.

“We shall now attempt to shew,” he observes, “that the processes by which alimentary matters are assimilated in animal bodies, are constantly liable to be deranged, both in degree and in kind; that such derangements

of the assimilating processes are manifested by corresponding derangements in the various products secreted from animal bodies by the organs designed for such purposes, and especially by the liver and kidneys; and, lastly, that the relations existing between the deranged products of secretion, and the proximate principles by which animals are maintained, and of which their bodies consist, enable us, in many instances, to acquire a more correct knowledge of the organ deranged, as well as of the nature of its derangements, than can be obtained by any other means."

"We divided the proximate alimentary and staminal principles of animals into four great classes; viz. the aqueous, the saccharine, the albuminous, and the oleaginous classes; and, in conformity with this arrangement, we shall consider the physiology of the assimilating processes under four similar heads."

Under the first of these is discussed the pathology of aqueous assimilation and secretion, and of excess or deficiency of urine.

In the second chapter we find the pathology of saccharine assimilation and secretion; of *diabetes*; and of diseases connected with the unnatural development of the *oxalic* and *lactic* acids.

With regard to diabetes and saccharine urine, their essential nature may probably be thus stated. The *reducing function* of the stomach is too active, so that the food is, in a great measure, brought into the form of a low sugar. The converting function is defective, so that this low sugar passes unchanged into the blood, and the greater part of it, instead of being assimilated, is separated by the kidneys in a crystallizable form. And not only these primary assimilating processes are deranged, but the secondary also; that is to say, the gelatinous tissues, when absorbed again into the blood, are also mal-assimilated, and converted into some form of saccharine matter.

From these views of the nature of diabetes, many important inferences may be drawn concerning its treatment. The diet should, as a general rule, consist of *high* or *strong* aliment, not too easy of *reduction*; animal food, twice a day, with bread, and any simple articles that can be prepared from flour,

milk, eggs, &c. to the exclusion of *low* farinaceous matter, (such as potatoes, arrow root, &c.), and most especially of every form of crystallizable sugar, and of fruits containing it. Porter, from its containing no undecomposed saccharine matter, is to be preferred to ale. Much comfort and relief from the constant craving and thirst may be obtained by using oleaginous matters, such as butter and fat bacon, in considerable quantity. Further, Dr. Prout believes that, "from the chemical properties and analogies of gum, it is probable that it is not converted (at least so readily) into sugar or oxalic acid as farinaceous matters." He is not yet able to state the effects of it as an aliment in diabetes, but throws out the hint as well worthy of being pursued.

Whilst speaking of the complication of diabetes with hepatic disorder, Dr. Prout makes a few judicious observations on the employment of mercury in general.

"The extraordinary and immediate benefit often afforded by mercury in common affections of the assimilating organs, is one of the great sources of its abuse. Medical men know they can produce by its means a certain off-hand effect; and to save themselves trouble, and, at the same time, to gain the doubtful reputation of being decisive and quick in their practice, they resort to mercury without due regard to its remote consequences. Twenty or thirty years ago, this abuse of mercury was carried to a much greater extent than it is at present. A calomel pill at night, and a black dose in the morning, were the panacea for all diseases in all kinds of constitutions; from the congested liver of the overgorged alderman, to the torpid liver of the weak and indolent female."

In proceeding to particularize the ill effects of this kind of practice, he observes—

"Patients who habitually take calomel are more than ordinarily subject to periodical congestions, or *biliary attacks*, as they are termed; and such biliary attacks will rarely yield to any other remedy than calomel. Nor is the insensibility to gentler expedients, thus too often produced in the soundest constitutions by the use of mercury, its only fault; the habitual use of this remedy is capable of exciting positive mischief on the assimilating functions,

and on the kidneys of some individuals, as will be shewn in a subsequent part of this volume. Moreover, those who are under the influence of mercury, in a degree far short of salivation, are notoriously liable to take cold, rheumatism, &c. from slight exposure; and various formidable and fatal diseases, as phthisis, &c. can often be distinctly traced to such exposure under the influence of mercury." "I am prepared for the objection that many individuals begin the use of mercury early, and continue it with the same evident advantage to extreme old age; and I meet this objection by the statement, that many individuals begin the free use of wine and spirits at an early age, and continue to use them to extreme old age; but no one, forgetting the millions that have in the meantime fallen victims to the abuse of fermented liquors, will contend that such ought to become the rule; and the same remark is strictly applicable to the abuse of mercury."

The presence of oxalic acid in the system is to be attributed sometimes, although rarely, to the use of vegetables containing it, as articles of diet—rhubarb stalks and sorrel, for instance; but of course, as a general rule, it can only be ascribed to a derangement of the converting power of the stomach, whereby saccharine matter is converted into oxalic acid; or to a mal-assimilation of the gelatinous tissues. The observations of Dr. Prout concerning the prevalence of the oxalic diathesis during the Asiatic cholera in 1832, and during the late influenzas, are highly interesting.

Another acid, formed through mal-assimilation of saccharine matter, is the lactic; "the development of which, in excepting the primary organs of assimilation, constitutes one of the most troublesome forms of dyspepsia; whilst its development, during certain forms of secondary mal-assimilation, is supposed to give occasion to many serious and most painful affections; of which rheumatic and neuralgic affections are the most remarkable." We wish that we could extract some of the practical observations on common indigestion, bilious attacks, nervous headache, the operation of malaria, and the abuse of alkalis, which Dr. Prout has embodied in this section.

The third chapter is devoted to the

pathology of albuminous assimilation and secretion; and the first section of it to derangements, accompanied with excess or deficiency of urea in the urine; a subject very little understood before the publication of the first edition of this work in 1821.

The second section treats of albuminous urine in the two forms of *chylous* and *serous*. "The presence of the albuminous matter of chyle in the urine he supposes to depend, partly on some imperfection in the assimilating process, by which the chyle is not raised to the blood standard; and partly on an imperfection in the function of the kidney, by which such imperfectly assimilated chyle is not, as in the healthy condition of the kidney, converted into the lithate of ammonia." The serous, or what is commonly known by the term *albuminous*, urine is then elaborately treated of, together with that degenerated condition of the kidneys which has been illustrated by the labours of Bright, Rayer, and Christison.

The two next sections treat of the lithic acid, and of that rare kind of urinary deposit termed cystic oxyde. "With regard to the general pathological relations of lithic acid and its compounds, we may remark, that as the lactic acid, developed principally during the secondary mal-assimilation of the gelatinous tissues, was considered as the characteristic feature in rheumatism; so the lithic acid, developed principally during the mal-assimilation of the albuminous textures, may be considered as the characteristic feature in gout. Moreover, when the lactic and lithic acids are developed together, as they may be, and often are, the phenomenon may be supposed to show that the mal-assimilation involves both the gelatinous and albuminous textures; and that the accompanying disease partakes of a mixed character, or, in fact, constitutes what is not improperly termed *rheumatic gout*; a form of disease which every one knows is usually of a more deep-seated and obstinate character than either gout or rheumatism alone."

The following observation respecting alkalis is worth notice. "By giving alkalis in combination with tonics, the good effects of both are often lost; but their separate use is often highly beneficial. Thus I often give tonics (even

the mineral acids) before and between meals, at the same time that alkalis are given after meals; and with the best effect."

In the next chapter is considered the pathology of oleaginous assimilation and secretion; of obesity and leanness; and of concretions of cholesterine, or gall-stones. We cannot help quoting Dr. Prout's caution to the lovers of the *pâtés de foie gras*. He thinks it probable that the wretched geese sometimes have their revenge. "Indolent and dyspeptic individuals, who partake of these diseased and poisonous productions, can scarcely be supposed, in all instances, to assimilate them; and consequently run considerable risk in inoculating and converting their own livers, or other organs, into a mass of disease." Nor can we omit the following query. Having noticed the large proportion of fat, and the existence of phosphorus in the nervous and cerebral tissues, he says, "As the quantity of fatty matter in animals seems to bear an inverse relation to the quantity of bodily and mental, *i. e.* of *nervous* activity; can the leanness proverbially produced by nervous activity be referred to the more rapid consumption, and consequent renovation, from the oleaginous principle, of the general mass of nervous matter? Farther, is not this extra consumption of the nervous matter attended by the presence of a larger proportion of phosphorus in the urine? And cannot the occasional appearance of a large proportion of the earthy phosphates in the urine be referred to the same operations?"

The next chapter is devoted to the pathology of the incidental principles; of the phosphorus, lime, and magnesia, and of the urinary deposits formed from them.

The remainder of the work discusses, in due order, the subjects of calculi in the kidneys and ureters, and the diseases likely to be confounded with them; of calculi in the bladder, and of diseases of the bladder; of hæmorrhage from the urinary organs; the incontinence, retention, and suppression of urine; of the effects of solvents on stone in the bladder; and of lithotrity and lithotomy; and we must not omit to mention, that the whole is illustrated with statistical tables, and with plates of urinary deposits and calculi.

We would gladly extend our analysis, did our limits permit. But we believe that we have enabled the reader to comprehend the philosophical plan on which the work is constructed; and the length to which we have already gone will show sufficiently our sense of its value. In fact, we can honestly say, that there is not one element of pleasure or of profit that can be derived from any medical work, that may not be found in this. The practical man will meet with original observations on the effects of remedies, that bear the stamp of truth and experience, and with clear directions for the discrimination and treatment of disease; and not of disease merely, but of those states of imperfect health, the regulation of which will be confessed by all, who know their profession practically, to be quite as difficult and important as the cure of actual disease. And, lastly, respecting the physiological views embodied in this work, time may or may not show them to be entirely true; but we believe, with their author, that they are neither useless nor groundless; and that they furnish an insight into the more recondite operations of the animal economy, as well as many indications for the treatment of disease that are as yet attainable by no other means.

MEDICAL GAZETTE.

Friday, June 4, 1841.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

MEDICAL EDUCATION.

THAT which more than any other part of the present system of the Medical Schools obstructs every real improvement in medical education—which stands in the way of every attempt to make good use of the annual hospital examinations, and which precludes the possibility of a careful system of constant, personal, and individual instruction of the students—is, as we have often remarked, the multiplicity and

difficulty of the subjects which are put before the medical pupil during his first winter's studies in London. They are such as no student possessed of any short of the most extraordinary talents can master, and such as the great majority utterly fail in their endeavour to become acquainted with.

If any person, of good natural talent, well-cultivated, and knowing the extent and the difficulties of the study of sciences in these days, were told that (being previously altogether unacquainted with them) he must in the next seven months learn a considerable amount of Anatomy and Physiology, both practical and theoretical,—of Chemistry in all its wide-spreading branches,—of the *Materia Medica* with Pharmacy and Therapeutics,—and of the Theory and Practice of Surgery, he would surely start at the exorbitance of the demand, and confess his incompetency to accomplish one half of such a task. He might, indeed, undertake to acquire a smattering of each—a vague notion of their general principles and their most important facts; but for the attainment of a sound practical acquaintance with them all, and of a knowledge of them which should enable him to bring in the applications of each to the practice of a profession—for this, we repeat, no man of good judgment, or acquainted with his own power of acquiring knowledge, would ever profess himself competent. And yet all this is actually put before medical students, the majority of whom, when they arrive in London, have never fundamentally learned a single science, and have had their minds no further cultivated than in the ordinary routine of a provincial school, and have lost much even of the good that that might have given, in the listless and unprofitable practice of dispensing.

Now, it may be said, such a scheme, though defective, can do no harm; to ask

of a man more than he can accomplish may make him diligent—may induce him to do his best, and may keep him from idleness. But even for this the present scheme is inefficient. It is notorious that the majority of students in their first season do not so much as attempt to learn all that is set before them. The idlest and most modest, in despair of accomplishing any thing, give up every thing; the wisest and most industrious neglect two or three of their subjects, and profitably study the remainder; the most confident and most unwise attempting them all, succeed in none, and at the end of the season are scarcely more advanced in knowledge than those who have altogether neglected their studies. So that this excess of demand does not even engender industry, and in all who attempt to comply with it produces a result of ignorance.

For the truth of what we assert we would refer those who are dissatisfied with our authority, to any one having a general practical knowledge of the condition of medical students as to their acquirements in the several branches of their studies: not, indeed, to any one lecturer, for he, it is probable, will not know any thing further than the state of his own class; but to one who has a general knowledge of the results of education, and is acquainted with the state in which students usually present themselves for examination. Or, if this be not sufficient, then let them look to the circumstances in which students contend at hospital examinations; and they will find that, although the standard set for the attainment of prizes does not involve more knowledge of a subject than each student ought, for the public safety, to possess, yet the prizemen are almost invariably those who have neglected some of their subjects of study that they might devote themselves exclusively to those in which they have succeeded. Or again

(and this is perhaps the best evidence of all), let them learn the proportion of students (even the more industrious among them) who, having in the regular course of their studies made themselves acquainted with the most important and practically useful parts of their profession, go to *grinders* to be provided for a few days with the little knowledge of the rest that is requisite for passing their examinations. From this, as plainly as from all the rest, undeniable proofs will be obtained that the curriculum of medical education is greatly overcharged; and that, in the first winter session more especially, the amount of knowledge required to be learnt is greater than can be reasonably demanded of any one whose talents or intellectual cultivation are not much higher than those of the great majority of medical students.

The evils of this error branch out very widely; they are by no means confined to the loss of the student's time, or to the idleness to which, disgusted with the difficulty of his pursuits, he too often resorts; the whole machinery of medical education is put out of order by their existence. Each lecturer is, for his own part, bound to endeavour to make his pupils learn the subject that he teaches, and, regardless of the rest, must recommend it to their especial attention. Thus his authority comes in confirmation of the error; and too often the least useful subjects are studied to the neglect of the most important. Or, if this be not the case, the lecturer, finding that some of his pupils advance well in his subject, while others, engaged perhaps with something else, gain no knowledge of it, cannot wait for these last, but must leave them in their ignorance to study at some future time, in any way they may be able. Thus the regular practice of a systematic personal and individual instruction by examinations, without

which medical education will always lamentably fail of its object, is wholly prevented; and what should be teaching degenerates into an useless and unfruitful system of lecturing and listening.

Now if it be granted that, in the study of medicine, no knowledge which is not sound and capable of being applied in practice is worth attaining (and of this we suppose there is no doubt), it is obvious that the system which necessarily involves the imperfect learning of one or of all the subjects appointed to be studied, must be injurious. That the present system does this is certain; and it is therefore the plain duty of those who guide the schemes of education to contemplate the propriety of altering it, either by spreading the subjects of study over a wider space of time, or by diminishing their number.

In looking through the list of lectures to be attended in the first winter session, with a view to determine which of them may best be spared, the decision at once falls on those on chemistry. We have never been advocates for a low standard of general knowledge in medical practitioners, but surely custom more than sound sense has led to the retaining of this science, considering what it has now become, in the list of ordinary medical studies. What are the relations between the almost boundless investigations of modern chemistry and the practice of medicine and surgery? They are really very few, and are no more than are, or ought to be, taught in the lectures on *materia medica*. No doubt, some half century or more ago, chemistry and medicine were much more intimately connected. But of late years the two sciences have widely diverged; medicine has become much less a matter of physic, and chemistry has almost entirely given up drugs and remedies of disease, and has betaken itself to heat and light, gal-

vanism and magnetism, and a hundred other things which have really no greater connection with the practice of medicine than have astronomy and geology. So complete is the separation of chemistry from medicine, that the former, which used to be taught by physicians only in the medical schools, has passed into the hands of professed lecturers, who know no more of medicine than the average of practitioners do of modern chemistry.

The education in a medical school ought, we maintain, to be exclusively professional; the knowledge of such subjects as are preparatory to medical study, and that cultivation of the intellect by the study of some of the abstract and exact sciences, which alone render the pupil capable of appreciating and duly learning others, should be brought to the medical school—should be obtained, if at all, before the attendance on medical lectures is commenced. It is too much to expect of any man that he will both learn to study, and will study medicine in the same year; and if for the intellect's sake chemistry must be studied by every medical pupil, he should come to his professional pursuits ready prepared with it: he should learn it with his Latin and Greek, and with his mathematics. In a word, it should form part of general not of medical education.

We hold this the more confidently, because if chemistry be entirely removed from the list of school-studies, all its useful and practical applications will still be taught in the course on *materia medica*, one half or more of which is in fact composed of lectures on medical chemistry. We refer to the lectures by Dr. Pereira, which we published some years since, and confidently ask whether any more chemical knowledge than is contained in them is essential to a sound and safe practitioner. If it be

said that much of physiology consists of applications of chemical science, we grant it, but reply, that the object of an ordinary medical education is to make—not learned physiologists, but good practitioners, and that this can be accomplished without a knowledge of physiological chemistry, but not without a steady and energetic application of the mind to practical subjects for at least three years. Or if, again, it be urged, that in prescribing, and other parts of practice, chemical laws are daily to be regarded, we answer, that all that relates to these things is taught in lectures on *materia medica*; and that if it were not, they may be learned as mere rules of routine, and are in fact only so known by the majority of those whose practice is most successful, and whom it would be happy if medical students could be made to equal in their skill.

ROYAL MEDICAL & CHIRURGICAL SOCIETY.

Tuesday, May 25, 1841.

THE PRESIDENT IN THE CHAIR.

A Case of Subclavio-axillary Aneurism, successfully treated by Operation. By F. C. SKEY, Esq. F.R.S. Assistant-Surgeon to St. Bartholomew's Hospital.

THE aneurismal tumor in the case here related was of small size, and was situated at about an inch from the outer border of the left scalenus muscle. It had existed about two months, when the patient put himself under the care of the author; and, as it was rapidly advancing, the operation was immediately determined on. The mode of performing the operation is thus described:—“I commenced an arched incision about three inches above the clavicle, close to the outer border of the sterno-mastoid muscle, and carried a little outwards, curving it in towards the clavicular origin of the muscle, which I exposed to somewhat more than one-half of its length. This flap, convex towards the acromion, I reflected with the platysma muscle. A little careful dissection with a blunt silver knife exposed the lower

belly of the omo-hyoideus, and a portion of the sac, through the walls of which the pulsations of the artery were visible. On the inner side the external border of the scalenus was also exposed; and by tearing away the cellular tissue in this space by means of a blunt hook and a silver knife, the transversalis colli and supra-scapular arteries were brought into view, arising from the thyroid axis within the scalenus muscle, and proceeding outwards across the bottom of the wound to their destination. About the transversalis colli was felt the subclavian artery, and above it the lower branches of the axillary plexus of nerves. Having slightly detached it from the rib, I had no difficulty in passing around it an armed needle, at a quarter of an inch on the outer side of the scalenus."

The progress of the case was tolerably favourable until the 17th day after the operation, when appearances manifested themselves in the left leg and thigh which the author attributed to phlebitis. The treatment of this symptom consisted in excoriating the surface over the affected vessels with water nearly boiling, and anointing the excoriated surface with Ung. Hydrag. mixed with Opium. The ligature separated on the 47th day after the operation.

The author declares his decided preference to the mode of incision adopted in this case over the more usual one along the line of the clavicle, on account of the greater facility of approaching the vessel to be tied, as well as the greater probability of escaping troublesome if not dangerous hæmorrhage.

The paper concludes with observations on the severe constitutional symptoms which frequently follow the ligature of large vessels, and which, in the case now related, were particularly urgent.

Case of Aneurism of the Right Subclavian Artery, in which a ligature was successfully applied. By JOHN P. HALTON, Esq. Surgeon to the Liverpool Infirmary. [Communicated by Sir B. C. Brodie, Bart.]

The patient was a warehouseman, æt. thirty-five, of robust frame, and was admitted into the infirmary in December last, with a strongly pulsating aneurismal tumor immediately below the clavicle, raising that bone considerably from its natural position. The disease had its origin in an accident which occurred three months previously to his admission, when having fallen from a pile of cotton, his fall was suddenly arrested by a hook, which suspended him by his arm. Notwithstanding the appearance of the swelling three weeks after the accident, and the great suffering which attended its rapid increase, he continued his laborious employment until the 24th of December, and did

not present himself at the infirmary until the 30th.

Between that time and the 8th of January, when the operation was performed, a rapid increase took place in the size of the tumor. Unlike the author of the paper briefly alluded to above, Mr. Halton prefers the mode of making the first incision which was first recommended by the late Mr. Ramsden, namely, that in the line of the clavicle, from a belief that thereby hæmorrhage is more likely to be avoided. "The integuments were drawn down a little over the clavicle, and, with the platysma myoides, divided by a scalpel upon the upper edge of the bone to the extent of about three inches: the incision commencing nearest the shoulder, and terminating just beyond the sterno-mastoid muscle. The integuments above the incision, and on the outer edge of the sterno-mastoid muscle being pinched up, were next separated by one sweep of a sharp-pointed bistoury, cutting from within outwards from about the middle of the first incision in a line upwards and backwards, for two inches and a half, due regard being paid to the external jugular vein." By means of the freedom afforded by the external incision just described, the author was enabled to complete the remaining stages of the operation without difficulty, and a double ligature was passed around the artery at a depth of two inches below the clavicle. The hand, arm, and shoulder, were enveloped in a thick layer of warm carded cotton. The result of the case was most favourable: the ligature came away on the twelfth day, the incisions healed rapidly, his recovery was uninterrupted by any bad symptom up to the 27th of Feb., on which day the aneurismal swelling had almost disappeared, and pulsation was distinct in the radial artery. After this the swelling became again large and painful, though without pulsation, and matter formed in the situation of the sac. This being evacuated by trocar, to the amount of twelve ounces, was found to consist of very offensive pus mixed with putrid blood. On the 3d of April the patient was entirely well.

The discussion which followed the reading of these papers was rather irregular, and included several different subjects: we have therefore reduced it to some degree of form.

With reference to the cause of the difficulty of swallowing and the dyspnœa which took place soon after Mr. Skey's operation, the President said he supposed it was to be referred to a kind of nervousness, which might be called hysterical.

Dr. Addison suggested that it was rather to be referred to some injury or affection consequent on the operation of the recurrent nerve, perhaps to one of its branches being included in the ligature. The symptoms were very like those produced by the pres-

sure of ordinary aneurisms of the aorta upon the vagus nerve or its recurrent branch, in which he had had frequent opportunities of observing the occurrence of occasional fits of extreme dyspnoea; and he thought similar cases must have presented themselves to the surgeons present.

Mr. Skey said he was perfectly sure that no nerve had been included in the ligature; nor, if that had been the case, would it be possible to explain by it why the symptoms should have come on, not directly, but at a distant period after the operation; nor why, having come on, they should so soon and for ever cease.

Sir B. C. Brodie having paid a high compliment to Mr. Skey on the mode in which the operation was performed, said it was quite impossible that any important nervous branch should have been included in the ligature. He had often seen the symptoms alluded to by Dr. Addison produced by aneurisms of the aorta, but they were certainly not always the result of pressure on the vagus or recurrent nerve. They would arise from the direct pressure of the tumor upon the trachea itself. He recollected many years ago examining the body of a servant of the late Mr. Canning. He had suffered for a considerable time from tracheal symptoms, and from severe attacks of dyspnoea, coming on suddenly and often, on going up stairs or making any great exertion. No one could make out the source of his complaints; but after death (at which time Sir Benjamin first saw him (he found an aneurism of the arch of the aorta pressing upon the trachea, narrowing its canal to about a third of its usual calibre, and accompanied by intense vascularity and redness of the mucous membrane. (The account of this case was confirmed by Dr. Kerrison.) He was inclined to think the symptoms in this case must have resulted from something of a peculiar state of nervous disorder, which he had sometimes seen after severe operations. He remembered a case in which he tied the subclavian artery outside the scalenus muscle, for an axillary aneurism, in an otherwise healthy man. The operation was performed quickly, and without difficulty; and for a time all seemed going on well. Shortly after, however, the man became very ill; he could not remember the exact symptoms, for the case happened fifteen years ago, but he remembered that the man sweated most profusely, and that the lower extremity of the opposite side to that on which the artery was tied became very swollen, and quite dark, as if from extreme congestion of its veins. After a few days the man died, and they found at the examination that the swollen limb was in a state closely approaching to gangrene, but that there was no venous inflammation

whatever. Another case of the same kind happened to him some years afterwards. A lady consulted him for a large pulsating tumor—an aneurism by anastomosis, as it was absurdly called—on the forehead; on this he operated, destroying it by ligature, and it did perfectly well, and seemed altogether removed. Two years afterwards, however, she came to him again, and he found that at one of the edges of the cicatrix the disease had returned, and formed a pulsating tumor about as large as the end of his little finger. He was not anxious to remove it, but she much wished for it, and he therefore again tied it, passing needles under it, between it and the tendon of the occipitofrontalis muscle, and thus tying threads very tightly round and beneath the needles. Some few hours after the operation, the lady, who had always been very nervous and hysterical, became extremely excited, and soon passed into a state exactly similar to that known by the name of traumatic mania. One of her lower limbs also became very large, and quite dark, as if from venous congestion. She died in a few days, as if exhausted by the mania; and on examination her leg was found perfectly gangrenous, but no further inspection of the body was permitted.

Dr. Copland said that he believed in these cases the signs of dyspnoea, &c. were the result of the ligature being placed on branches of the ganglionic nerves. These, it was known, formed a complete plexus, and almost a sheath, around the large vessels, and had numerous communications with the most important plexuses in the body; and in such difficult and important operations it was almost certain that some of these would be included or injured. In all these cases, therefore, it was necessary to look, not at the mechanical effect of putting a ligature on a large artery, but at its vital influence.

Mr. Arnott said it was impossible to ascribe these symptoms to the ligature of ganglionic nerves. These were just as likely to be tied in the operations on the great arteries of the extremities, the external and internal iliac and the femoral, for example; yet ligatures of these vessels never produce such a result, and it was therefore necessary to believe it the consequence of some local injury. He had been often able to confirm Dr. Addison's remark on the influence of aneurisms in or near the arch of the aorta in producing these symptoms; and it was well for surgeons to be aware that these symptoms were so like those of actual inflammation or other obstruction of the trachea, that he knew cases in which tracheotomy had been performed, with the view of affording relief to the suffocating dyspnoea.

Mr. James Blake said that he could not believe the signs alluded to to be the result

of any distension of the vessel below the ligature. Some such distension would probably take place as it did after tying a healthy artery, but not sufficient to produce the symptoms. On the subject of facility with which the blood gains access to a limb after its main artery had been tied, he believed that a difference must be made between the cases in which healthy arteries were tied, and those in which they were tied for the cure of aneurism, in which last he thought the circulation was restored, and nearly the full force of the blood regained in the limb beyond the ligature, with particular facility. He had lately performed an experiment on the subject. Having tied the femoral artery of a dog, he introduced the tube of the hemadynamometer into its trunk below the ligature, with the point directed towards the capillaries, and he had found that directly after the ligature the force of the blood in the trunk beyond it, carried thither by the anastomosing circulation, was fully two-thirds of what it had been previously. But he believed that in cases of ligature for aneurism the force would be still more completely restored; for in these the branches by which the collateral circulation would bear to be carried on were already in great measure enlarged, though the passage of blood through them was obstructed by the pressure of the aneurism. Now the immediate effect of the ligature was to reduce the volume of the tumor, and the amount of compression in the collateral branches, and he therefore thought it most probable that directly after the application of the ligature in such circumstances, the force of the blood in the limb beyond it would be almost completely restored.

Dr. Addison explained that in speaking of injury or irritation of the recurrent nerve, he had not limited it to the effects of a ligature placed on one of its branches; that might not have happened, but irritation might have spread to the nerve from the wound, or from some nerve wounded in the operation.

On the subject of the phlebitis stated to have occurred in the course of recovery, Mr. Perry asked Mr. Skey whether he had been led to the use of the friction of mercury and opium into the excoriated surfaces of the limbs by any benefit which he had seen derived from it before; and whether he had any further proof than was afforded by this case, in which its results appeared very satisfactory, that it was a good remedy in phlebitis.

Mr. Skey said he had had no experience of it. He had resorted to it, in fact, as a *pis aller*. It was absolutely necessary that something should be done; the case did not admit of bleeding, nor of the free use of mercury, and he had adopted this means as likely to afford at least some relief to the

symptoms. He thought the result had proved it a remedy worth trial on future occasions.

Dr. Copland said there was no proof that there had been any phlebitis at all; and that the subsequent treatment for it could not, therefore, be appealed to as having contributed to the successful termination of the case.

Mr. Arnott, also, said that he entertained great doubts whether there had been phlebitis. There often occurred a fulness and tension of the veins of the limbs which might be taken for phlebitis, but in which none existed, and such he thought might have been the case here. Mr. Skey had spoken, he believed, of a glandular enlargement of the groin; that might have had some influence on the state of the veins. But, indeed, if this were a case of phlebitis, what had been observed in it could be no guide to what should be done in future cases, for this was quite different to the ordinary and most severe cases of traumatic phlebitis. The veins supposed to be inflamed were not seated in or near the parts where any had been wounded, but at a considerable distance from them. This was itself some reason for thinking the case not phlebitis, although he was aware that the disease did sometimes happen in veins far distant from those first injured. He had seen two such cases, where, in consequence of injury of a vein in the neck, phlebitis had taken place in the veins of the lower extremities.

Mr. Skey said there could be no reasonable doubt of this having been a case of phlebitis. The veins were not swollen, or congested, or tense merely; they were hard, cord-like, and knotty; and, he had no doubt whatever, were blocked up by coagulable lymph.

The previous discussions were interspersed with a rather warm conversation between Dr. Copland, Mr. Perry, and Mr. Skey, on the length of the last-mentioned gentleman's paper, which Dr. C., considering that a great number of papers were waiting to be read during the session, thought unnecessarily prolix. Mr. Skey admitted the fault; but said he had been unwarily led into it by the belief that the Society still held the opinion that he knew they did some years ago, namely, that individual cases, without any comments upon them, were not worth communicating. Mr. Perry said it would be well if the members of the Society would, in future years, communicate their papers much earlier in the season; at the beginning of this season it was difficult to find papers enough for the meetings; now, on the contrary, there were so many that, whatever might be their length, it would be impossible to do justice to them in the remainder of the meetings.

COURT OF QUEEN'S BENCH.

Tuesday, May 25th, 1841.

The Society of Apothecaries v. Greenough.

THIS was an action to recover penalties from the defendant for practising as an apothecary at the town of St. Helen's, in Lancashire, without possessing the certificate required by the Act of the 55th Geo. 3, c. 194, which was tried before Mr. Baron Maule and a Special Jury, at the Liverpool Summer Assizes, 1839, when a verdict was found for the defendant. It appeared from the evidence of the witnesses called by the plaintiffs, that the defendant's sister, Mary Greenough, kept a chemist's and druggist's shop in St. Helen's, and that the defendant was her assistant; that he had attended patients for pleurisy, inflammatory fever, violent cold, typhus fever, and other medical complaints; had supplied them with medicines prescribed by himself, and in two instances bills were produced which he had sent in to the parties. In one of the bills charges were made for visits and medicines, and in the other for medicines only, and in both he described himself as "Surgeon." It further appeared that the defendant's practice as a medical man had been very extensive.

Mr. Cresswell having in a former term obtained a rule *nisi* for a new trial, on the ground of misdirection of the learned judge, the rule now came on for argument.

Mr. Justice Coleridge having read the judge's report,

Mr. Watson appeared, on behalf of the defendant, to shew cause against the rule. The learned judge had told the jury that what was done by the defendant was an acting as an apothecary, unless the jury thought that he was acting as chemists and druggists acted at the time of the passing of the Apothecaries' Act; and that it was a question of fact for the jury to find what the practice of chemists and druggists had been at that period.

Lord Denman.—Can you defend that ruling, Mr. Watson?

Mr. Watson.—I apprehend most decidedly I can defend it, my Lord.

Lord Denman.—If he has acted as an apothecary, what would it have signified if he had acted as a chemist also?

Mr. Watson.—Because the statute expressly provides for it in the 28th section.

Mr. Justice Coleridge.—That is not to prejudice the trade of a chemist and druggist in preparing and dispensing drugs.

Mr. Watson.—"Dispensing" is the very word used both in the Apothecaries' Charter and this Act of Parliament. Apothecaries were persons who kept shops, and who dis-

pensed medicines at the request of their patients.

Mr. Justice Coleridge.—What operation do you say the Act of Parliament has then?

Mr. Watson.—It has this—it leaves the chemists and druggists to dispense medicines in any way that chemists and druggists had been in the habit of dispensing medicines before. According to the last opinion of all medical practitioners, the uniform opinion on the subject is this, that the Act of Parliament left chemists and druggists to deal with medicines as before; that is to say, either to sell medicines over the counter, or, if they were bought, to send them to the patients.

Mr. Justice Coleridge.—But not to go and visit a patient, and attend upon him?

Mr. Watson.—There is no objection to their visiting if they merely charge for their medicines. That is the very question that has been agitated with regard to physicians.

Mr. Justice Coleridge.—One of the defendant's bills expressly charges for visits.

Mr. Watson proceeded to contend that a person who merely sells medicines, though he may visit—though he may prescribe, still, if he does it merely as a chemist and druggist, and does not hold himself out as an apothecary, he is not within the Act. That under the 28th section of the Act, chemists and druggists were protected in doing whatever chemists and druggists had done before the passing of the Act; and that the judge was right in leaving it as a question of fact for the jury to say what the practice of chemists and druggists was before the passing of the Act; that the learned judge could not have taken upon himself to say that attending a patient, or prescribing for a patient and making up the medicines, was practising as an apothecary, and not as a chemist and druggist.

Lord Denman.—You say there was no evidence of what a chemist and druggist was before the passing of the Act. Does not the direction amount to this—that a chemist and druggist might act as an apothecary before the statute?

Mr. Watson.—No, my Lord; the learned judge's direction was this: "Gentlemen, you are to tell me this:—we want to know what the practice of a chemist and druggist was." The jury said, "may we ask for the distinction between a chemist and an apothecary?" upon which Mr. Baron Maule said, "Gentlemen, you must tell *me* that, and then I will tell you what the law is on the subject;" so that really after all it was a question of fact. The learned counsel then cited the case of the College of Physicians against Rose, 1 Brown's Parliamentary Cases, 78, and observed that, from the reign of Henry VIII, down to the reign of

Queen Anne, it was supposed that no one could prescribe any medicines but physicians.

Mr. Justice Coleridge.—In 1815, when the Act passed, what distinction do you say there was between chemists and druggists and apothecaries?

Mr. Watson.—I say there really was none substantially.

Mr. Justice Coleridge.—If so, then why were not the chemists and druggists put under the same regulation as the apothecaries. I want you to tell me the distinction between them at the time of the passing of the Act?

Mr. Watson.—I say, my Lord, that at that period the apothecary went and attended his patients from time to time, and that chemists and druggists did the same when they were asked to do so.

Mr. Justice Coleridge.—Then there was no distinction.

Mr. Watson.—Only people went to the apothecaries because they were a better educated class.

Mr. Justice Coleridge.—Then according to your argument the better educated class were governed, and the worst educated class were left as they were.

Mr. Watson.—If my client had set over his door "Doctor Greenough," and had got a gold-headed cane, and had gone out prescribing, I submit there was no law to prevent him. Supposing a coach-load of passengers had been upset, as I said when I addressed the jury, and he had attended them all for six months, and cured them, and prescribed for them, he could have recovered from them all if he had held himself out as a surgeon. It is not an Act to regulate the medical profession.

Mr. Justice Coleridge.—It is to regulate the apothecaries. You say an apothecary and a chemist was the same thing at that time.

Mr. Watson.—They both engaged in pharmacy, but they were not an equally educated class. No doubt, my Lord. what I contend for is this—that as chemists and druggists, if I went into a shop and ask them to prescribe for me, I put myself into the hands of a person who had not received so good an education, and I must take the chance of that, but still there was nothing to prevent them from doing so. If a man holds himself out as a chemist and druggist, and I choose to go to him and ask him for his advice, as to what medicine I ought to take, that is merely practising as a chemist and druggist, as it would be merely (using the words of the statute) a "dispensing of his medicines."

Mr. Justice Coleridge.—Surely it cannot depend upon his recommending you medicines in his shop, or his getting into his carriage.

Mr. Watson.—If a person holds himself out as an apothecary, and goes round and visits as an apothecary, then I admit at once he would be liable to the penalties.

Mr. Justice Williams.—In what way do you say this man acted?

Mr. Watson.—I say he held himself out as a chemist and druggist, and nothing else, my Lord.

Lord Denman.—What was the meaning of chemists and druggists before the Act? Were they in the habit of attending as well mixing the drugs? And if so, would not the doing of that afterwards be an infringement of the act?

Mr. Watson.—Then cited the case of *Alison v. Haydon*, which decided that a surgeon who was not an apothecary could not recover for medicines and attendance in a case of typhus fever; and argued that the plaintiff had in that case held himself out as an apothecary, and that it was no authority against the present defendant. The learned Counsel concluded by calling upon the court to support the direction of the learned Judge.

Mr. Cresswell.—My Lords, I am quite sure that no jury having that respect for the opinion of a learned judge, which all juries are bound to pay, could have found any other verdict than that which they did under his Lordship's direction. It amounted, my Lords, to a direction for the jury to give the verdict they did, while the jury almost remonstrated against it.

Mr. Watson.—I do not know where you find that.

Lord Denman.—I think it is clear the direction assumed that character.

Mr. Cresswell.—The jury came back and said, in the absence of any evidence of what was the practice of chemists and druggists before the Act, we find for the defendant, desiring to hand a written paper to the learned Judge, which he refused to receive; he saying, I give you my opinion on the law, you had better find a verdict absolutely one way or the other according to that opinion; and then they found a verdict for the defendant.

Lord Denman.—At all events there was a *primâ facie* case, and it was for the defendant to take himself out of the Act.

Mr. Justice Patteson.—I do not see the least possibility of doubt in this case, unless it can be said that an apothecary and a chemist mean the same thing, whereas the Act distinctly negatives it.

Mr. Justice Williams and Mr. Justice Coleridge concurred, and the Court, without hearing the plaintiff's counsel in support of the rule, made the rule for a new trial absolute.

SHOTLEY BRIDGE SPA, IN
DURHAM.

As it now appears, the Spa water at Shotley Bridge holds a middle place between the absolute chalybeates, and the purgative chalybeate springs of this country. It is an "alterative chalybeate," eminently calculated to relieve and cure diseases of weakness and obstruction in the circulation, glandular affections of the mesentery, dyspepsia, deficiency of tone in the intestines, impurity of blood, or tendency to decomposition, each calculated to produce cutaneous diseases and land scurvy; and, when used as a warm bath, rheumatic complaints have been singularly benefited by it. The water, in fact, is a most valuable one, containing a large yet manageable proportion of proto-carbonate of iron, with neutral salts sufficient to prevent the inconveniences which mere chalybeates sometimes occasion. Three distinct muriates, all of them valuable, are present in the water besides the iron; and in addition to them we find an excess of carbonic acid beyond that which goes to form the proto-carbonate of iron, combined with soda. The presence of bromine, also, though in minute quantity, and of traces of carburetted hydrogen gas, add to the medicinal efficacy of the water. In fact, properly studied and properly attended to, the Shotley Bridge Spa water may be made instrumental in the recovery of many disorders which no other water in the country can cure. The water differs in its composition from all the others I have examined in my recent tour.—*The Spas of England, by Dr. Granville.*

BOOKS RECEIVED FOR REVIEW.

Sir James Clark on the Sanative Influence of Climate; with an Account of all the best places of resort for Invalids in England, the South of Europe, &c. Third Edition, 1841.

Dr. James Arnott's Treatise on Stricture of the Urethra; containing an Account of Improved Methods of Treatment; with an Appendix on Dilatation by Fluid Pressure in the Treatment of Urinary Calculus and other Diseases. Second Edition, 1840.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED
CERTIFICATES.

Thursday May 20, 1841.

Charles Cowell Turner, Bideford. — John Tooker Spry Jolly, Tourquay. — Horace Vidal, Ilfracombe. — Robert Newham Barnes Mathews, Lowestoft. — James Alexander, Edenbridge. — William George Tiley, Reading. — Duncan Robert M'Nab, Epping. — William Burdett Letters, Rochdale. — Edwin Pinder Fordham. — Evan Burnell Jones, Kennington. — John Adam Townsend, Farnham.

Thursday, May 27, 1841.

George Thomas Vicary, Warminster. — William Collard Pyne, Wellington, Somerset. — Charles Saunders, Foulsham, Norfolk. — R. H. Carter, Kent. — Thomas Lightfoot, Nottingham. — John Duncan, South Shields, Durham. — Charles Fox, Devonshire. — John Gray, Bath.

A TABLE OF MORTALITY FOR THE
METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 22nd May, 1841.

Small Pox	27
Measles	13
Scarlatina	6
Hooping Cough	43
Croup	7
Thrush	3
Diarrhoea	4
Dysentery	0
Cholera	0
Influenza	4
Typhus	20
Erysipelas	4
Syphilis	0
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	127
Diseases of the Lungs, and other Organs of Respiration	237
Diseases of the Heart and Blood-vessels	11
Diseases of the Stomach, Liver, and other Organs of Digestion	61
Diseases of the Kidneys, &c.	1
Childbed	9
Ovarian Dropsy	0
Diseases of Uterus, &c.	3
Rheumatism	1
Diseases of Joints, &c.	3
Ulcer	0
Fistula	1
Diseases of Skin, &c	1
Diseases of Uncertain Seat	77
Old Age or Natural Decay	51
Deaths by Violence, Privation, or Intemperance	15
Causes not specified	5
Deaths from all Causes	734

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

May.	THERMOMETER.		BAROMETER.	
Wednesday 26	from	52 to 76	30·06 to	29·98
Thursday . 27		57 78	29·89	29·84
Friday . . . 28		57 74	29·89	30·00
Saturday . 29		49 69	30·05	30·06
Sunday . . 30		53 70	30·00	29·95
Monday . . 31		47 75	29·95	29·99
June.				
Tuesday . 1		50 69	30·07	30·12

Wind, N.E. on the 26th, and following day; S.E. on the 28th; North by West on the 29th; West by North on the 30th; since S.W.

On the 26th, and following day, clear. The 28th, morning cloudy, otherwise clear. The 29th, morning clear, otherwise overcast; rain in the evening. The 30th, and two following days, clear.

A storm of thunder and vivid lightning on the evening of the 27th, and morning of the 28th.

A brilliant halo on the 29th, during the greater part of the day.

Rain fallen, '11 of an inch.

CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, Lon on.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, JUNE 11, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXXVIII.

Paralysis agitans. Mercurial tremor. Hysteria: two forms of hysteric paroxysm; diagnosis from epilepsy; class of persons most liable to hysteria; diseases apt to be simulated by hysteria; treatment; prevention.

Paralysis agitans.—In the last lecture I spoke of chorea, and of some singular forms of disorder that have sometimes been included under the same appellation; and I shall begin the present with a few observations concerning a disease very closely allied to some of those which we were then considering, and yet distinct enough to deserve and require a separate notice. I refer to what has been called the *shaking palsy*—*paralysis agitans*. Allusions to this form of disease are to be found in many of the older systematic writers on physic; but it never was much attended to in this country until Mr. Parkinson published an essay upon it in the year 1817; and a very interesting little pamphlet it is. He defines the disease thus:—"Involuntary tremulous motion, with lessened muscular power, in parts not in action, and even when supported: with a propensity to bend the trunk forwards, and to pass from a walking to a running pace: the senses and intellects being uninjured." The latter symptoms constitute the *scelotyrbe festinans* of Sauvages; and the former symptoms of the definition are not always attended by the latter. In old persons you may often observe inces-

sant and involuntary nodding and shaking of the head, without any tendency to run forwards. There is an old woman whom I see regularly sitting in the aisle at church every Sunday: she walks to her seat slowly and steadily enough, and sufficiently upright; but her head never ceases to nod and wag, and tremble in various directions. It may be that she is in the less advanced stage of the malady; but I have remarked her for three or four or more years, and I see no change.

Mr. Parkinson's notice was first called to the disease during his professional attendance upon a person affected by it. From observation of that case, and of several others that he subsequently met with, his account of the disorder was drawn up. He states that its first approach is insidious, and its progress often so slow and imperceptible that the patient cannot recollect precisely when it began. A sense of weakness, and a disposition to trembling, fastens on some particular part: sometimes it is the head, but more commonly it is one of the hands or arms. These symptoms gradually become more decided; and at length the morbid influence is felt in some other part. At a still more advanced period the patient is found to be less strict than usual in preserving an upright posture, even when standing or sitting, but especially when walking. By degrees he finds a difficulty in making the hand obey the dictates of the will when he is engaged in any delicate manipulation—in writing, for example; and he is obliged to walk with circumspection and care: his legs are not raised to that height, nor with that promptitude, which the will directs; so that much attention is necessary to prevent frequent falls. Then, as the malady proceeds, the propensity to lean forwards becomes more strong—the patient is forced to step on the toes and forepart of the feet, while the upper part of the body is thrown so far forward as to render it difficult to avoid falling on the

face : in some cases he is irresistibly impelled to take much quicker and shorter steps than common, and thereby to adopt unwillingly a running pace. When once this state has been pointed out, I make no doubt that some of you may recognise it, in old persons, whom you may have seen walking about. But the disorder does not stop here ; the unhappy patient becomes unable to feed himself ; or to walk at all without an attendant, who walks backwards before him, and prevents his falling forwards by the pressure of his hands against the forepart of the patient's shoulders : his powers of speech and deglutition fail ; and the saliva dribbles from his mouth : he can no longer retain his urine or fæces ; and at length death closes the miserable scene.

Mr. Parkinson conjectures that this complaint results from some chronic change of the upper part of the spinal cord, or of the medulla oblongata : but dissections are wanting to support or to refute that conjecture. Some of the patients, whose cases he has given, had been intemperate livers ; hard drinkers : other had not been guilty of any such excesses : several had suffered a good deal from rheumatism, which he thought might have laid the foundation of their lamentable disease. But a more exact pathology of the shaking palsy is still wanted. Dr. M. Hall observes that the symptoms have, in several particulars, a marked resemblance to the effects observed by M. Serres (and related in his *Anatomie du Cerveau*) of disease of the tuber annulare, or of the tubercula quadrigemina.

Nor have we any ascertained means of curing this disease ; or rather, this state of decay. Dr. Elliotson indeed says that he succeeded in one instance (of which, however, the particulars are not given), with the carbonate of iron : but that he had tried the same medicine in vain in several other cases. We must administer to symptoms, and endeavour to set those functions right which may be obviously wrong : to regulate the bowels, to procure sleep, to nourish and uphold the patient without unduly stimulating him : and this is all that I can tell you of the shaking palsy.

Mercurial tremor.—Another analogous disorder, deserving a moment's notice, is that peculiar kind of trembling which is apt to occur in persons who are much exposed to the poisonous fumes of mercury : *mercurial tremor* it is called ; and popularly, *the trembles*. It consists in a sort of convulsive agitation of the voluntary muscles, which is most violent whenever efforts are made to move the limbs by the help of those muscles ; whenever, in fact, volition is brought to bear upon them. It differs therefore from the shaking palsy, inasmuch as the tremor ceases

when the muscles are supported, or are not called into action. It is also more susceptible of relief by medicine. The last person in whom I have witnessed this curious affection has been twice my patient in the Middlesex Hospital, and has twice got well there. John Chattin, 33 years old, was first admitted in August 1837. He was led unto the room, walking with uncertain steps, his limbs trembling and dancing as though they had been hung upon wires. While sitting on a chair he was comparatively quiet ; you would not have supposed that he ailed any thing ; but as soon as he attempted to rise, and to walk, his legs began to shake violently with a rapid, incessant, and irregular motion. He could neither hold them steady, nor direct them with precision. Indeed without support he must have fallen down. His arms were agitated with similar involuntary movements. His tongue was tremulous, and he spoke in a hurried, abrupt, interrupted, staccato manner, not natural to him. He had no fever. His pulse was 66, and soft ; his skin natural ; his bowels costive. He complained of slight nausea. At the end of six weeks he went out well, or with very slight remaining weakness of his knees, and a little occasional tremor upon unusual exertion. In June 1839, he again presented himself, in a similar state of agitation and helplessness.

This man was a water-gilder ; and had been employed in that business for 18 years. Till somewhat more than a twelvemonth prior to his first appearance at the hospital, he had been free from disease. Then he began to tremble a little ; but for a fortnight before his admission the shaking had become so much worse that he could not go up stairs, nor even walk upon uneven ground. The trembling, when once brought on by efforts to move, did not cease until he sat down, or got one of his fellow-workmen to grasp his limbs tightly.

This singular disorder is produced by the agency of mercury as a poison upon the body ; and especially by the absorption of that metal when raised into vapour by heat, and inhaled in breathing. It is accordingly very common among water-gilders. Water-gilding is the gilding of metals, and of silver in particular, by means of fire. It is called *water-gilding*, I believe, to distinguish it from other kinds of gilding, called *gilding in oil*. The silver to be gilded is covered with an amalgam of gold and mercury, and then is placed over a charcoal fire, by which the mercury is raised in fumes, and driven off, and the gold alone is left adhering. To these fumes the workmen are necessarily exposed ; and numbers of them become affected with this tremor, which is not a common result of mercury applied to the system in other ways. The same complaint is frequent among the workmen in the quicksilver

mines of Friuli and of Almaden, where the crude ore is purified by the aid of heat. Dr. Bateman relates, in the 8th volume of the *Medico-Chirurgical Transactions*, some cases like that which I have been describing. But the best account of the disorder that I have seen is given by Merat, in an appendix to his book on the *Colique Métallique*.

The malady comes on sometimes suddenly, more often by degrees. The patient is less sure of his arms than usual; they become tremulous, and at last shake, and, if the patient continues to pursue his employment, the force of the trembling goes on increasing, till at length it is so general and violent that he can continue no longer. His power of locomotion is impaired; his mastication, his speech, all his manual operations, are interfered with; he becomes unable to convey food to his mouth, and is obliged to be attended to and fed, like an infant; and by and by, if he does not quit the poisonous atmosphere, graver symptoms supervene—wakefulness, delirium, loss of consciousness.

As the tremor increases, the digestive organs become disordered; the appetite falls off, nausea is felt, the tongue becomes furred, and gas collects in the intestines. The patients acquire a remarkable brown hue; and their teeth turn black. The pulse is generally full and slow.

The time required for the production of these effects varies much in different cases; from two years to five-and-twenty. Something depends, no doubt, upon the quantity and intensity of the fumes. Chattin told us that the workmen became ill whenever they had a *large job* on hand. In both his severe attacks (and very often besides, both in him and in his companions) the mercury produced salivation. This was unfrequent in the patients observed by Merat. The duration of the complaint is considerable: it may last two or three months, or longer; and sometimes it is not completely recovered from at all. Yet it is not a fatal disorder.

Although the visible affection is of the *muscles*, the mischievous operation of the poison is really upon the *nerves*, weakening their natural influence. When the will is directed upon the muscles, they contract unsteadily, and with frequent remissions; their action is not sustained; and it is a general observation by all who have written upon the disease, that it is aggravated by all kinds of mental emotion, by alarm, anger, surprise. My patient's shaking was, at first, augmented by the shock of the shower-bath: and always became excessive in thundery weather. So, on the other hand, it has been noticed that whatever tends to stimulate and steady the nervous power, does temporary good: a glass of wine, for example. Chattin informed us that, while the malady was coming on, he could not get up stairs to his work with-

out first swallowing half a quartern of gin: and that he was obliged to drink porter two or three times a day.

The treatment consists in withdrawing the patient from the injurious atmosphere, and in administering tonics.

Conium has been recommended by Mr. M'Whinnie. Quina has been found useful. But I have most faith in preparations of iron. My patient Chattin mended decidedly and rapidly when he began to take steel. It was not the mere avoidance of the cause of the complaint that produced the improvement, for he had been away from his work for a fortnight before he applied for admission.

To prevent this effect of mercury, the workmen should be instructed to avoid, as much as possible, inhaling the poisonous fumes, to ventilate the room freely, and to pay great attention to cleanliness. I believe that the furnaces may be so built that the metallic vapour cannot reach the operator. If he cannot avoid being involved in it, perhaps some sort of respirator might afford protection.

Hysteria.—I proceed to the subject of *hysteria*: a subject highly interesting and important, as well as obscure and difficult. I scarcely know how to arrange what I have to say, so as to present the disorder to your notice in the most intelligible manner. *Hysteria* has characters peculiar to itself: but it also is apt to assume the form, and mimic the symptoms, of various other diseases of a much graver nature. If we are not capable of distinguishing the true malady from that which is its double, we shall be constantly committing most serious mistakes in the prognosis, to our own damage and discredit; and in the treatment, to the injury of our patient. I shall first attempt to describe to you the phenomena which are peculiar to *hysteria*; and then to point out the class of persons who are most subject to it; and afterwards I shall briefly advert to the imitative freaks which we are almost daily witnessing in hysterical constitutions, and to some other points connected with this extraordinary complaint.

I need not tell you that the hysterical *paroxysm* is almost exclusively confined to women. It occurs under a great variety of forms, but they may all be reduced, for convenience of description, to two. The first of these has a general resemblance to an epileptic fit. The trunk and limbs of the patient are agitated with strong convulsive movements; she struggles violently, like a person contending, rises into a sitting posture, and then throws herself back again, forcibly retracts and extends her legs while her body is twisted from side to side; and so powerful are these muscular contortions that it often is all that three or four strong persons can

do to restrain a slight girl, and prevent her from injuring herself or others. The head is generally thrown backwards, and the throat projects; the face is flushed; the eyelids are closed and tremulous; the nostrils distended; the jaws often firmly shut; but there is no *distortion* of the countenance: the cheeks are at rest, unless when, as often happens, the patient is uttering screams, or exclamations. If the hands are left at liberty, she will often strike her breast repeatedly and quickly, or carry her fingers to her throat, as if to remove some oppression there; or she will sometimes tear her hair, or rend her clothes, or attempt to bite those about her. With all this her breathing is deep and laborious; and the heart palpitates. After a short time this violent agitation is calmed: but the patient lies panting and trembling, and starting at the slightest noise or the gentlest touch; or sometimes she remains motionless during the remissions, with a fixed eye; till all at once the convulsive movements are renewed: and this alternation of spasm and quiet will go on for a space of time that varies considerably in different cases: and the whole attack frequently terminates in an explosion of tears, and sobs, and convulsive laughter.

There is a *variety* of this form of hysterical paroxysm, in which the patient suddenly falls down insensible, and without convulsions: with slow and interrupted breathing, a turgid neck and flushed cheeks; and she recovers from that condition, depressed in spirits, fatigued, and crying.

You will observe that the symptoms I have been enumerating belong to the nervous system; and indicate great derangement in the functions of animal life. In the other of the two forms to which all the various modifications of the attack may be reduced, the principal marks of disturbance are referrible to some of the viscera. The patient experiences a sense of uneasiness in some part of the abdomen, frequently towards the left flank; a ball appears to roll about, and to rise first to the situation of the stomach, and then to the throat, where the patient feels a choking sensation; the action of swallowing is frequently repeated; the abdomen becomes distended with wind, loud rumblings and sudden eructations take place; there is much palpitation of the heart, the patient is sad and sorrowful, and prone to shed tears.

After the paroxysms, these patients commonly void a large quantity of limpid, pale urine, looking almost like water; and this is sometimes expelled during the fit.

Such is a brief, and, I am aware, incomplete account of the hysterical paroxysm: it sets forth, however, the outline of the two principal varieties of the attack: and you are to observe that the last, the quieter form, is

often the prelude to the convulsive; but it not seldom also occurs alone, and then is as indicative of hysteria, as the *petit mal*, to which it is somewhat analogous, is of epilepsy.

Diagnosis from epilepsy.—And before I go any farther, let us again enquire into the circumstances which distinguish the *paroxysms* of those two diseases, epilepsy and hysteria. I have shortly adverted to them before; but we shall be better able to appreciate them now that the main features of each diseased state has been under our consideration. It is of great importance to be able to render the diagnosis certain and accurate. It is a dreadful announcement to have to make to a father or a mother that their child is epileptic: whereas hysteria, though it is sufficiently distressing, is attended, in 999 cases out of 1000, with no ultimate peril either to mind or body. In some instances the diagnosis is perfectly easy; in others it is dubious and full of anxiety. Whenever you fail to satisfy yourselves completely as to the nature of a given case, you will do well, in legal phrase, to give your patient the benefit of your doubt, and acquit her of epilepsy; or pronounce her guilty of the minor offence of hysteria.

The points of resemblance, and the points of distinction, belonging to the hysterical and epileptic paroxysm respectively, have been very clearly summed up by Foville.

There are two principal forms of each disorder. In each, one of these forms is convulsive, and the other is not. The non-convulsive form of epilepsy relates exclusively to the sensorium: it is characterized by vertigo, and a suspension (however brief and transitory) of the mental powers. The non-convulsive form of hysteria has little apparent connexion with the animal functions: its palpable phenomena consist in derangement of the organic functions of the thorax and abdomen. It is the ganglionic portion of the nervous system that seems chiefly disturbed.

In the epileptic *fit* there is an entire loss of consciousness. The patient, on emerging from the paroxysm, recollects nothing of what has been going on during its continuance. It is not so in the hysterical fit: the loss of consciousness is very seldom complete; and it never occurs at the outset of the attack. The patient often is able to repeat (though she may not always choose to confess it) what has been said by the bystanders during the period when she appeared insensible. This is a point of distinction well worth remembering, for more reasons than one. It not only helps the diagnosis when the fact comes out; but it suggests certain cautions to ourselves. We must take care not to say any thing by the bed-side of an hysterical patient which we do not wish

her to hear ; and we may take advantage of her apparent unconsciousness, and pretend to believe in it, and speak of certain modes of treatment which she will not much approve of, but the very mention of which may tend to bring her out of the fit.

In the epileptic paroxysm the face is usually livid ; and foam which is frothy with air, or red with blood, escapes from the patient's mouth. These are symptoms which we do not see in the fits of hysteria. The convulsive movements even offer some characteristic shades of distinction. In epilepsy they are often more marked on one side of the body than on the other ; and the same movements are rapidly repeated : there is a strangling rattle in the breathing : while in hysteria the forcible flexion and extension of the limbs, and the contortions of the trunk, are more sudden, irregular, and, as it were, capricious ; the respiration is deep, sighing, mixed with cries, and sobs, and often with laughter. But, perhaps, the convulsive motions differ most in the face : the epileptic expression is usually frightful : the eyelids half open, the eyeballs rolling, the teeth grinding, and the gums exposed by the retraction of the lips, the tongue protruded and bleeding ; the complexion leaden : while in hysteria the cheeks are red, but at rest ; the eyelids are closed and trembling ; if you raise the upper one, you will see the eye fixed, perhaps ; but it is bright, and very different from that of the epileptic, which, if it be not rolling, is dull, projecting, and the pupil usually dilated.

Foville states that when, besides a sudden loss of consciousness with convulsive movements, there are also lividity of the face, and an escape of frothy saliva from between the lips, and the convulsions are more pronounced on the one side of the body than the other, the disease is epilepsy, and not hysteria ; and I think he is right.

The hysterical seizure may be over in a quarter of an hour, or in less time than that ; or it may last many hours, or even several days.

The hysterical seizure is almost peculiar to women : and it seldom occurs in them except during that period of their lives in which the menstrual function of the uterus is or ought to be in activity. In this country it is most apt to occur between the ages of fifteen and forty ; and in the vast majority of patients who do suffer it, you will find some marked derangement of that particular function. These facts alone afford a strong corroboration of the ancient theory, which ascribed the whole of the phænomena to uterine disorder ; and *named* the disease accordingly. You will hear or read of disputes as to whether the womb, with its appendages, or the nervous system, is the seat of hysteria. But such disputes are

merely verbal, I conceive : no doubt the convulsive movements, and the mental affection, and the unnatural sensations, depend upon some altered condition of the brain and nerves ; but it does not follow that the disease originates in that altered condition. We know that the uterus or the ovaries cannot of themselves determine the muscles to contract ; but if they be in an unhealthy state they may act upon the muscles through the *medium* of the nervous system : and such I take to be the fact. *How* they do so we no more know, than we know how the little finger is bent when we resolve to bend it.

But, say some, we every day meet with diseased conditions of the uterus and ovaries—amenorrhœa, dysmenorrhœa, menorrhagia, even disorganization—without any of these nervous symptoms : true ; and we cannot always fathom the mystery of this. But one thing is certain, that there exists in some persons a much greater readiness to take on the disease, upon the application of the exciting cause, than in others. This predisposition I have had occasion to advert to again and again, since I began to speak of the *spasmodic diseases* of the nervous system. Such diseases occur in certain individuals only ; and in these individuals there pre-exists a peculiar condition of the nervous system, “ for which,” says Dr. Alison, “ we have no more precise or definite expression than *nervous irritability*, or *mobility* ; a condition which is more common in women and children than in men, and more common in all persons when in a state of weakness, than when in the full enjoyment of muscular strength ; in women, particularly, more common about the menstrual periods, and immediately after delivery, than at other times ; more common likewise in those in whom the monthly discharge is habitually *excessive*, or *altered*, as in *leucorrhœa*, or suddenly *suppressed*, or more gradually obstructed in the different forms of *amenorrhœa*, than in others. In this condition of mobility, both sensations and emotions are intensely felt ; and their agency on the body is stronger and more lasting than usual ; continued voluntary efforts of mind, and steady or sustained exertions of the voluntary muscles are difficult, or impossible ; the muscular motions are usually rapid and irregular, and the ‘ *animus, nec sponte, varius et mutabilis.*’ ” In persons of this moveable temperament, spasmodic complaints are easily excited : and the tendency to their recurrence is increased by each repetition of them.

Now the persons who suffer hysteria are of this class. They are commonly young women, in whom the process of menstruation is in some way or other disordered ; and who either are naturally of a feeble constitu-

tion, or have been debilitated by disease, or by their habits of life. They often are pale; have cold hands and feet; are subject to chilblains; eat but little, and do not fancy meat, which they sometimes absolutely dislike and refuse; or their taste is depraved and capricious; they will devour wax candles, wafers, chalk, sealing wax, slate pencil, and such trash. And, what is very curious and characteristic, although they often abstain almost entirely from animal food for weeks or months together, and take very little nourishment of any kind, they do not in general emaciate. You might expect that, under such a mode of life, they would waste away: but they continue round, and plump, and smooth. Some of them are even ruddy.

And belonging to women of this peculiar constitution there is one other very remarkable character, which it behoves us to make ourselves thoroughly acquainted with. Almost any part of the nervous system, in these persons, is liable, under the influence of slight causes, and even without any obvious cause, to fall into a disordered state of action and suffering more or less resembling that which inflammation or organic disease might excite in the same part.

This is a most important fact: because if we erroneously ascribe symptoms which really result from inflammation to mere nervous or hysterical disorder, we may suffer the patient to perish for want of active measures that would have saved her: and, on the other hand, if we apply to these nervous, imitative, hysterical complaints, the treatment proper for inflammation, we shall generally, indeed, relieve our patient for the time; but we shall leave her more prone to the nervous affection than before, and permanently damaged by our mischievous activity.

Disorders simulated by hysteria.—I say that almost every kind of serious disease may be mimicked by what we must call hysteria. And your skill will sometimes be severely tasked to determine the true import of the symptoms, and the real nature of the case.

One of the diseases which is most often copied by hysteria, is *inflammation of the peritoneum*. You will find a patient complaining of acute pain of the abdomen, aggravated by the slightest pressure; and she shall have, perhaps, a hot skin, a quick pulse, and a furred tongue. When you meet with such symptoms in a young female, in whom there is any derangement or irregularity of the uterine functions, you will do well, before you bleed her to syncope and cover her abdomen with leeches, to ask yourselves whether all this suffering may not be simply nervous. Search into her previous history as much as you can: if you find that she has had similar attacks before;

if she has been known to suffer hysterical fits; and if the tenderness is excessive, and, as it were, superficial, felt upon the slightest touch as much as when firmer pressure is made, you may generally spare the blood-letting, purge the patient well, and cause an assafoetida enema to be thrown into the rectum; and in a few hours you will find that the peritonitis has vanished.

Among the pains that infest females of the hysteric constitution, and which are apt to be erroneously ascribed to inflammation, *stitches and pains in the hypochondria* are probably the most common. They are oftener complained of in the left hypochondrium than in the right. These things are much more generally understood now than they used to be even a few years ago. I cannot tell you how many persons I have seen who had been diligently treated with leeches, and blisters, and blue pill, for supposed chronic inflammation of the liver or spleen, or still more actively depleted for presumed pleurisy or pericarditis, when no such inflammation existed, and when the treatment, by reducing the strength, tended to rivet that mobility of system which was the chief predisposing cause of the pains.

You would scarcely suppose that *palsy*—perfect hemiplegia or paraplegia—could be simulated by hysteria: yet this certainly is the case; and I have seen instances of it even among hospital patients. They are difficult and perplexing cases. The sudden occurrence of the paralysis, without any of the other symptoms which commonly mark the real disease, its sudden disappearance, and, above all, the supervention of an hysterical paroxysm, will often disclose the true nature of the affection. Hysterical affections referred to the throat are very common. *Aphonia*, for example: the voice being lost on a sudden, and returning as suddenly. *Mock laryngitis*. I remember being asked by Sir Charles Bell some years ago to see a young woman in the Middlesex Hospital under his care. She had recently arrived, and was breathing with the stridulous noise peculiar to inflammation of the larynx. She had twice before, in the country, had tracheotomy performed for similar attacks; and there were the scars of the operations on the neck: but both Sir Charles and myself were satisfied, upon considering all the circumstances of the case, that the difficult inspirations were spasmodic and hysterical; and she recovered under the remedies which do good in hysteria. Inability to swallow, *dysphagia*, is another of the hysterical vagaries relating to the parts about the throat. Dr. Bright has a very instructive case of that kind. A patient was sent to Guy's Hospital for stricture of the oesophagus. It was stated that the difficulty of deglutition had existed for several weeks, and was increasing.

The surgeon under whose care she was admitted was instantly struck by certain circumstances which did not seem to consist very well with the notion that there was organic disease. Her appearance belied it, and her age. But he thought it right to examine the œsophagus by means of a probang; and no sooner was the instrument introduced, than the patient went into an hysterical fit, which was followed immediately by hysteria in several females in the ward. The complaint turned out to be nothing but an hysteric constriction, and was soon completely removed. This incongruity and want of harmony among the symptoms often affords a clue to the real character of the complaint. Dr. Conolly states that he was summoned to a lady, whose uterine functions were disordered, and who was affected with what she called *asthma*, a peculiar *difficulty of breathing*. He observed this great inconsistency: the respirations were forty in the minute; the pulse only twenty. And he says that he has met with the same interchange as it were of frequency between the number of pulsations of the heart, and the number of the breathings, in other hysterical females: "the pulse being sometimes as slow as the ordinary respiration, and the respirations sometimes advanced to the ordinary rapidity of the pulse."

Among the hysteric affections of the air-passages, there is a peculiar kind of *cough* which you ought to be acquainted with. It is loud, harsh, dry, more like a bark than a cough. Sometimes it is incessant, sometimes it occurs in paroxysms which, I verily believe, are more annoying to hear than to suffer. Hysterical affections of the diaphragm again are by no means rare. I had a very obstinate case of that sort in one of my hospital patients. She would sit in her bed all day long, uttering every eight or ten seconds a loud and most discordant *hiccup*. And I remember an out-patient, who presented a picture of perfect health, and who came week after week, to be cured of what I could consider nothing but an hysterical *eructation*; it was continual and distressing, and prevented her from obtaining any employment as a servant. Hysterical *vomiting* is also frequent, simulating cancer of the stomach. Nay, hysterical *hematemesis*. A romantic girl was for some *months* under my care in the hospital with that complaint. She vomited such quantities of dark blood, (which did not coagulate, however), as I would not have believed if I had not seen them. Day after day there were potfuls of this stuff; yet she did not lose her flesh, and she menstruated regularly; and what was very curious, the vomiting was always suspended during the menstrual period, and recurred again as soon as the natural dis-

charge ceased. I said she was romantic; but I should rather have said that she had that peculiar mental constitution which belongs to hysterical females. She used to write me long letters of thanks for my attention, though I was heartily tired of her; and these were couched in all the fine language of the Minerva press. At last I sent her away; just as bad as when she came into the hospital. This was five or six years ago; and last year she called at my house with a present of some game, and told me she had got married to a hair-dresser, and was quite recovered.

There is a kind of sanguineous *expectoration* belonging to females of this class, and very likely to mislead the unwary. I meet with two or three instances of it every year. The patient excretes daily, or at irregular intervals, a thinnish fluid something like saliva, more or less tinged and streaked with brown or florid blood. A young hand investigates diligently the source of the bleeding, and puzzles himself to determine whether the case be one of *hematemesis* or of *hemoptysis*. Nine times out of ten it is neither the one nor the other. The blood comes from the mouth or fauces.

Hysterical affections of the *joints* are very common. A young girl became my patient in the hospital for some trifling ailment, and after a short time she began to complain of great pain in her knee and hip; she could not stand upon the limb, nor bear to have it moved or touched. I got one of my colleagues to see her: he was so satisfied of the nature of the case—so convinced that it was a genuine example of inflammation and ulceration of the hip-joint—that he gave a little lecture to the pupils who stood round the bed upon the characteristic position in which the patient lay; and he took her into one of the surgical wards to be under his own care. Some time afterwards I had occasion to go into that ward, and there I found my former patient with her heel drawn tight up against her buttock. It turned out that she had had no serious disease of the hip at all: both it, and the rigid contraction, gave way under measures which could have done no good to an ulcerated joint. I think the first clue to the real nature of her malady was the occurrence of a fit of hysteria. Sir B. Brodie says that among the higher classes of society, at least four-fifths of the female patients who are commonly supposed to labour under diseases of the joints, labour under hysteria, and nothing else.

Another prank belonging to hysteria, and one which it is very necessary that you should be on your guard against, is that of mimicking disease of the bones of the spine. The patient complains of pain and tenderness in her back, and of weakness probably in her lower extremities: and it is now be-

come notorious that scores of young women have been unnecessarily confined for months or years to a horizontal position, and have had their backs seamed with issues, for supposed disease of the bodies of the vertebræ, who had really nothing the matter with them but hysteria, and who would probably have soon ceased to complain, if, instead of being restricted to that unnatural imprisonment and posture, they had taken a daily gallop on horseback.

It is curious enough how the mind is apt to become affected in some of these cases. After the patient has been lying supine for some weeks, she is unable to stand or walk, simply because she *thinks* she is unable. The instant she makes a fair effort to use her limbs again, she can and does use them. Her condition is at once reversed. *Potest quia posse videtur*. Mr. Corfe, the present apothecary to the Middlesex Hospital, has no little trouble with patients of this kind; but he generally succeeds in *making* them walk, and in convincing them, as well as himself, that they may do so with impunity. Sometimes, though the authority of the Doctor may not be efficacious in this respect, some stronger influence prevails. A lady told me not very long ago that an acquaintance of hers, a member of a family of distinction, had been lying I know not how long on her back; that position having been prescribed to her by some medical man for a presumed disease of the spine. She lost all power of using her legs; but she got quite fat, as, indeed, well she might, for her appetite was remarkably sharp, and she lived chiefly upon chickens; and the number of chickens she devoured was incredible. She lived at some little distance from town, and at last Sir Benjamin Brodie was sent for to her. Now Sir Benjamin, to use a vulgar phrase, is *up* to these cases; and he wished to see her *try* to walk: but she declared that the attempt to do so would kill her. He was resolute, however, and had her got out of bed; and in a few days time she was walking about quite well, and very grateful to him for his judicious conduct. A medical man of less name, or of less determination, would probably have failed. Dr. Bright has a good example of a somewhat similar kind; shewing the power of another form of influence. He was asked to see a young lady who had been confined to her bed for nine months. If she attempted to move she was thrown into a paroxysm of agitation, and of excruciating agony, affecting more particularly her abdomen. She had almost lost the use of the lower extremities; and she and her friends seemed to have given up all hope of her restoration. But she presented no appearance of important disease; her countenance bore no marks of visceral mischief; nor was it possible to discover any proof of

organic change. Dr. Bright set the case down in his own mind as one of hysteria. She seemed to have derived relief from some stimulating injection, and from certain pills. As her friends were in moderate circumstances, Dr. Bright talked seriously to the mother, and recommended that simple water should be employed for the injection, and that bread pills should be substituted for those she had been taking. The mother soon perceived that these means produced the same tranquillizing effects on her daughter which had hitherto been ascribed to the medicine. "My visits," he says, "became less frequent; I was absent a fortnight; on my renewing my visit, no change had taken place. I attempted to get her shifted gently from the bed to the sofa, but it was impossible; the paroxysm almost overcame her. Once (after having attended altogether about nine months) I called after an absence of nearly a month; her sister met me at the street-door with a smiling face to tell me that our patient was quite well: and on inquiry, she related how, three mornings before, under a *deep religious impression*, she had completely recovered all her powers; and I found her sitting up, working and amusing herself as if she were completely convalescent from some ordinary illness."

These are the cases which suit the purposes of miracle-mongers. A few years ago all the journals belonging to a certain party in the religious world were full of an instance of miraculous cure. The patient was a young woman; her legs had been paralytic, or contracted, I forget which; some enthusiastic preacher had influence enough with her to make her *believe* that if on a certain day she prayed for recovery with a strong faith, her prayer would be successful, she would recover at once; and she did so. No one can doubt that it was just such a case as those I have now been mentioning. *Many* of these pseudo-diseases terminate suddenly under some strong moral emotion. A fall—a fire in the house—any overwhelming terror, will sometimes put an end to them. And where the joints have been the parts affected, several patients have declared that they felt a sensation as if something had snapped or given way in the part, immediately before the sudden recovery took place.

Some of the shapes assumed by this pathological Proteus are hideous and disgusting. Paralysis of the muscular fibres of the bladder, or spasm of its sphincter, sometimes really occurs, sometimes is only aped, in hysteria. It is a common trick with these patients to pretend that they labour under *retention of urine*; and that, although the bladder is full, they cannot make water. The daily introduction of the catheter by a dresser or apprentice appears to gratify their morbid and prurient feelings. Sometimes,

no doubt, the difficulty is real; but it is oftener feigned or exaggerated. I have again and again known it disappear upon the patient's being left, without pity, to her own resources. But girls have been known to drink their urine, in order to conceal the fact of their having been obliged and able to void it. The state of mind evinced by many of these hysterical young persons is such as to entitle them to our deepest commiseration. The deceptive appearances displayed in the bodily functions and feelings find their counterpart in the mental. The patients are deceitful, perverse, and obstinate; practising, or attempting to practise, the most aimless and unnatural impositions. They will produce fragments of common gravel, and assert that these were voided with the urine: or they will secrete cinders and stones in the vagina, and pretend to be suffering under some calculous disease. A young woman contrived, in one of our hospitals, to make the surgeons believe that she had *stone in the bladder*; and she actually submitted to be placed upon the operating table, and to be tied up in the posture for lithotomy, before a theatre-full of students; and then the imposture was detected. Sometimes they simulate *suppression of urine*, and after swallowing what they have passed, vomit it up again, to induce the belief that the secretion has taken place through a new and unnatural channel.

It is impossible, I say, not to pity the unhappy victims of this wretched disorder, when their morbid propensities drive them to such acts as these. I mention them because you must expect to meet with such cases; and because, while you take care not to express your suspicions prematurely, or on light evidence, you should be on your guard against the mortification of being deceived, by the false signals held out, into active and ill-directed measures of treatment.

There is another very common hysterical *pain* which I ought to have mentioned, viz. a pain occupying some one point in the head; the patient speaks of it as a sensation like that which would be caused by driving a nail into the part; and the affection has therefore been called the *clavus hystericus*. It is often situated just above one eyebrow; and it sometimes comes on every day, at the same hour. Now in these cases it imitates very closely the hemicrania, which constitutes no uncommon form of an intermittent, and is called, accordingly, the *brow ague*. The distinction between the two—whether the affection, I mean, be hysterical or aguish—is not of any great consequence: but in many of the former examples of hysterical pain mimicking organic or inflammatory disease, the diagnosis is obviously of the greatest moment.

How, then, is it to be made? You may generally, I believe, be led to a right judgment if you look to the several points that I have incidentally touched already. You may guess that the affection is hysterical if the patient be a young unmarried woman; if there be any disorder or irregularity in the uterine functions; if you can trace any history of previous hysterical disease; and especially if she is subject to *fits* of hysteria. The suspicious symptoms may often be traced back, and found to spread themselves over a considerable previous period of time; yet there is no such wasting or commensurate deterioration of the general health and strength as might be expected in organic disease. When the complaint simulated is some acute local inflammation, and there is pain increased upon pressing the part, you will find that the pain is aggravated by the gentlest touch; it is more felt if you brush your hand over the surface, or slightly pinch the integuments, than when firm pressure is made: and you will find also that this exquisite tenderness is not limited to the part complained of. Suppose it is the abdomen, the patient will shrink and exclaim if you suddenly put your finger on her neck, or her arm. The suspicion that the disorder is nervous or hysterical will also be corroborated if the symptoms which resemble the symptoms of inflammation arise and subside rapidly, without obvious cause for such fluctuation; and if various organs appear to be attacked in succession. Between the several symptoms that mark real disease there is always (as we learn by experience) a certain congruity and relation; but in the simulative displays of hysteria the symptoms are apt to be irregular, inconsistent, contradictory. When, after the most careful investigation of the case, you still doubt, it will be right either to pause, or to treat it upon the most unfavourable supposition. The consequences of suffering active inflammation to go on unchecked would be far worse than the temporary and slight and remediable injury to the system which might result from once applying the remedies of inflammation to a case of mere hysteria. There is another hazard also which you must be aware of, and seek to avoid; that of overlooking real disease when it is mixed with, and masked by, hysterical symptoms. It is not easy to lay down positive rules of action for all these supposable cases; but I trust that I have said enough to convince you of the importance of making the diagnosis of hysterical complaints a careful object of your future study.

I have hitherto spoken of hysteria as if it were exclusively a malady of females. Etymologically, to apply that term to the diseases of males would be absurd. But

that peculiar modification of the nervous system which is observed in hysteric girls does certainly present itself, though rarely, in young men. I have seen two or three instances of what I could give no other name to than hysteria, in males. One of them was in the person of a young surgeon who had been house-surgeon to the Middlesex Hospital. I believe he applied to not less than a dozen medical men for advice; and in that batch I happened to have my turn. He had some of the symptoms that are ascribed to hypochondriasis; *i. e.* he was exceedingly attentive to his own sensations, and fancied he had a number of diseases which had no existence but in his own imagination: he showed great unsteadiness and infirmity of purpose; was what is called "very nervous;" and had occasional bursts of choking, and tears, and laughter, exactly resembling those which we so often witness in the other sex. Many cases of hysteria in the male have been recorded by different writers. The same moveable state of the nervous system, and the same symptoms referable to that system, may exist in both sexes. In females, in nine cases out of ten, or in a much larger proportion, the exciting cause of the hysteria is connected with the sexual functions; and that is all that can be meant when it is asserted that for the female the complaint is not badly named, but has an intimate dependance upon the uterine sympathies. At the same time it is quite true that the "uterus is not the only organ of which the irritation may so affect the nervous system as to produce hysteria."

As in epilepsy, so also in hysteria, the treatment to be adopted regards, first, the paroxysm itself; secondly, the condition of the patient during the absence of the paroxysm.

One object, during the paroxysm, is to prevent the patient from injuring herself, by her hands, or by her teeth, or in her convulsive movements. Her dress should be loosened; but it may be necessary to confine her hands and arms. The next thing to be aimed at is the putting an end to the fit. Various measures are found more or less useful for that purpose. The patient should be surrounded, as far as that is possible, with cool fresh air. If she is able to swallow, you may sometimes shorten the attack by administering a couple of ounces of the *mistura assafoetidæ*; or half a drachm of æther, with fifteen or twenty minims of laudanum, in camphor julep; or a draught containing half a drachm of the tincture of valerian. When the patient cannot or will not swallow, she may sometimes be brought about by stimulating volatile substances offered to the nostrils. Signal good may also be effected by foetid or stimulant enemata: the enema *assafoetidæ*, for example,

made by mixing two drachms of *assafoetida* with half a pint of water, by means of the yolk of an egg; or the turpentine injection, made in the same manner, and containing half an ounce of turpentine: or the same quantity of ice-cold water thrown into the rectum, or applied to the pudenda, will often bring the fit to a speedy termination. Indeed I believe there is more virtue in cold water, in hysterical diseases, than in any other single remedy. In the paroxysm it may be freely and repeatedly sprinkled, or dashed with some force, upon the face and chest. Active purges are beneficial and requisite in almost all these cases. There is commonly a costive, sometimes an obstinate, and always an unnatural, state of the bowels.

In those long paroxysms—if they may be so called—in which some other disease is simulated by hysteria, the cold affusion is a most valuable resource: especially in those forms of the disorder in which a limb is permanently bent, or incapable of motion. In several instances, in which such contraction had existed for a long time, it has yielded, in the Middlesex Hospital, to a few minutes' application of the cold *douche*. Mr. Corfe, as I stated before, takes much pains with these cases: he pours cold water from a tea-kettle, or any other convenient vessel, in a small stream, from a moderate height, upon the contracted limb: it has been bent up for weeks perhaps; no power that you are able to exert can extend it, and any *very forcible* attempts to straighten it give the patient extreme pain. After the stream of water has been kept up for a short time, the patient complains of it very much; but Mr. Corfe is inflexible—more so than the culprit limb—he goes on; presently the limb begins to tremble, the tight state of the muscles is evidently on the point of yielding, and in no long time they are entirely relaxed and manageable, and the member becomes as lithe and moveable as ever. It often happens that the state of contraction recurs; but a repetition of the *douche* has always the same good effect, and by degrees the habit is broken, and the patient set free. It requires some determination to put this expedient in practice. The patient looks upon you as a monster of cruelty; and, in private, the friends will not always allow such "rough" treatment, as they consider it. Sir Charles Clarke, who necessarily sees a great number of these cases—they are more common in the upper than the lower classes of society—is a great advocate of this ducking system. A paper of his upon the subject was read before the College of Physicians a few years ago. He recommends a "sudden and lavish" application of water to the face; or the immersion of the whole body. He describes the class of patients, in whom the hysterical

affection which is curable by that method occurs, as being generally females of a pasty complexion, fat, pale, and weak; or such as evince the ordinary signs of debility, a feeble pulse, cold extremities, and purpleness of parts distant from the centre of circulation. The age of the patients varied from ten to thirty years; in many of them menstruation was imperfect, or absent. A medical practitioner whom I met lately at a patient's house, told me he had just come from another patient, upon whom he had seen a surprising cure performed. A young lady, for many days, had been affected with trismus. She was unable to open her jaws, and therefore could neither speak nor eat. At last Sir C. Clarke was called in to see her. He presently comprehended the nature of her ailment, had her placed with her head hanging over a tub by the side of the bed; and proceeded to pour pitchers of water on her face. Before he had emptied the second the patient began to scream and complain, and to give very audible indications that she could open her mouth. I say although these patients get great relief by the treatment, they do not like it; and if they are convinced that it will be put in force, they will generally contrive not to require it. Of all the spasmodic affections, hysteria is that which is most readily propagable by what may be called moral contagion. If, in a large ward, one girl goes off in a fit, half a dozen others perhaps, all who happen to possess the hysterical diathesis, will experience a strong inclination to follow her example. But this chorus, as it were, of hysteria, is much more common in some wards than in others. A stern nurse, or a general order that the cold affusion shall at once be employed in every instance of an hysterical fit, will keep the complaint wonderfully in check: and on the other hand, great sympathy with such patients has a striking effect in encouraging the paroxysms. These facts show that the symptoms are, to a certain degree, under the patient's control: the fits are not wholly wilful; neither are they wholly unconquerable.

I have but little to say respecting the medical management of such patients in the intervals between the paroxysms. The objects to be aimed at are, to restore the nervous system to the requisite degree of stability: and to correct the disordered functions of the uterine system. Now much the same plan of treatment is applicable to both these objects; and I have spoken of the remedies that are found most beneficial for giving tone and firmness to the system, when upon the subject of epilepsy, and other nervous spasmodic ailments. The following points must be kept in view. The regulation of the bowels, which are mostly sluggish, by

aloetic aperients; the exhibition of some form or other of steel; the steady employment of the shower bath; regulated exercise, both on foot and on horseback; the avoidance of hot rooms and of late hours, both in respect to going to bed, and to rising from it; the avoidance also of strong moral emotions, of novel reading, and of all the other thousand modes of dissipation, mental and bodily, which always accompany, and abate the blessings of, a high state of civilization. Marriage often proves a cure: sometimes it does not.

The disposition to hysterical disorder may be more easily prevented than cured; but upon this point medical men are not consulted. Parents do not foresee the misery they are often laying up for their daughters by the unnatural mode of life to which they are subjected for the sake of filling them with fashionable accomplishments. I cannot close this subject, and this lecture, better than by quoting Sir Benjamin Brodie's remarks on the same point, as I find them in a little work very recently published by him, and containing many highly valuable observations and instructions in respect to *local hysterical affections*.

"You can render (he says) no more essential service to the more affluent classes of society, than by availing yourselves of every opportunity of explaining to those among them who are parents, how much the ordinary system of education tends to engender the disposition to these diseases among their female children. If you would go further, so as to make them understand in what their error consists, what they ought to do, and what they ought to leave undone, you need only point out the difference between the plans usually pursued in the bringing up of the two sexes. The boys are sent at an early age to school, where a large portion of their time is passed in taking exercise in the open air; while their sisters are confined to heated rooms, taking little exercise out of doors, and often none at all, except in a carriage. Then, for the most part, the latter spend much more of their time in actual study than the former. The mind is over educated at the expense of the physical structure: and after all, with little advantage to the mind itself: for who can doubt that the principal object of this part of education ought to be, not so much to fill the mind with knowledge, as to train it to a right exercise of its intellectual and moral faculties; or, that other things being the same, this is more easily accomplished in those whose animal functions are preserved in a healthy state, than it is in others."

LECTURES
ON THE
FUNCTIONS OF THE NERVOUS
SYSTEM.

BY W. B. CARPENTER, M.D.

LECTURE V.

Functions of the nervous system in Vertebrata. General history of opinions. Functions of the spinal system of nerves.

THE functions of the nervous system in Vertebrated animals are so complex in their nature, and our means of analysing them are so imperfect, that the inquiry is confessedly one of the greatest difficulty, and needs all the light which can be thrown upon it from external sources. The great accession to our knowledge of them, which has been made within the last few years, chiefly by the labours of Sir C. Bell and Dr. M. Hall, has so far changed the aspect of this department of physiological science, as to render it difficult for those, who were acquainted with its previous condition, to make themselves fully masters of its present state; and, on the other hand, those who now enter upon the study of it for the first time, will be perplexed, in referring to the practical treatises of physicians of no distant date, by the discrepancies which they will constantly meet with, between present and past opinions on the subject. On both these accounts, therefore, it seems desirable to preface our more detailed inquiry into the functions of the nervous system of the Vertebrata, with a brief historical sketch of the progress of opinion in regard to them.

The general fact of the connection of the nervous system with the functions of sensation and motion, has been known from the most ancient times; and it would be difficult, if not impossible, to trace the discoverer of this relation. It appears, too, that there was very early a vague idea that different parts of the structure might minister to these two functions respectively. This idea was prominently expressed by Galen, who certainly believed that the nerves of motion are *structurally* distinct from those of sensation, and that they are connected with different parts of the brain. He seems to have thought, however, that the difference of *texture* between the nervous trunks—those of motion being hard, and those of sensation soft,—was the cause of their difference of function; the soft nerves being more susceptible of impressions, and the hard ones less impressible, but stronger, and therefore better fitted for action. He maintained that a nerve which at its origin possesses one endowment may change its character in its course, by virtue of a change of texture;

and he had evidently, therefore, no idea of any *essential* difference in the character of the various parts of the central organs, or in that of the nervous trunks as derived from them. This general view of the subject made but little advance towards detailed accuracy until the 17th century, when the anatomy and functions of the nervous system were more minutely investigated by various inquirers, amongst whom our own Willis holds a prominent rank. His attention was particularly directed to the cranial nerves, the physiological actions of which he rightly guessed, in the majority of cases, from their distribution only. In fact, his knowledge of these has been much overlooked, in consequence of the adoption of the numerical nomenclature which he introduced. The attention of Willis, in common with other physiologists, was much directed towards the involuntary movements, which he regarded as taking their origin in the cerebellum, whilst the voluntary movements were supposed to result from the influence of the cerebrum. In this point an advance towards truth was evidently made; but it was not until some time afterwards, that the true source of the involuntary movements was explained. Willis distinctly states that the first and second pairs of cranial nerves minister to sensation only; and that the two next are especially subservient to motion,—the third pair performing only the voluntary movements of the eye, and the fourth pair being the channel of the involuntary movements, and movements of expression, which he regarded the cerebellum as influencing. He notices the fifth pair as different from the first four, inasmuch as it ministers both to sensation and motion; but he regarded all its branches as possessed of this double endowment, and considered its motor power to be exercised in obedience both to volitional and emotional influences. He noticed, however, the special destination of the third branch to the masticator muscles; and states his belief that the sense of taste also is due to this nerve. The sixth pair he states to be a muscular nerve simply, and to have for its office the abduction of the eye for the purpose of gaining a backward view, as is done by animals under the influence of fear. The *portio mollis* of the seventh pair he regards as purely a nerve of sense; and the *portio dura* as a motor nerve, destined to bring the muscles of the face into advantageous co-operation with the auditory sense. With regard to the ninth pair, he states most distinctly that it is the nerve of the motions of articulation, whilst the fifth pair is that of the sense of taste: and that the reason of this organ being supplied with its two nerves is its double function. From this time we find that the olfactory, optic, and auditory nerves, were generally spoken of as nerves

of sensation only, (although it was usually imagined that they bestowed common as well as special sensibility on the organs to which they were respectively distributed); whilst the third, fourth, and sixth pairs were, from their exclusive connection with muscles, recognized as specially if not exclusively *motor* nerves. Still, no attempt was made to account for the difference in their respective functions, by more minute researches into their origin and their connection with different parts of the central organs.

So vague, indeed, were all the notions entertained upon this subject, that many writers seem altogether to have lost sight of this distinction; and the old doctrine, which regarded the brain as the elaborator of the "animal spirits," which were transmitted by the nerves to every part of the body, still held its place in the schools. Even Haller, who contributed so greatly to our knowledge of the true influence of the nervous system upon the organism at large, had very confused notions on the subject of the nerves themselves. He was the first to give prominent expression to the fact (for such we think it well established that it is) that the property of *contractility* is inherent in muscular fibre; and that the nervous power or influence, transmitted from the central organs, is only one of several exciting causes by which that property may be called into action. The property of contractility was termed by him the *vis insita* of the muscle; and he denominated the nervous influence the *vis nervosa*. This *vis nervosa* he speaks of as originating in the nervous centres, and as being the medium by which their influence is transmitted to the muscles. Haller was perfectly aware that for a *sensation* to be felt, an impression must be propagated along the nerves from the circumference to the centre; and that, for a muscular contraction to be produced by a nerve, an impulse must be propagated along the trunk from the centre to the circumference; and yet he maintains that the same fibres may convey either sensory or motor impulses, according to the direction in which they are transmitted. This opinion does not seem to have been opposed with any general degree of success, until Sir C. Bell anatomically and experimentally demonstrated their distinctness.

Contemporary with Haller was the celebrated Whytt, Professor of Physiology at Edinburgh. He carried on a long and important controversy with Haller on the subject of Muscular Contractility, in which he upheld the doctrine that this power is not an independent endowment of muscular fibre, but is always derived from the nervous system. Hence he attributed *all* muscular actions to the influence of the nerves;—a fact of which it is necessary to be aware, when consulting his valuable treatise on

Nervous Diseases, or when endeavouring to trace the complicated history of this department of physiological science. It is also necessary to state that, by the operation of the *sentient principle*, or of *sensation*, Dr. Whytt certainly did not mean necessarily to imply what *we* ordinarily mean by sensation, since he speaks of its operation in many instances as unattended with consciousness. The true import of the term, according to his use of it, can only be understood by remembering that Whytt was a follower of the Stahlian school, who regarded "the soul" as diffused through the whole body, and the *primum mobile* of all its actions. In his treatise on the Vital and Involuntary Movements, Whytt clearly distinguishes between the "determination of the nervous influence into the muscles," as owing "either to the power of the will, or to a stimulus." The former are of course the voluntary, and the latter the involuntary movements; and it was the chief object of this work to show, which he did most satisfactorily, that the so-called sympathetic movements are dependent upon a *stimulus* propagated to the central organs, and *there* causing the determination of the nervous influence to the muscles; in opposition to the opinion that such movements are occasioned "by the consent of parts and continuity of membranes." This stimulus he considered as ordinarily applied to the muscles themselves; but he adduced some cases in which it was applied to a different texture, as in the case of the movements of the pupil and of those of respiration, the nature of which he considered analogous, and of which he gives explanations, which are even now correct in all but subordinate details. It is quite true that he assigned as results of the same cause, some actions (as those of the heart and alimentary canal) which later physiologists have generally agreed with Haller in considering as independent of the cerebro-spinal system of nerves; but this does not detract from the general value of his reasoning, or of the conclusions to which they led him. The state of anatomical knowledge at that time did not enable him to *limit* the participation of the nervous centres in the actions of this class to the spinal cord (including the medulla oblongata) as we can now do; but he was well aware of the independent power of the spinal cord in affording the required supply of the nervous influence, when separated from the brain. In the general views just stated, Whytt was followed by Cullen, who expressed them very clearly, and distinctly stated the non-dependence of the greater part of the motions thus excited, upon sensation, as upon volition.

During the latter part of the 18th century many important contributions were made to the anatomy and physiology of the

nervous system. Dr. *Monro primus* pointed out that the ganglia on the spinal nerves are formed upon their posterior roots alone; and other anatomists, especially *Paletta*, made the same distinction in regard to the two roots of the fifth pair, remarking further that the fibres of the small root pass entirely into the third division of it, and are distributed to the muscles alone. Still no advance was made towards the general demonstration of the anatomical distinctness of the motor and sensory fibres; for those who examined the anatomical relations of the nerves with the greatest minuteness were ignorant of the physiological facts they revealed; or, at most, they derived from them one or two inferences which had only a very limited application. On the other hand, those who held the notion that the sensory and motor nerves are distinct, were content with the mere speculation, and did not attempt to support it either by the evidence of anatomical relation, or by that with which experiment might have supplied them.

About the same period *Prochaska* gave an exposition of the nature of the sympathetic or excited movements, which, considering the state of neurological knowledge at the time he wrote, must be allowed to possess great precision. But, as in the case of *Whytt*, it is necessary to take his own explanation of the meaning of the terms he uses, in order to avoid being misled by the import which we, at the present time, are accustomed to attach to them. Thus in modern writings the term *sensorium commune* is used to express the locality at which impressions, by affecting the conscious mind, become sensations; but by *Prochaska* it is defined to be that part of the nervous centres at which external impressions, conducted to it by afferent nerves, give rise to certain and determinate motions, through respondent motor nerves. This centre of reflexion he expressly limits to the spinal cord and its prolongations into the brain; adverting to experiments on decapitated animals, as proving the independent power of this organ. He further remarks that this reflexion may take place, whether the mind be conscious or unconscious of it; and adverts to the movements observed in profound sleep, in apoplectic patients, and in decapitated animals, in support of this position. The laws governing these movements he states to be the preservation of the body from external injury, by the production of motions in response to external impressions, tending to ward off and remove the source of injury; and also the conservation of the body, by the reflexion of external impressions into motions for its benefit. It is quite true that, like *Whytt*, he seems to have attributed the movements of the heart and alimentary canal to reflex action; and he

has used them in illustration of his position. But he does not speak with certainty upon this point, and puts it as a query whether these actions are not the result of impressions reflected through the visceral ganglia. One or two erroneous illustrations, however, by no means prove that he did not understand the principles he was advocating; since they show no more than that, in these particular instances, he was in error as to their application. The opinion that the motions of the heart and alimentary canal do not proceed from the immediate stimulation of their contractile fibres, but are dependent upon a reflex action of the nervous system, finds many supporters at the present time; and it might be held without prejudice to the most perfect comprehension of the nature of reflex action in other cases. No reader of *Prochaska's* works can avoid the conclusion that he entertained the speculative opinion, that the nervous fibres conducting impressions to the central organs, and transmitting motor impulses from them, were distinct; but he does not seem to have had the slightest idea of a system of fibres terminating in, and originating from, the spinal cord, separate from those which communicate with the brain. The idea of such a system is unquestionably due to Dr. M. Hall.

During the period of which the history has just been sketched, a very important change took place in the views of physiologists in regard to the sympathetic, ganglionic, or more truly the *visceral* system of nerves. This had been formerly described as a sort of offset from the cerebro-spinal, descending from its origin in the fifth and sixth cerebral nerves, and deriving reinforcements from the spinal nerves. Previously, however, to the time of *Bichat*, juster views of its nature were prevalent; and its title to the character of a distinct system was generally recognized. Availing himself of this, and of a certain superficial analogy between the chain of ganglia it presents, and that which constitutes the central apparatus in the Articulated classes, he propounded his specious view of its functions as the nervous system of organic life. To establish its exclusive connection with the organic functions, was a grand, and probably correct generalisation; and it may be regarded as the first of three important eras in neurological research, each of which has been marked by the restriction of a particular group of functional changes to a special division of the nervous system. It is questionable, however, whether the influence of the visceral nerves upon the organic functions is of the nature or extent that *Bichat* supposed; and it is undoubted that the sensory and motor properties which the sympathetic nerve has been ascertained to possess, are derived from its connections with the cerebro-spinal system. Still there is good

reason to believe that, although not individually dependent upon such a system as Bichat imagined to exist, the organic functions are harmonized and regulated by it. We may for the present put aside the consideration of it, and pursue the train of enquiry into the functions of the several parts of the cerebro-spinal system; the most successful labourers in which have been Sir C. Bell, and Dr. Marshall Hall.

There can be little doubt that, when Sir C. Bell and Dr. M. Hall commenced their respective investigations, the results of which have been so triumphantly successful, they were, in a great degree, ignorant of what had previously been taught on the subject. But that *they* were ignorant only shows the *general* want of information on the subject; and their merit as original discoverers remains unaltered. Had Sir C. Bell, however, been better acquainted with the minute anatomy of the fifth nerve, he would have been prevented from falling into an error which most seriously defaced his early results; and had Dr. M. Hall been more fully acquainted with the writings of Whytt (which have been continually made the subject of professional exposition in Edinburgh, from his own time to the present), not to mention those of Prochaska, he would have found that there was much in his early doctrines regarding the reflex function, in which he had been anticipated; and he would, by claiming less, have probably excited less opposition to his views among those whose historical acquaintance with the subject gave them better means of separating what was only clothed in a different dress, from what was really novel.

Sir C. Bell's chief merit as an original enquirer consists in directing attention to the *roots of the nerves*, as a part of the whole nervous system most adapted to afford information to the experimenter. He was led, by the study of the complex anatomical distribution of the nerves, especially in the head, to enquire whether a reason might not be found for it in the different endowments of the different trunks; and whether two separate portions also of the same trunk, connected with different parts of the central system, might not be proved to have different functions. The double roots of the spinal nerves appeared to him to afford the most advantageous opportunity for the solution of this question; and accordingly his first experiments were performed upon them. The results of these were not, however, decisive; and they were modified, in Sir C. Bell's own mind, by the anatomical error of supposing that the anterior strands of the cord terminate in the cerebrum, which is the grand organ connecting the mind with the body, receiving nerves of sensation, and giving off those of voluntary motion; whilst he re-

garded the posterior strands as terminating in the cerebellum, which he regarded as having for its office to control the movements concerned in the maintenance of the vital functions. The results of Sir C. Bell's first experiments were simply that the posterior fasciculus of roots could be cut across, without convulsing the muscles of the back; but that the muscles of the back were convulsed immediately that the anterior fasciculus was cut with the knife. Misled by his erroneous anatomy, he appears at first to have regarded the anterior roots as ministering both to sensation and motion; and to have been left in uncertainty as to the precise function of the posterior.

Impressed with the idea, however, that the participation of the roots of the spinal nerves in the sensorial functions was not likely ever to be satisfactorily determined by experiment, on account of the severe nature of the operation requisite to expose them, Sir C. Bell abandoned this mode of enquiry for another, in which he could more advantageously apply the same principle. He had early ascribed to the irregularly distributed cranial nerves a difference of function corresponding to that which he endeavoured to establish in the two roots of the spinal nerves. Having obtained indications that the motor powers of the latter were principally, if not entirely, inherent in its anterior root, he traced the anterior fasciculus of the cord upwards to the crus cerebri; and finding that the ninth, sixth, and third nerves arise by single roots from this tract, he was led to the belief that it was not only peculiarly but exclusively motor, since these were well known to be entirely muscular nerves. The inference then presented itself that the posterior column and the posterior roots were for sensibility; and this, again, was opposed by the exclusive connection of the intervertebral ganglia with these,—a notion being then generally prevalent that the function of these ganglia was to cut off sensation. Some very decided experiments were necessary to overturn this dogma; and hence arose the enquiries into the functions of the fifth pair, and of the portio dura of the seventh, which were attended with such important results. Sir C. Bell's first conclusions on both these points were, however, by no means free from error. Prominently impressed with the idea that the fifth pair is altogether analogous to the spinal nerves, possessing a double endowment in virtue of its double root, he fell into the mistake of attributing to all its branches this double endowment, being ignorant of the fact, with which many previous anatomists were familiar, that the small root, which arises from the motor tract, passes into the third division alone. He early satisfied himself, however, that the seventh pair has no participation in sensa-

tion; and, comparing *its* single aganglionic root with the double root of the fifth pair, he came to the conclusion that the ganglionic root of the latter was the channel of its sensory endowments; and he virtually extended this conclusion to the corresponding roots of the spinal nerves, before any other physiologist had definitively assigned their function.

Having been at first misled in supposing that the fifth pair was the general motor as well as the sensory nerve of the face, he was also brought to a wrong conclusion in regard to the functions of the portio dura; which he regarded as having no concern with the voluntary movements, but as subservient *only* to the respiratory actions, and the movements of expression, or emotional actions. In the first of these errors he was speedily corrected by Magendie, who showed that the two superior branches of the fifth pair do not possess any motor endowments; and Mr. Mayo showed that the portio dura serves as the channel of voluntary movement. Neither of these physiologists, however, showed that they had correct notions of the relative functions of these two nerves; and it is to Sir C. Bell and his relative, Mr. J. Shaw, that we owe the complete elucidation of them, which was obtained not only by experiment, but by observation of cases of disease. It was thus shown that, whilst the fifth pair is the exclusive nerve of common sensation by which the face is supplied, its motor endowments are restricted to the actions of mastication; and that the seventh pair is the channel through which are excited the ordinary movements of the facial muscles, as well as those concerned in the expression of emotions, and in the functions of respiration, with the actions dependent upon it. The consideration of the peculiar importance of this function, which was forced upon Sir C. Bell in the course of his enquiries, and the obvious connection of the greater part of the nerves concerned in it with the medulla oblongata, led him to consider that the respiratory nerves form a distinct group, which have nothing in common with the ordinary sensorivolitional fibres, except being occasionally bound up in the same trunks with them. This view appeared to derive support, not only from the anatomical distribution of these nerves, but from pathological cases, in which the voluntary power over the respiratory muscles was lost without their regular movements being disturbed, and *vice versa*. As all the respiratory nerves do not terminate at once in the medulla oblongata, Sir C. Bell was obliged to assume the existence in the spinal cord of a peculiar respiratory tract, from which the intercostal and other respiratory nerves originated; and this tract he considered to be as distinct from the sensory and motor columns as these are from each

other. This doctrine was strenuously resisted, however, by those physiologists, who were not so far dazzled by the brilliancy of Sir C. Bell's other discoveries, as to admit readily whatever he should advance; and it was particularly opposed by Dr. Alison, who, in a valuable paper on Sympathetic Actions (published in 1826), gave an exposition of Whytt's doctrines, in regard to the involuntary movements, as modified and rendered more precise by the discoveries of Sir C. Bell in regard to the distinctness between the afferent and efferent nerves: he did not, however, go so far as to admit that sensation was not essentially concerned in them; but distinctly propounds it as a general fact, that the sympathetic actions are excited by sensations whose influence is reflected downwards to parts frequently distant from those on which the stimulating impression is made. This appears to have been Sir C. Bell's opinion concerning the respiratory movements; for he speaks of their being under the guidance of a sensibility more certain in its effects than the will. It is not, therefore, correct to represent him as maintaining that the medulla oblongata is the primum mobile of the respiratory actions, since no sensibility can operate, except through an impression conveyed by the afferent nerves to the central organs. But Sir C. Bell's error consisted in singling out the respiratory movements as the only actions performed by a group of nerves to a certain extent distinct from the rest. "He seems to have forgotten," it has been well remarked by Dr. J. Reid, "that there are other extensive associated and sympathetic movements of the muscles of the body besides those which he has so beautifully illustrated; for it is obvious that, if a particular tract in the spinal cord be necessary to carry on the respiratory movements, there ought also to have been a defecatory tract, a urinary tract, and so on, to carry on the other sympathetic movements, in which a number of distant muscles are engaged in simultaneous action." If Sir C. Bell had directed his attention to the Invertebrated animals, in which the centres of nervous action are much more scattered than in Vertebrata, he would have found that not only the respiratory apparatus has a source of action distinct from the sensorivolitional system, but that the apparatus of deglutition, that of locomotion, and sometimes others, have distinct centres, which are all combined in the spinal cord of Vertebrata. He might thus have advanced from particulars to generals, until he had arrived at the same conclusion in regard to the distinct character of the spinal cord as a centre of nervous action, and of the nerves connected with it, as that which Dr. M. Hall has attained by a contrary course of reasoning. But being led by

the evident importance of the continual maintenance of the respiratory actions, to fix upon them an exclusive attention, he advanced no further in this branch of the inquiry.

To Sir C. Bell, then, belongs the exclusive merit of originating the mode of investigation which has led to the positive knowledge of the structural distinctness of the afferent and efferent nerves, which may be considered as the second great era in the history of Neurology; an era more important than the first, both in the more complete nature of the discovery itself, its greater practical utility, and its more extensive bearing upon the general doctrines of nervous action.

During the progress of Sir C. Bell's inquiries, however, other investigations were being prosecuted upon a different branch of the subject,—that which relates to the character of the spinal cord as a distinct centre of nervous action, and the exclusive source of those sympathetic actions which had before been vaguely attributed to the brain as well as to it. Many physiologists were disposed to regard the spinal cord as a mere bundle of nerves, whose action depended on the brain; but Legallois proved that it has a large amount of independent power; for that on it alone the maintenance of the regular movements of respiration depended; and that it might be divided into several parts without necessarily putting a check on the functions of the nerves connected with each division. Of these functions, however, he formed an erroneous estimate; for he considered that the movements which might be excited in response to a stimulus in any segment of the body thus isolated from the rest, indicated the persistence of sensation and volition in the division of the spinal cord with which it was connected. A different conclusion had been previously drawn, however, by Sir G. Blane, from this and other facts: he found that, when the spinal cord of a rabbit was divided below the last dorsal vertebræ, the posterior extremities were agitated when the tail or one of the feet was compressed; and, connecting such results with observations made upon anencephalous infants, he had come to the conclusion that the automatic movements are not dependent upon sensation or consciousness. In this and several other views he closely corresponds with Prochaska; but he entertained more correct opinions than that writer on the subject of the action of the heart and alimentary canal, which he regarded as due to the direct application of stimuli to the muscular fibre itself, independently of nervous agency. With these views, however, Legallois does not seem to have been acquainted; for he maintains that the action of the heart, no less than the respi-

ratory movements, is dependent upon the spinal cord. However, he made a real advance in the inquiry, by clearly proving the independent power of the spinal cord, which was quite overlooked, except so far as the respiratory system was concerned, by Sir C. Bell. Legallois was followed in the same course of inquiry by Flourens, who had been preceded in his researches by Rolando; but the proof afforded by Sir C. Bell, of the anatomical distinctness of the motor and sensory fibres in the nervous trunks, now gave to that, which had been previously supported on other grounds, more of the character of a positive fact. Flourens applied to that property of the spinal cord by which it is enabled to give rise to motions in response to stimulating impressions, the name of *excitability*; and this he clearly separated from its function as the channel of sensation and of voluntary action. Being himself unaware, however, of the anatomical distinctness of the sensory and motor nerves, he could not express his meaning so clearly as any one who coupled Bell's researches with his own could scarcely avoid doing. In maintaining that the spinal cord has properties of its own, distinct from sensation and volition, and operating on the muscular system when stimulated by impressions conveyed to it from without, Flourens may be said to have given a distinct enumeration of what has since been termed the Reflex Function of the Spinal Cord; but being unacquainted, as just stated, with the anatomical distinctness of the afferent and efferent nerves, still less could he have had any idea of the distinctness of the nerves, through which the excitability of the spinal cord was aroused and carried into operation, from those which minister to the functions of the brain. This idea, which has been developed into one of the happiest generalizations of modern physiology, entirely originated with Dr. M. Hall.

The nature and results of this gentleman's inquiries may, perhaps, be best understood by considering them historically. He commences his first Memoir by referring to those of Legallois and Flourens; also to the opinion maintained by Cruveilhier, that the idea of the distinct or independent character of the spinal cord is a notable error in physiology; and to the question put forth by Cuvier, in his Report on the work of Flourens, whether the results obtained by experiments are equally applicable to entire animals, and to those deprived of their encephalon. He then continues:—"In the entire animal, sensation and voluntary motion, functions of the cerebrum, combine with the functions of the medulla oblongata and medulla spinalis, and may, therefore, render it difficult or impossible to determine those

which are peculiar to each. If, in an animal deprived of the brain, the spinal marrow, or the nerves supplying the muscles, be stimulated, those muscles, whether voluntary or respiratory, are equally thrown into contraction, and, it may be added, equally in the complete and in the mutilated animal; and, in the case of the nerves, equally in limbs connected with and detached from the spinal marrow. The operation of all these various causes of muscular contraction may be designated *centric*, as taking place *at*, or at least in a direction *from*, central parts of the nervous system. But there is another function, the phenomena of which are of a totally different order, and obey totally different laws, being excited by causes in a situation which is *eccentric* in the nervous system; that is, distant from the nervous centres. This mode of action has not, I think, been hitherto distinctly understood by physiologists." In this last statement Dr. Hall was certainly not borne out by the historical facts already stated; for it has been shewn that, from the time of Whytt, there have always been physiologists who maintained precisely this very doctrine, though not in precisely the same terms. The general fact, however, that many actions concerned in the maintenance of the vital functions, and in the preservation of the body from danger, are excited by "impressions made upon the extremities of certain nerves, conveyed to the medulla oblongata and medulla spinalis, and reflected along other nerves to parts adjacent to, or remote from, that which has received the impression," had been pointedly stated. Dr. Hall, however, directing his special attention to the applications of this fact, made it apparent that they are much more extensive and important than had been previously supposed. He shewed, in the first place, that this reflex function "presides over the orifices and terminations of each of the internal canals in the animal economy, giving them their due form and action." The action of deglutition, occurring when the substance to be swallowed is carried by volitional movements of the tongue and jaws as far as the fauces; the usually open state of the glottis, and its closure upon the application of an irritant; the closed state of the sphincters, and many other actions, were shewn to be dependent upon the continuity of the afferent and efferent nerves connecting these organs with the spinal cord, and upon the integrity of the segment of the cord between their points of connection with it. If the circle be interrupted at any point, whether by dividing the trunks, or by breaking down the spinal cord, the power of exhibiting reflex actions is altogether destroyed. It is remarkable, however, that Dr. M. Hall should have so completely

put the respiratory movements beyond the pale of his generalization. He constantly speaks of them, in this first Memoir, as having a source distinct alike from volition and from reflex action—originating in the medulla oblongata, as voluntary movements originate in the cerebrum. In this he was regardless of the generalization of Whytt, who had justly comprehended them in this class of sympathetic movements; and with this generalization, as well as with other doctrines which had been at different times propounded on the subject, it may be surmised, from the total absence of all reference to them, that Dr. M. Hall was at that time unacquainted.

A considerable part of the first Memoir is occupied with facts and arguments supporting the view that the reflex movements arise from a principle essentially different from sensation as well as from volition. That sensation is not a necessary participator in them was maintained (as we have seen) by Whytt and his followers; it was more strongly urged by Sir G. Blane and Prochaska; and its complete want of connection with them was stated by Flourens. Still the *proof* of this assertion was incomplete; and many physiologists, among whom were Cuvier and his follow-reporters to the Institute on the merits of Flourens' researches, regarded the very performance of these movements as indications of the persistence of sensation; and at a later period Dr. Alison more positively stated, that the spinal cord and medulla oblongata afforded all the conditions necessary for the reception of sensations and the excitement of motions consequent on them. Hence more evidence was needed to substantiate this position. Dr. Hall principally rests upon the results afforded by experiments upon the lower animal. On dividing the spinal marrow of a snake, between the second and third cervical vertebræ, the voluntary movements of the animal, which had previously been active and vigorous, immediately ceased; and it lay perfectly quiescent, except when stimuli were applied. When stimulated, "the body began to move with great activity, and continued to do so for a considerable time, each change of position or situation bringing some fresh part of the surface of the animal into contact with the table or other objects, and renewing the application of the stimulus. At length the animal becomes quiescent; but the slightest touch with a hard substance, the slightest stimulus, will renew the movements in an active form." "Now that this phenomenon," continues Dr. Hall, "does not depend upon sensation, is further fully proved by the facts, that the position last assumed, and the stimuli applied, may be such as would be attended by extreme or

continued pain, if the sensibility were undestroyed: in one case the animal remained partially suspended over the acute edge of the table; in others, the previous infliction of punctures, and the application of a lighted taper, did not prevent the animal, still possessed of active powers of motion, from passing into a state of complete and permanent quiescence." This experiment, which harmonizes completely with the facts stated in the third lecture in regard to the movements exhibited by decapitated insects, cannot be said to *prove* the absence of sensation, however probable such a conclusion may be. Unless animals, which are the subjects of such experiments, can *tell* us that they do not feel the application of stimuli, the absence of sensation cannot be determinately stated. Indeed, the very character of the reflex movements they exhibit has been urged by some physiologists as an argument in favour of these being performed in response to sensation: a frog, for example, whose spinal cord has been divided in the middle of the back, will move its hind-legs in such a manner as to push away a probe which is irritating the cloaca. The only evidence, then, of a perfectly satisfactory nature, is that derived from observation of the results of those natural experiments which are performed by disease in the human being. This now affords ample testimony to the truth of Dr. Hall's conclusions; but at the time these were first propounded, they could not be said to rest upon any basis much more satisfactory than that which Sir G. Blane, Prochaska, and Flourens, had established.

The *tone* of the muscular system, by which the balance is maintained amongst its opposing parts, when none of them are peculiarly called into action, had been generally regarded by previous physiologists as due to a property of the muscles themselves. Dr. Hall, however, distinctly proved in this Memoir that it results from the reflex action of the spinal cord; being retained whilst the muscles are paralysed to the influence of the will, so long as their circle of communication with the spinal cord is uninterrupted; but destroyed immediately that the spinal cord is destroyed, or its connection with them severed. That the "constant contraction of the sphincters, and the tension of such muscles as are balanced by antagonists," was dependent upon the influence of the central organs of the nervous system, had been the opinion of Whytt; but the opinion had been forgotten, and it was brought forward in a new and striking form by Dr. M. Hall, to whom the limitation of this influence to the spinal cord, and the explanation of its mode of operation, most unquestionably belongs.

The next point urged by Dr. Hall, in his

first Memoir, is that the reflex function of the spinal cord admits of exaltation or diminution; and that a considerable number of nervous diseases may thus be explained. If, for example, a frog be made tetanic by swallowing a solution of opium or strychnine, the phenomena are seen to be those of augmented reflex action; the slightest stimulus producing violent movements; but all capability of the performance of these movements is destroyed by removal of the medulla spinalis. On the other hand, by hydrocyanic acid, the excitability (to use the expression of Flourens) of the spinal cord is diminished, the reflex actions are seen to be less energetic, and they speedily disappear altogether.

The observation of these and other phenomena of reflex action led Dr. Hall to the study of the pathological phenomena connected with it. He commences by observing that it appears to "reveal and explain, in a manner totally new, a series of facts in pathology; and to lead to a new division of the diseases of the nervous system, coinciding with the different modes of operation of their causes, into those of *centric*, and those of *eccentric* origin." To the latter he refers the variety of morbid actions connected with dentition, which, as the reflex function is more excitable in the young animal than in the adult, are more severe in the first than in the second dentition. In this case, a stimulant conveyed by an afferent nerve to the spinal cord excites reflex actions in various muscles through the respondent motor nerves.

In epilepsy and chorea, again, the spasmodic actions may result from a cause operating at a distance from the central organs, or by a direct affection of the spinal cord itself, in which latter case the disease is spoken of by Dr. Hall as of *centric* origin. Asthma, tenesmus, strangury, tetanus, and hydrophobia, are in like manner shown, by Dr. Hall, to be disordered conditions of reflex action, arising either from the application of unusual stimuli, or from an undue excitability of the nervous instruments. On this it is to be remarked, that those who had made nervous diseases their study, were previously well aware of the general fact, that spasmodic actions might result from unusual stimuli acting through the nervous centres, though applied at a distance from them (as in the case of traumatic tetanus), or from a morbid condition of the nervous centres themselves; although the fact had not been expressed in the same words. It was further suspected that the channel of the operation of such stimuli was the same as that which is subservient to the ordinary sympathetic movements; since it was frequently evident that spasms were no more than the contractions ordinarily concerned in

the maintenance and regulation of the vital functions, in a state of undue and irregular excitement. On this point, then, Dr. Hall must be considered to have spoken too strongly, when he asserts that neither the reflex function itself, nor the pathological conditions dependent upon it, were previously known to physiologists.

It would be very unjust towards Dr. Hall, however, to assert that this first Memoir did not constitute a very important accession to the literature of physiological science. It cannot be questioned that the principle of reflex action was here shown to have an extensive and important concern in the maintenance of the regular functions of the animal body, which had not been previously suspected; that its distinctness from other modes of nervous operation had never before been so clearly explained; that its seat had never before been so accurately limited; and that its pathological relations had never before been so philosophically sketched. In fact, no single physiologist had previously endeavoured to combine so many phenomena under one generalization; and the result of the prominent attention which he fixed upon it was soon apparent, in the accumulation of important facts which poured in upon him, and in the really novel and striking doctrines which he was soon able to found upon them. These were the subjects of his Second Memoir, read before the Royal Society in the spring of 1837, but not published in its Transactions. That it should have been excluded from them must now be a source of regret to those concerned in the publication, since this memoir may in future years be looked to as taking rank with those of Sir C. Bell, which have been justly regarded as an honour to its volumes. Diminished as it is in value by the imperfect account given of preceding opinions, in what professed to be a complete history of them, and deficient as it frequently is in proofs of the new doctrines advanced, it must be regarded as having in a remarkable degree anticipated proofs which were subsequently offered, and as containing all the fundamental principles which have been subsequently upreared into a stable and harmonious edifice.

The physiology of the spinal system of nerves, which will form the subject of the succeeding lecture, will be treated of entirely in accordance with the *essentials* of Dr. Hall's doctrine; but his own researches will be the less expressly referred to, as having been sufficiently noticed in the present historical retrospect.*

* These Lectures are resumed from page 63, and will hereafter be continued regularly till their conclusion.

AN ACCOUNT OF A NEW PLESSER AND PLESSIMETER :

ALSO, REMARKS ON THE RELATIVE VALUE
OF THE FLEXIBLE AND OTHER STETHO-
SCOPES.

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THE employment of auscultation and percussion in the investigation of diseases of the chest is no longer a question. Experience has established both the utility and necessity of these aids in diagnosis. It therefore only remains for us to perfect the methods of practising auscultation and percussion, either by improving those already in use, or by inventing others more suitable.

The object of the present communication is to introduce to the notice of the profession two instruments which I have employed for upwards of twelve months for the purpose of percussion; and, also, to speak of the suitability and convenience of the flexible acoustic tube for the purpose of auscultation. This tube I have used at the Westminster Hospital for some years past.

Percussion, like auscultation, may be employed immediately with the fingers, after the manner of Avenbrugger and Laennec; or, mediately, after the manner of Piorry, by a circular piece of ivory, or other material, or by the fingers of the left hand.

Immediate percussion, after the manner of Laennec, is practised with the three first fingers of the right hand held tightly together; the ends being in the same plane, and supported by the thumb. Mediate percussion by the fingers is practised by laying the three first fingers of the left hand flat upon the chest, and striking the backs of these with the fingers of the right. Of these plans the latter is preferable, as being more agreeable to the patient. Lannec's method, as he himself observes, is difficult to practise, inasmuch as several points require to be accurately attended to. Care must be taken that the skin be equally stretched on both sides; that the rib be not struck on one side, and the intercostal space on the opposite; that both sides be struck with equal force; that the percussion be made with the same hand,

exactly in the same position relatively to both sides; and it must be remembered that the slightest difference in the inclination of the fingers will give a widely different result; and that even with the greatest attention to these points, it is almost impossible to percuss in an equally perpendicular manner the two sides of the chest. For these reasons Laennec regards percussion as more difficult to practise well, and as demanding more care and attention, than auscultation. Laennec's method is, moreover, ineligible, on the score of giving pain to the patient, which it is apt to do unless practised delicately, and unless the nails are kept short on purpose.

Mediate percussion, by means of the fingers of the left hand, is scarcely more exact than Laennec's method; it being difficult and fatiguing, even painful, to practise on many parts of the chest: for percussion should not be limited to the antero-superior regions of the chest, but practised over every part from which sound can be elicited.

The ivory plate employed by Piorry was made trial of by myself for a considerable time, but finding it difficult to hold in many positions of the chest, and painful to strike when it was necessary to examine the whole chest, perhaps of several patients in quick succession in hospital practice, I relinquished it in favour of the circular pieces of sole leather recommended in one of the periodicals several years ago. These pieces of sole leather, about one inch and three-eighths in diameter, rounded at the edges, answer well; one piece held in the left hand being placed flat upon the chest, and struck by the other held perpendicularly, that is, edgewise, in the right hand. Sole leather is the most agreeable and best adapted medium for percussion that I have made trial of, and is adopted in the instruments I am about to recommend. Caoutchouc I have found unfit, and cork in no way desirable.

The instruments in question are two—a *plegger* and a *plessimeter*. The *plessimeter* consists of a circular piece of sole leather, full one inch and a quarter in diameter, fixed in a steel stirrup by means of screws at the ends of the cheeks; and to the head of the stirrup is attached a handle, partly of steel, partly of wood: the whole about eight inches long. The screws admit of the

free motion of the piece of leather upon its axis in the stirrup, by which means it can adapt itself to any part of the chest at any inclination.

The *plegger* resembles a hammer; the head formed of a piece of solid steel nearly cylindrical, having one end bored to admit a cylinder of sole leather, of which the extremity projects out half an inch, and is slightly conical, like the end of the fore-finger. It is with this cylinder of leather that the horizontal circular piece in the *plessimeter* is struck. See figs. 1, 2, and 3.

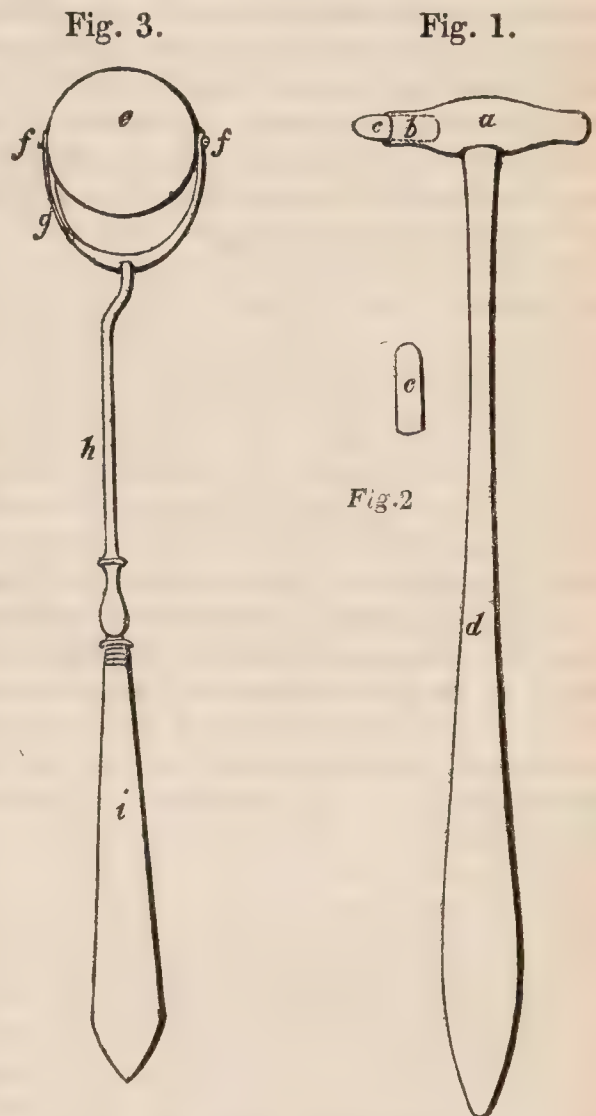


Fig. 1.—*The Plessier*.

- a. The steel head of the plessier.
- b. The dots marking the bore of the steel head to receive the cylinder of leather.
- c. The projecting end of the leather cylinder.
- d. The handle of the plessier.

Fig. 2.

The solid cylinder of leather, rounded and slightly conical at one extremity.

Fig. 3.—*The Plessimeter*.

- e. The circular piece of sole leather.
- f. Two screws through the extremities of the stirrup into the leather, forming the axis on which the circular leather turns.
- g. The steel stirrup in which the circular leather is held.
- h. The steel part of the handle.
- i. The wood part of the handle.

The instruments are made by Weiss and Son, Strand.

The method of using these instruments is simple. The plessimeter, held in the left hand, is applied to the chest with moderate pressure, care being taken that the whole surface of the circular leather is in exact and equal contact with the skin: it is then struck perpendicularly with the plessor, held in the right hand, with a force proportioned to the thickness of the integument and muscle covering the parts of the chest percussed. The essential precautions of Laennec—that the skin be equally stretched on both sides; that the rib be not struck on one side, and the intercostal space on the opposite; that both sides be struck with equal force—are easily observed; as well as the additional one, that the plessor should always fall perpendicularly upon the plessimeter.

By the aid of these instruments every region of the thorax will be percussed with facility; an exact comparison of the resonance of relative points or regions of both sides can be instituted; and resonance can be elicited from the parts most thickly covered, not with muscle only, but with the scapula, and even with the dress. The dexterity acquired by habit is necessary to regulate the force of the stroke, and to adapt it to the various regions of the chest. The less the force with which the degree of resonance can be determined, and the greater the delicacy with which it can be applied, the better.

On account of the more frequent occurrence of tubercles in the summits of the lungs, practitioners are apt not to extend their investigations beyond the antero-superior regions of the thorax, where percussion after any method is readily performed. The cases, however, are very numerous in which it is impossible to form an accurate opinion without examining the whole chest; and here I trust the instruments proposed will be found to facilitate, and render more satisfactory, the investigation.

These instruments may, at first view, appear formidable; but if judiciously made, as by Weiss and Son, they are light and portable; and experience has satisfied me of their utility. It is in hospital practice that they are especially convenient, where many cases require to be examined, and where it is a great object to obtain results with-

out loss of time, and in a manner instructive and demonstrative to those around. Nor have I found difficulty in their use in private practice; patients, with some exceptions which occur under any circumstances, being generally well pleased to have their cases fully investigated.

For the exploration of the abdomen by percussion these instruments are equally available; the usual precautions being observed.

Stethoscopes in great variety are employed for the purpose of auscultation; their shape and size varying according to the fancy and habit of individuals, and particularly according to their portability: indeed, the efficiency of the stethoscope is often much sacrificed to this last object. Portability is unquestionably a desideratum, and a short instrument is so far preferable; but at the same time it is very inconvenient to use. No one point is more essential in auscultation than that the position of the auscultator should be free from constraint; otherwise he cannot listen with effect. If he is obliged to stoop low, or to place himself in any other uncomfortable posture, it is quite impossible he can hear accurately, or analyse correctly what he does hear; disadvantages which belong to the short small stethoscopes.

There are certain qualities in the stethoscope which experienced auscultators are now generally agreed upon as requisite: namely, that it should be of one rather than of two pieces of wood; that the wood should be of medium specific gravity, as cedar; that the grain should be fine and straight; that one extremity should be funnel-shaped, the margin smoothly rounded, and, including the wood, about one inch and a quarter in diameter; and that the other extremity should be surmounted by an ear-piece sufficiently large to allow the ear to rest pleasantly upon it. The length most suitable for general use is nine inches: some prefer it shorter, others longer; in which latter case the instrument must be in two pieces.

The stethoscope used and recommended by Dr. Stokes combines the essential qualities in a greater degree than any other I have met with, and has been adopted by myself for some time past with great satisfaction. If

it consisted of one piece, about nine inches long, instead of two pieces, which make it eleven inches, it would be preferable. Dr. Stokes recommends that the ear-piece should be sufficiently hollowed out; in which I entirely concur. I have many times used an ear-piece shallow or flat, but have found it intercept the sound by pressing upon the tragus and partly closing the meatus; an imperfection to be avoided by having it made sufficiently concave.

Perhaps there is scarcely any person who, on first using the stethoscope, does not think he can improve its construction: but after many experiments he comes back to the point from which he started, and finds that scarcely any modification of instrument can surpass in efficacy the cylinder, which, after a great many trials, Laennec decided on as the best.

In the construction of the wood stethoscope it must be remembered that the vibrations of the chest are conveyed to the ear by the two-fold medium of the solid sides of the cylinder and of the central column of air; and hence the advantage of an ear-piece, instead of merely a tube to enter the meatus; whereas in the flexible stethoscope, by which the vibrations are conveyed only by the central column of air, a tube is preferable, as conveying the vibrations more continuously and directly into the meatus, and so to the tympanum.

The flexible stethoscope *. — Besides the wood stethoscope, I have used for some years past at the Westminster Hospital, and in private practice, a flexible acoustic tube similar in construction to that in use by deaf persons, the dimensions being altered and adapted to the purposes of auscultation. The length, for instance, is reduced to about twenty-eight inches, and the funnel-shaped extremity to the size of the wood stethoscope: but the ear-tube is retained as preferable to the ear-piece. The length of this flexible stethoscope

may vary from twenty-two to twenty-eight inches, according to the pleasure of the auscultator. My friend Dr. Stroud prefers eighteen inches, which I find inconveniently short; it being an object to have sufficient length to reach every part of the chest from the same side of the patient's bed, and without being obliged to stoop uncomfortably. It happens, too, that after some time the end of the tube affixed to the funnel-shaped extremity becomes so much worn as to require to be cut off an inch or two, which, if the tube were originally only eighteen inches long, would shorten it so much as to render it nearly useless; certainly to do away with its great advantage, that of enabling one to auscultate at a convenient distance, and in an easy posture.

The question of preference, as regards the ear-tube to be inserted into the meatus, or the circular ear-piece similar to that affixed to the wood stethoscope, is decidedly in favour of the tube. I have made ample trial of both, and have found the tube convey sounds with greater power and distinctness. Dr. Stroud, who began by using the ear-piece, has discarded it for the meatus tube. The reason is obvious. There being no solid medium in the flexible stethoscope to convey vibrations, they wholly depend, for transmission, on the central column of air, which is conducted to the tympanum more continuously and effectively by the meatus-tube than by the ear-piece.

It is of consequence that the spiral wire should be complete up to each end, or even a little projecting, and that the wire itself be very elastic, and not thicker than will admit of perfect flexibility. Tubes less flexible, and made of coarse wire, convey sound less distinctly.

The advantages offered by the flexible stethoscope are on the score of convenience and comfort; not on any superiority possessed by it over the cylinder. Indeed, so far from superiority, it is by no means equal to the cylinder in the conveyance of sound. It answers every purpose admirably when the sounds are loud, as in mucous wheezes, coarse crepitations, harsh frictions, and the stronger cardiac bruits: but fine crepitations, and delicate frictions and cardiac bruits, which are distinct and well marked when the wood stethoscope is employed, are ob-

* It was not without surprise that I read in the MEDICAL GAZETTE of December the 11th, 1840, "Observations on the advantages presented by the employment of a stethoscope with a flexible tube," by Golding Bird, M.D. in which a passing reference only is made to Drs. Clendinning and Stroud, who have for some years used this same instrument, and no reference to myself, who have employed it also for some years at the Westminster Hospital, where Dr. Bird's brother became clinical assistant in May, 1840; the Doctor himself not having used this stethoscope till a month afterwards, viz. in June.

scure or not audible with the flexible tube. The flexible, therefore, cannot supplant the wood stethoscope.

To measure the impulse of the heart, a solid cylinder of wood is preferable to every other medium.

The convenience and advantage of the flexible stethoscope are particularly felt in cases of extreme weakness, as adynamic fever with pneumonia, and under all other circumstances when, from the reduced powers of the patient, or other causes, he cannot be raised in bed, or easily got at. Here, by turning the patient a little over on the side, the flexible tube can be applied to every part of the chest, and the condition of the lungs be ascertained; which with the cylinder would not be practicable: added to this, the comfort of being able to gain this information at a distance that places one beyond the annoyance of the uncleanness attendant on poverty, or of the odours emanating from disease. I have met with ladies unwilling to allow the cylinder to be applied, who would not have objected to the flexible tube, the cup of which they could themselves have adapted to the chest, and so have avoided the proximity and exposure they dreaded. To professional men at all corpulent the flexible tube will prove a desirable substitute for the cylinder, as a means of preventing the congestion of blood in the head attendant on any stooping posture.

For the reasons assigned, the flexible stethoscope may be strongly recommended for adoption as a most desirable additional medium of auscultation, but not as a substitute on all occasions for the cylinder.

24, Lower Brook Street,
June 1, 1841.

PLURALITY OF CHILDREN AT BIRTH.

To the Editor of the Medical Gazette.

SIR,

ON referring to my father's note book I met with the following case. He was requested, January 4th, 1829, at 6 A.M., to attend Mrs. Calling, æt. 36, then in labour of her sixth child, who resided at Rawstenstall, near New Church, in the forest of Rossendale, and county of

Lancaster. He found his patient to be only about seven months pregnant, and she attributed the premature symptoms of labour to blows received from her husband on the previous day. On examination per vaginam, my father states that he found the os uteri dilated to the disc of a shilling, thin and yielding, membranes unruptured, &c. He deemed it proper (as her pains recurred at regular intervals, together with the irritable state of his patient, which irritability arose, in a great measure, from the severe treatment she had received) to wait with his patient, and allow nature to proceed with her work unmolested. About 11 A.M. the waters came away, when she was in a few seconds delivered of a male child. On applying his hand over the abdomen, the uterus appeared much distended; and on an examination the head of a second child was found entering the true pelvis, which was born in a few seconds (also a male). The pains still continuing, on examination he found the head of a third child pressing upon the perineum, which was shortly expelled (a male). After waiting for some time, and using gentle traction with the cords of the placenta or placenta, and apparently making no progress, he introduced his hand in utero, with the view of removing the placental mass, being of opinion that it had adhered to the uterus; but, to his great surprise, he found the feet of a fourth child, which he brought down, and, after securing the funis, he again passed his hand, and found the hand of a fifth child presenting, which he delivered by the feet. (The two last were females). On examining the placental mass, a rather singular phenomenon presented itself, three of the umbilical cords being inserted by one common radicle into the substance of the placenta; the other two cords were also inserted in the same manner.

There are many cases on record of five children being born at one birth, one of which occurred in the practice of my much esteemed friend, Mr. Hull, surgeon, of Blackburn, now Dr. Hull, late of Manchester. In Dr. Hull's case the whole of the children weighed only about as much as one of those which happened in my father's practice. Now in my father's case the whole of the children survived their respective births from

fifteen to thirty minutes; each measured from twelve to thirteen inches in length; and the whole of them weighed ten pounds. Another feature in the case is the termination of the umbilical cords, three of which, as already stated, arose from the umbilical cord, about two inches from the substance of the placenta; the remaining two were inserted as already described in the case of the other three. Dr. Ramsbotham* remarks, when speaking upon this subject, that, "according to the law of nature, each individual child is distinctly enveloped in its own membrane; so possesses its own quantity of liquor amnii, has a separate funis, and separate placenta, the circulations not inosculating." Yet in my father's case, as well as in others related by Dr. R., we perceive how nature adapts herself for the fulfilment of her purpose, in the procreation of our species, and that one placenta is as efficient to maintain three children as one in utero. On laying open the umbilical cord at that point where the three coalesce into one, the three umbilical veins were found to terminate in a *cul de sac*, from the posterior part of which arose the umbilical vein, which was soon lost in the substance of the placenta. On opening the umbilical cord a few lines beyond the point where it emerges from the placenta, the umbilical artery was found to divide into three branches in one, and two in the other, opposite the superior and anterior portion of the *cul de sac* formed by the umbilical veins.

Cases like these may call forth many hypotheses from the ingenuity of men's grasping intellect, and which hypotheses may, from their apparent resemblance to truth, be received as granted, that such was the order and manner in which nature worked to produce what we frequently see as her handy-work. So obscure are nature's ways that the most profound divine, acute metaphysician, and erudite physiologist, have not been enabled to find them out; so hidden are they from our observation, that it requires something superhuman to ascertain the laws by which generation is affected.

My father delivered Mrs. C. of a still-born child in forty weeks after the

birth of the five. Your insertion of the above case and remarks in your valuable periodical will oblige

Your obedient servant,
T. H. WARDLEWORTH,
Surgeon.

Rochdale, May 25th, 1841.

OVARIAN DROPSY.

To the Editor of the Medical Gazette.

SIR,

IF the following be of sufficient interest to merit a corner of your valuable journal, oblige me by giving it insertion.—I am, sir,

Your obedient servant,
JOHN FOSSE HARDING, M.R.C.S.
13, Spencer Street, May 31, 1841.

About twelve months since I published in the MEDICAL GAZETTE a case of ovarian dropsy coexisting with pregnancy, since which period the patient had been relieved by several operations; after which, from examination, I found the tumors which existed were rapidly enlarging, and after the repeatedappings the constitution became much impaired, and general atrophy set in, with anasarca of the legs, and the patient gradually sunk in April last.

By permission of the friends I was allowed a post-mortem examination. On cutting through the abdominal parietes I found the fibres of the muscles so attenuated as to be scarcely visible, having previously drawn off the fluid mixed with a quantity of flocculi. The abdominal cavity had the appearance as if it had been deprived of the whole of the viscera. The peritoneal sac was very much thickened, connecting and compressing the intestines towards the spine and pelvis, and the liver up under the diaphragm; showing the whole of the pelvic region, to the umbilicus, occupied by an immense ovarian tumor in a state of suppuration, with tuberculous matter, mixed up with hydatids, and many cysts containing a kind of gelatinous fluid, weighing altogether about eight pounds. The uterus was quite flattened, about the size of a crown-piece, and had the appearance of a ligamentous substance. I could not satisfactorily make out the left ovary, which I am of opinion must

* Vide Principles of Obstetric Medicine and Surgery, p. 625.

have been much atrophied; the right one being implicated in this heterogeneous mass. The kidneys, spleen, and liver, were all healthy; the gall bladder quite choked with bile. I was surprised to find so much disease in so advanced a stage, as the patient had within twelve months been delivered of a fine full-grown male infant, which is now living, and in robust health. The patient had been operated on altogether eleven times, and thirty-four gallons of fluid drawn off.

REMARKS ON SOME LATE OBSERVATIONS CONCERNING THE CORPUSCLES OF THE BLOOD.

To the Editor of the Medical Gazette.

SIR,

IN the late volume of the Transactions of the Provincial Medical and Surgical Association there are some material errors, to which I beg to direct your attention, respecting the history of the blood corpuscles. I venture to hope that you will correct these misstatements, as you have always appreciated the interest belonging to the physiology of the vital fluid, and have shewn a most praiseworthy spirit of impartiality in awarding justice to the pretensions of different observers in the various departments of anatomical science. I will first submit to you an abstract from the work in question, and then subjoin a few brief remarks, carefully avoiding any statement not supported by a sufficient voucher.

“Professor Owen found the blood corpuscles of the elephant twice as large as the ordinary corpuscles of human blood; hence they are larger than those of any mammiferous animal hitherto described. Those of the very small animal, the armadillo, rather exceeded those of the rhinoceros. In the dromedary he noticed the elliptical form described by Mandl, but amongst them were a few of circular form. This form has also been observed by Gulliver, in the *Auchenia* species. The corpuscles of the giraffe have the circular form of the mammiferous ruminants generally; but the average size was nearly one-third smaller than those of the human subject. The result of the examination of this animal, which

is the largest of the Ruminantia tribe, Mr. Owen considers interesting, inasmuch as it indicates that the size of the blood corpuscles relates to the condition of the whole organization, rather than to the bulk of the species. It would appear from the examination of the blood discs in the goat, sheep, and ox, that their unusually small size is associated with the peculiarities of the ruminating structure. The elliptical form of the corpuscles also in the llama and vicugna, as well as in the dromedary, confirms the fact that the camel tribe of ruminants present, among other peculiarities of organization, the singular exception of the form of the blood corpuscles, which has hitherto been observed only in the oviparous vertebrata. The blood of the tiger, the typical felis, had not been previously examined; and Mr. Owen agrees with Müller and Wagner that the corpuscles of the strictly carnivorous are intermediate between those of the omnivorous species and those of the strictly herbivorous, being smaller than those of man and the Quadrumana, their average size being $\frac{1}{53\frac{1}{4}}\frac{1}{8}$ of an inch in diameter. The blood corpuscles of the chevrotain, the smallest of the Ruminantia, presented the smallest hitherto observed in the vertebrate animals, being $\frac{1}{12800}$ of an inch.”—Vol. ix. p. 190.

1. With regard to the blood corpuscles of the elephant. The fact of the large size of the blood discs of this animal was long since announced by M. Mandl, who gave an explicit measurement of them in comparison with another measurement of the human blood discs (*Anatomie Microscopique*, 1838; liv. I. p. 17); but Mr. Owen was either unacquainted with this circumstance, or did not choose to mention it; for this would have appeared rather awkward with respect to the *discovery* of the Professor. A copy of M. Mandl's work has long been in the library of the College at which Mr. Owen holds his professorship.

2. It is difficult to understand the meaning of the author with respect to the corpuscles of the camel tribe. Does he mean to say that Gulliver also discovered the *circular* form in the blood corpuscles of these animals? The fact is simple enough; for Mandl discovered the *oval* form in the blood discs of the dromedary and paco (*Anat. Micros.*

iv. I. p. 17), and Mr. Gulliver subsequently observed the same form in the discs of the vicugna (*Auchenia Vicugna*) and llama (*Auchenia Glama*), as may be seen by reference to the Med. Chir. Trans. Vol. 23; and Dublin Medical Press, Nov. 27, 1839. Now Mr. Owen's observations on the blood of the vicugna appeared for the first time in the MEDICAL GAZETTE, Dec. 20, 1839; and he does not mention either the llama or paco.

3. A very clear measurement is given by M. Mandl, in the work already cited, of the blood discs of the giraffe in comparison with those of man.

4. As to the blood corpuscles of the chevrotain being "the smallest hitherto observed in the vertebrate animals," Mr. Gulliver published this fact on the 26th and 27th of November, 1839, as a reference to the books mentioned above will shew; and Mr. Owen's account of the blood of this animal was originally published in the MEDICAL GAZETTE, December 20, 1839. That he could not be ignorant of Gulliver's observations on the subject may be fairly supposed from the fact that they were noticed in the Philosophical Magazine, Dec. 1, 1839, as well as in the Magazine of Zoology and Natural History, of the same date. Well, therefore, might the College professor say, "that the blood corpuscles of the chevrotain, *as he anticipated*, were the most minute," &c. especially as all the works mentioned above are regularly placed in the College library. It should be mentioned, however, that the Professor calls the animal in question "the chevrotain; *Moschus pigmæus*." This is the old scientific name, with a French vulgar one. The animal is in fact the Napu musk deer, *Moschus Javanicus*, Pallas; or *Tragulus Javanicus*, (Raffles, vid. Cat. Mammalia, in Museum Zool. Soc. 1838, p. 37) and the label attached to the cage of the animal in the Society's menagerie. It is unnecessary to make any further observation on this proceeding of Professor Owen's.

In conclusion I have only to remark that I think you will agree with me that we have a right to expect common accuracy from gentlemen professing to give retrospective accounts of the progress of science. It may, however, be fairly urged in excuse for the writers of these accounts, that the necessary sources of information might not be

easily accessible to them. But where the claims of Professor Owen were to be spoken of, it might have been learned from the MEDICAL GAZETTE that there was, unfortunately, some reason not to trust implicitly to the scientific statements of the College professor.—I am, sir,

Your most humble servant,

A GENERAL PRACTITIONER.

London, May 31st, 1841.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à alonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Complete Practical Treatise on Venereal Diseases, and their immediate and remote consequences; including Observations on Certain Affections of the Uterus attended with Discharges. By WILLIAM ACTON, late Externe at the Female Venereal Hospital, Paris. 8vo. pp. 410. London, 1841.

THE whole of this work proves the author to be a faithful disciple of M. Ricord, and ready to adopt his opinions rather than his own, or those of any other writer, on the subject of syphilis. We think, however, that Mr. Acton has done wisely in not publishing a mere translation of M. Ricord's works. Although, on all the subjects on which that surgeon has written, his opinions are, we believe, without question received, yet by taking upon himself the responsibility of an author, rather than that of a translator, Mr. Acton has compelled himself to make the work more nearly a *complete treatise* than he would otherwise have done, and has thus produced a work which will probably be more useful than those by M. Ricord, or any other English writer.

After an introduction containing a brief history of the venereal diseases, taken chiefly from Astruc, the work proceeds to its proper subjects; and these are disposed of in two parts—the first treating of syphiloid diseases, the second of syphilis; the former term, however, being ill chosen, inasmuch as the main burden of the first part of the work is to establish that, except in their common origin in sexual intercourse, the syphiloid diseases have no relation to the syphilitic.

The first order includes "Blennorrhagia," and these are all mucous or muco-purulent discharges from the uro-genital organs (including of course gonorrhœa), excoriations, herpes, eczema, stricture, effusions of urine, fistula in perineo, venereal affections of the prostate and testicle, false passages by the urethra, &c. In all this, although it presents little that is at the same time both novel and important, the work affords a very good practical guide in all ordinary circumstances to both the practitioner and the student. Of its several parts we should chiefly recommend that relating to blennorrhagia in the female, for it certainly presents a better idea of the pathology, or rather, of the morbid anatomy, of the class of diseases included under this name, than any other that we have met with. And we say this, although upon the nature of the disease, we believe, the author, following M. Ricord, holds opinions which are most probably incorrect, and certainly entertained upon very insufficient evidence. It is assumed that what is commonly called leucorrhœa is identical with what is usually described as chronic gonorrhœa; and this on no other evidence, as far as we can see, but that the discharge in the former is sometimes infectious, and that in the latter sometimes not infectious. The fact is overlooked that these *sometimes-effects* are the very exceptions to general rules. Probably the discharge of leucorrhœa, *i. e.* the vaginal or uterine discharge which occurs in great numbers of women who have never been infected with gonorrhœal matter, will not in one case in a hundred or more infect a man so as to produce a discharge from his urethra; nor in that one man will the discharge produced be that of ordinary gonorrhœa, or accompanied by its symptoms; whereas, the discharge remaining on a woman after gonorrhœal infection, will, with equal probability, produce gonorrhœa in ninety-nine men out of one hundred not habituated to the disease. Now the generalities ought to be the foundations of the rule; and if they be as we state them, it follows that the occasional exceptions in which leucorrhœa is infectious, and chronic gonorrhœa not infectious, cannot overthrow the conclusion that the two diseases are essentially different. All that we can yet say is, that we

cannot, in every case, determine to which of the two any given example belongs, except by the test of its being communicable by contact: by evidence, that is, of nearly the same kind as M. Ricord and Mr. Acton require for the discrimination between doubtful-syphilis and syphiloid disease.

In the second, and by far the best portion of the work, the subjects treated of are chancre and its consequences; and on all of these the information is full, and certainly more satisfactory than any hitherto published in England. There can be no doubt that the test of inoculation, to which M. Ricord has so freely resorted, has afforded him a means of obtaining a knowledge of the principles of general pathology which without it could not have been attained; and working out these into their details in a practice of unequalled extent, and with an energy well proportioned to his opportunities, he has accumulated a volume of evidence which renders both the history and the treatment of the venereal diseases much less uncertain than they have hitherto been. Of all this knowledge Mr. Acton has freely availed himself, and adding to it some of that which is more familiar in this country than in France, and a small quantity of his own observation, has formed a work that may on most subjects be confidently consulted. For although many of the opinions advanced in it seem to us rather hastily formed, they are happily not made the foundations of dangerous or immoderate treatment; and it is chiefly, we apprehend, by the confidence in gentle measures that the perusal of this work will give, that it will most improve the modern method of dealing with the disease.

But while we thus recommend the book to our readers, we are bound to recommend Mr. Acton also, should he again become an author, to improve his style of writing. If he will now calmly read his own book, he will find that his composition is inelegant and often ungrammatical; that the technical names, which he says he has chosen after much deliberation, do yet by no means well express the conditions to which they are applied; and that while there is often a pompous attempt at loftiness of style, which, even if successful, would be unsuited to the subject of the work, there is almost

as frequently a want of decency which even the subject of syphilis does not warrant. If, in a second edition, these errors are avoided, the book will merit little less than commendation.

The plates, which are published in a separate quarto volume, are excellent, though, through the deficiencies of the colourer, they appear, like many others of their kind, to be unworthy imitations of the originals. For the purpose of diagnosis, those of the varieties of chancre, and of the appearances of a sore in its several stages after inoculation will be highly useful; and the more so because the description of what they represent is imperfectly given in the text.

MEDICAL GAZETTE.

Friday, June 11, 1841.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”
CICERO.

REFORM IN THE COLLEGE OF PHYSICIANS.

IF the abandonment of old and long established prejudices, and an earnest desire to conform to the spirit of the times, be meritorious, then is the College of Physicians entitled to commendation above any corporation, lay or professional, with which we are acquainted; and we hold ourselves the better entitled to express this opinion, from having repeatedly censured the learned body in question—and that in no very measured terms—when they appeared to be actuated by different principles. It must be in the recollection of our readers, that, a few years ago, we took occasion repeatedly to denounce, in the strongest language, the illiberal and injurious spirit of monopoly displayed by the College of Physicians. But, in doing this, our indignation was excited against the measures in question, and the spirit which had given rise to them—not against the College simply because it was an old institution, and had a character, nor against the members simply be-

cause they were physicians. Now this appears to us to be the great and radical difference between the *Lancet* and ourselves: so long as there remains a corporate body in Pall Mall, or a set of practitioners designated physicians, so long will all they do be the subject of censure, and (would we did not feel compelled by experience, to add) of misrepresentation. “Few individuals (says our contemporary) can feel proud of their connexion with the recent proceedings of the College of Physicians.” In our humble judgment the parties alluded to deserve the highest credit, and the sentence ought to have been, “can feel *otherwise* than proud.” To prove this to any unprejudiced mind, no more is required than a fair and candid statement of what has been done. Let us, then, just see what those “recent proceedings” have been.

A few years ago the London College of Physicians was little better than a club of the most exclusive character, to which none but the graduates of Oxford and Cambridge could look for admission. A few—a very few others, were added to the list, but only one at a time, at long intervals, by the voice of the President, and under such circumstances as rendered the general ban more conspicuous and more galling. What change has been made in this respect? Simply this: that now the graduates of the English universities have no privilege or advantage whatever over others, either nominally or practically. The Fellows are exclusively elected from among the Licentiates; and a reference to the list will shew that a large majority, so chosen, have not had English degrees. The unfair advantage given to Oxford and Cambridge has thus not only been discontinued, but the members would seem to have acted as if they intended to turn the balance the other way.

Now, if this was a great and important change, still it was open to

this objection—that, although the order of Licentiates was raised by making all alike pass through it in the first instance, yet the Fellows were after all self-elected, as they alone voted when an addition was made to their number. The next step, and the most recent, has been, to make the election to the fellowship a general one, at which all belonging to the College—President, Fellows, and Licentiates—should vote alike. Hereafter there is not only no invidious privilege to be claimed by English graduates, but the Fellows are made to share their rights with the Licentiates, who, as they are the more numerous body, have thus virtually the power (if they choose to exercise it by combination) of filling up all the new nominations, even in opposition to those already in the fellowship.

This privilege at once and most unequivocally declares the Licentiate to be a *member* of the College; and if so, why not do away with the former objectionable appellation, and substitute the latter? We know no good reason why this should not be done: nor, we are happy to say, does the College; for they have done it! The term “Licentiate,” which never ought to have been adopted, is hereafter to be abolished, and the more appropriate one of “Member” substituted.

Again, the members, thus acknowledged as such, have the right of entrance into the College freely conceded, not only in the important function of electing the Fellows, but in the use of the library and museum; this last being of importance, however, chiefly as the establishment in their favour of a disputed privilege.

Now whether the above is all that might be done, may probably be made a question; but no candid man who knew what the London College of Physicians was a few years ago, and compares such state with its present condition, can deny that a very great

reformation has been effected. So far from the charge being true that those Licentiates, who, during the last few years, have been admitted to the fellowship, have neglected the interests of their former associates, we have reason to believe that it has been mainly through their support that the most recent and important measures have been carried. Many of the old and bigoted Fellows have died off or retired within the last few years; and others have succeeded them who are much more liberal, both in general and professional politics. By them, chiefly, have the above measures been propounded; but we repeat that, from all we learn, without the attendance and active support of the Licentiate-Fellows, there is great reason to believe that they would have been unable to carry them through; at all events, not with such triumphant majorities.

To recapitulate and place prominently before the reader the recent changes in the College of Physicians, we would state,—

1. That for the space of a year all physicians at present practising in this country, and who have their degrees from any British university, are to be admitted without subjecting them to any fresh examination.

2. That all so admitted, and all the present Licentiates, become and be designated “Members” of the College.

3. That all Members shall have the right of access to the College; the library and museum being henceforward open to them.

4. That the number of Fellows be raised to 200 (being an increase of about one-fourth); and that they be elected by the whole College at large on equal terms.

5. That there be an annual meeting of all the members for the election into the fellowship, and such other matters as may be agreed upon.

Such is the “reform” in the College

of Physicians recently agreed upon, and, as we understand, by large majorities; and we regard it as highly creditable to those by whom it has been effected. If experience should show that further changes would be beneficial, we trust that we shall see them carried into effect; and, from all that we can learn, there is far from being a wish, in the College at large, "to stave off the whole question" (see letter which follows), and we can scarcely believe that any one would be fool or knave enough to express such a sentiment. At the same time we entirely concur with "Senex," that immediate official promulgation should be made by the College of their intentions, and that every thing they can do without an Act of Parliament (which includes all the above changes), should be carried into effect without an hour's delay. A little patience and a little prudence would then suffice entirely to remove the dissensions which the former obnoxious regulations of the College created, and place this branch of the profession on a better footing than it has for many years enjoyed in this country.

A HINT TO THE COLLEGE.

To the Editor of the Medical Gazette.

SIR,

IN your last number there is a long letter published with the avowed intention "*of setting the profession at large to rights*" on certain points relating to the recent proceedings of the College of Physicians.

Coming immediately after your leading article, and being in large type, it appears to bear a certain stamp of official character about it, and it is in fact put forth as an explanation of the changes which are decided upon by the College. Those who are anxiously looking for an *authentic* statement (and there are at this moment a considerable number who are merely waiting for the proposed changes to give in their adhesion to the College, *provided they speedily*

take place), do not appear, however, so thoroughly satisfied with the present explanation as the writer of it would anticipate.

Although it is, I have no doubt, "a plain unvarnished tale" on the part of Philodemus; still, as an ex-licentiate, he is not perhaps aware of the opinion of many of the older Fellows; and the anxiety to act *bonâ fide*, on the part of some of them, may not be perhaps so readily granted, when an opinion has been expressed—"that now Parliament is no longer likely this session to meddle with the College, the *whole question* may be staved off till another time, as the proceedings were merely to shew to Parliament that the College itself was anxious for Reform."

Now should the College proceed "in this just and honourable course," the profession will at a future time be fully aware of what they are to expect from this body, and their own proceedings may be influenced in future by the knowledge of the past.

A much more satisfactory test, as to the intentions of the College, than the explanations of Philodemus, would be their speedily carrying into effect the changes which do not require an act of Parliament; and this proceeding only will allay the doubts which are daily strengthening into certainty.

The year of grace alluded to, why should it not commence immediately? If the intention really exists, why not instantly put it into execution? Then would the profession have good reason for expecting the other important changes which are voluntarily to emanate from the College. This would be tenfold more satisfactory than all the explanations of Dr. G. or Dr. C.

It is now universally admitted that important changes are required in the College of Physicians. Without paying attention to the monstrous and absurd propositions which are daily put forth, still, surely, it may be admitted that it might be a just policy to carry into operation *their own*, as far as it lies in their power, and thus give an earnest as to their anxiety for sanctioning the report of their Committee in those matters which require the approval of a higher court; recollecting, however, the trite adage—"Bis dat qui cito dat."

Your obedient servant,

SENEX.

London, May 25, 1841.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 29th May, 1841.

Small Pox	13
Measles	11
Scarlatina	8
Hooping Cough	45
Croup	5
Thrush	4
Diarrhoea	7
Dysentery	1
Cholera	0
Influenza	3
Typhus	26
Erysipelas	3
Syphilis	1
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	172
Diseases of the Lungs, and other Organs of Respiration.....	251
Diseases of the Heart and Blood-vessels	11
Diseases of the Stomach, Liver, and other Organs of Digestion	67
Diseases of the Kidneys, &c.....	7
Childbed	9
Ovarian Dropsy	1
Diseases of Uterus, &c.....	2
Rheumatism	3
Diseases of Joints, &c.....	1
Ulcer	1
Fistula	0
Diseases of Skin, &c.....	1
Diseases of Uncertain Seat	93
Old Age or Natural Decay.....	58
Deaths by Violence, Privation, or Intemperance	21
Causes not specified	7
Deaths from all Causes	833

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N. Longitude 0° 3' 51" W. of Greenwich.

June.		THERMOMETER.	BAROMETER.
Wednesday	2	from 48 to 72	30·12 to 30·16
Thursday	3	51 69	30·14 Stat.
Friday	4	42 67	30·30 30·25
Saturday	5	46 67	30·14 30·00
Sunday	6	48 59	29·95 29·94
Monday	7	40 59	29·88 29·93
Tuesday	8	45 59	29·93 29·91

Winds, S.W. and N.W.
On the 2d, and two following days, generally clear. The 5th, morning clear; afternoon and evening overcast, with rain. The 6th, generally clear. The 7th, and following day, overcast; with rain.

Rain fallen, ·02 of an inch.
Parhelia.— Never, perhaps, were these phenomena more brilliantly seen than on the evening of Friday, the 4th inst. At 6 h. 45m. from East and West of the Sun, distant about 23 degrees from the Sun, and, as usual, on the circumference of a halo, two mock suns formed, strongly coloured, and no less remarkable for intensity of light, then length of train, which, as invariably is the case, was in a direction from the Sun, and parallel to the horizon. The length of the luminous cone was sufficiently bright to be measurable at a distance of 18 degrees, and, probably, the apex must have been several degrees beyond. The upper part of a second halo, faintly coloured, also formed 26 degrees above the first.

CHARLES HENRY ADAMS.

ST. THOMAS'S HOSPITAL.

MR. SOLLY and MR. MACKMURDO have been elected Assistant Surgeons.

ST. GEORGE'S HOSPITAL.

DR. NAIRNE has been elected Physician, in the room of DR. HOPE; and DR. PAGE has been elected Assistant Physician.

ANTIDOTES TO ARSENIC.

IN a recent number of the *Medicinische Annalen* is reported a case, in which the hydrated peroxide of iron was successfully administered to five persons, three of them children, who had taken soup containing arsenic, and who exhibited the usual symptoms of arsenical poisoning.

In the *Medico-Chirurgical Review* (Oct. 1840), two cases are related by Mr. Serph of Welshpool, in which the *carbonate of iron* was successfully employed. In one of them a boy of twelve had swallowed about a scruple of arsenic, and in the other a man had *rubbed* himself freely with a strong solution of arsenic and sulphate of copper; both of them presented the usual symptoms of poisoning with arsenic. — *Edinburgh Monthly Journal of Medical Science.*

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Friday, May 28.

F. W. Peek.—J. W. Metcalfe.—T. Jarmain.—W. M. Culpeper.—W. Maynard.—J. Lewis.—J. J. Pocock.—C. B. Painter.—A. G. Melville.—J. Cresswell.

Monday, May 31.

H. D. Mitchell.—J. Macnamara.—W. H. Kent.—R. Williams.—R. Pitt.—M. O'Regan.—F. D. Gilbert.—J. N. Day.—J. G. Barratt.

Friday, June 4.

W. H. Stevenson.—W. Hannant.—R. Mapleton.—J. G. Bowlby.—W. H. Yelloly.—T. Handford.—W. F. Tuckett.—J. G. Forbes.—J. Lambert.

NOTICES.

WE regret that we cannot comply with the request of a Medical Student, by giving the list of Prize-men at Guy's, because it would involve the necessity of doing the same by all the other schools.

The papers of Mr. King, Dr. Fife, Dr. Hocken, and Dr. Richard Fowler, have been received, and shall have early insertion.

WILSON & OGILVY, 57, Skinner Street, London.

THE
LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, JUNE 18, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XXXIX.

Catalepsy. Extacy. Neuralgia: tic douloureux; sciatica; hemicrania.

THERE are yet some strange forms of nervous disorder which require to be mentioned; but upon which I do not intend to dwell. *Catalepsy* is one of these; and what is called *extacy* another. These affections are very rare as well as very wonderful: so wonderful and rare, that weak and superstitious persons have referred them to the interposition of supernatural agents in human affairs; and stronger minded persons, who happen never to have witnessed such diseases, deny their occurrence as fabulous, or laugh at them as the tricks and cheatings of imposture. They certainly do happen, however; and they happen mostly in the same class of persons in whom hysterical and nervous complaints of all kinds are most common. They often appear to be produced by similar causes with these: they resemble hysteria in being seldom attended with any danger to life; their pathology is, if possible, still more obscure than that of hysteria: and if I were to speak of the treatment which would seem to be most suitable for their cure or prevention, I should merely have to repeat what I said, upon the treatment and prevention of hysteria, in yesterday's lecture. I shall content myself, therefore, with a short description of these two affections, that you may be aware of their characteristic phenomena, and not be taken by surprise in case either of them should occur to you in your practice.

707.—XXVIII.

Catalepsy.—A fit of catalepsy implies a sudden suspension of thought, and sensibility and voluntary motion; the patient remaining, during the paroxysm, in the position in which she (for it is almost always a female) happened to be at the instant of the attack, or in the position in which she may be placed during its continuance; and all this without any notable affection of the functions of organic life.

This is certainly a very curious state, and one different from any that we have yet contemplated. We have had the muscles rigidly contracted with tonic spasm, while the powers of the mind, and the sensibility of the body, were unimpaired. We have had the same muscles shaken with clonic convulsions; both with and without accompanying disorder of the intellectual functions. But here we have a new phenomenon: the mental faculties are in abeyance, and the sensibility is abolished, and so also is the function of voluntary motion; but the limbs are not tied down by spasm; nor agitated by successive contraction and relaxation of their muscles; not yet left, like portions of dead matter, passively obedient to the laws of gravity: they assume any posture, however absurd, however (to all appearance) inconvenient and fatiguing, and that posture they retain, until some new force from without is applied to them, or until the paroxysm is at an end. The patient so affected, with open staring eyes often, and outstretched limbs, looks like a waxen figure; or an inanimate statue; or a frozen corpse. Indeed, Hoffman seems to have formed the strange conclusion that, as catalepsy, so far as he knew, occurred most frequently in winter, it must depend on congelation of the nervous fluid.

These singular attacks occur in paroxysms; and they have been known to alternate with well-marked hysteria; and to take place in connexion with insanity. I have never seen an instance of perfect catalepsy; which I

now regret, as I once had an opportunity of doing so of which I did not avail myself. Dr. Gooch has described a case of it, as he witnessed the disease in a patient who suffered puerperal mania. She had long been subject to the common forms of hysteria: this is illustrative of what I have often mentioned, viz. the consanguinity of these nervous disorders. It had become necessary to confine this patient in a straight waistcoat; she was attended by Dr. Gooch and Dr. Sutherland. I will quote Dr. Gooch's account of the cataleptic state; for it is authentic and modern. He says, "a few days after our first visit we were summoned to observe a remarkable change in her symptoms: the attendants said she was dying, or in a trance. She was lying in bed, motionless, and apparently senseless. It had been said that the pupils were dilated, and motionless, and some apprehensions of effusion on the brain had been entertained; but on coming to examine them closely, it was found that they readily contracted when the light fell upon them; her eyes were open, but no rising of the chest, no movement of the nostrils, no appearance of respiration, could be seen; the only signs of life were her warmth and pulse; the latter was, as we had hitherto observed it, weak, and about 120.

The trunk of the body was now lifted, so as to form rather an obtuse angle with the limbs (a most uncomfortable posture), and there left with nothing to support it; there she continued sitting while we were asking questions and conversing; so that many minutes must have passed. One arm was now raised, then the other; and where they were left, there they remained. It was now a curious sight to see her, sitting up in bed, her eyes open, staring lifelessly, her arms outstretched, yet without any visible sign of animation. She was very thin and pallid, and looked like a corpse that had been propped up, and had stiffened in this attitude. We now took her out of bed, placed her upright, and endeavoured to rouse her by calling loudly in her ears; but in vain. She stood up, but as inanimate as a statue. The slightest push put her off her balance. No exertion was made to regain it. She would have fallen if I had not caught her.

She went into this state three several times. The first time it lasted fourteen hours, the second time twelve hours, and the third time nine hours; with waking intervals of two days after the first fit, and one day after the second. After this the disease resumed the ordinary form of melancholia; and three months from the time of her delivery she was well enough to resume her domestic duties."

There is a minor form of this affection described, in which the patient is incapable

of moving or speaking, but is conscious of all that goes on around him at the time. I saw a lady last year, who was subject to these attacks of imperfect catalepsy; which have been whimsically, but very expressively, called also attacks of *day-mare*. From her time of life, her habits, and some other points in the history of the disease, I concluded that in her case these seizures of temporary loss of muscular power without loss of consciousness were dependant upon a diseased state of the blood-vessels of the brain. She afterwards consulted Dr. Chambers; and he told me that he had formed the same opinion of the nature and cause of the symptoms.

Extacy.—In what is called *extacy*, the state is different. The patient is lost to all external impressions; but wrapt and absorbed in some object of the imagination. The muscles are sometimes relaxed; sometimes rigid as in slight tetanus: but the loss of voluntary power over them is not complete or universal, for these patients often speak in a very earnest manner, or sing: they are, as the term *extasis* imports, out of the body at the time, wholly engrossed in some high contemplation. This state is not uncommon as forming a part of religious insanity: and sometimes it runs into ordinary hysteria. Nervous and susceptible persons are apt to be thrown into these trances under the influence of animal magnetism: and grave authors assure us that the intelligence which then deserts the brain concentrates itself in the epigastrium; or at the tips of the fingers: that people in that state read letters which are placed upon their stomach; answer, oracularly, enigmatical questions; describe exactly their own diseases; and even foretell future events. Credat Judæus Apella, Non ego. I take for granted that they who were in the habit of speaking, a few years since, in some of our places of worship, in what they called *unknown tongues*, were either gross impostors, who deserved to be publicly whipped, or persons labouring under this disease, and wanting physic. Dr. Copland mentions a curious fact in connexion with this subject. He says that many of the Italian Improvisatori are in possession of their peculiar faculty only while they are in a state of extatic trance; and that few of them enjoy good health, or consider their gift as otherwise than something morbid.

I repeat that I can add nothing respecting the pathology or the management of these diseases, to what I have already said in reference to the whole class to which they belong.

Neuralgia.—Leaving these nervous disorders, in which the function of voluntary motion is so curiously modified; and in which there sometimes is no alteration of the intellectual faculties, and sometimes very

great disturbance, or the complete suspension of them; I would beg to turn your attention to another class of complaints, in which the nervous system is still the part principally interested, but in which the deviation from the natural state is manifested chiefly in the function of sensation; the powers of thought, and of voluntary motion, being scarcely affected, or not affected at all. Complaints, I mean, in which the sensibility is perverted, and augmented: cases of nervous *pain*. We have considered before that modification of sensation which consists in numbness, or anæsthesia, *i. e.* in the diminution of the natural sensibility, or its total privation. We have noticed also incidentally many perversions of sensation; such as giddiness, nausea, faintness, and the like: and in the same incidental way the morbid *exaltation* of the sensibility which is called *pain*, has come before us, as a symptom of various other diseases; of inflammation, and of hysteria. But there are diseases which consist of pain, and of nothing else, that we can perceive: they are often attended by no inflammation, no detectible change of structure in the painful part, no fever. These affections are included under the general term, *neuralgia*. Now pain is one of the things which we are oftenest consulted about; and these neuralgic pains are often excessively severe and troublesome; and it cannot but be of importance to understand what has been ascertained of their nature, and causes, and capability of cure.

That pain is owing to some morbid condition, or to some irritation of a nerve, we may sometimes know, by finding that it occupies exactly the course, and follows the distribution of the nerve. But when, as often happens, the pain is confined to a certain spot, we then conclude it is neuralgic, if and because we can find no other explanation of its existence.

What increases the difficulty of making out the cause and origin of these nervous pains, is that they may be produced by some source of irritation situated at a distance from the part in which the pain is felt. It may be placed in the brain itself, or in the spinal cord; or in the trunk of the nerve that supplies the affected part; or in one of the branches of the same trunk, which branch is distributed to another part. If you strike the inside of the elbow in a certain way, so that the blow lights upon the ulnar nerve, a peculiar tingling sensation is felt in the little finger: that is, *not* in the part struck, but in the sentient extremity of the same nerve; and the same thing happens continually in disease. There is an excellent paper on this subject, by Sir Benjamin Brodie, published in one of the earlier volumes of the *MEDICAL GAZETTE*, in which he has collected numerous and striking illustrations of the pro-

duction of nervous pain by irritation situated in a distant part. Thus, to take a case in point; a man was admitted into St. George's Hospital on account of severe pain on the inner side of his knee. The joint was carefully examined, but no mark of disease could be detected in it. On tracing the limb upwards, however, an aneurism of the femoral artery, as big as an orange, was discovered in the thigh. This the patient thought nothing of; his only concern was the pain in his knee. Sir E. Home performed the usual operation for aneurism: and the moment the ligature was drawn firmly round the artery in the upper part of the thigh, the tumor ceased to pulsate, and the pain in the knee ceased also. This man died four or five days after the operation: and on inspection of the limb after his death, the aneurism was found reduced to one-half its former size; and some branches of the anterior crural nerve, which passed over it, and which must have been kept on the stretch previously to the operation, were seen to terminate in the part to which the pain had been referred on the inside of the knee. There is just such another case related by Dr. Denmark, in the *Medico-Chirurgical Transactions*. A sailor was wounded by a musket-ball in the arm. The wound healed; but the patient remained affected with agonizing pain, beginning in the extremities of the thumb and fingers, except the little finger, and extending up the forearm. His sufferings were so great that he willingly submitted to have the limb amputated; and the operation gave him complete and immediate relief. When the amputated limb was dissected, a small portion of lead, which seemed to have been detached from the ball when it struck against the bone, was found imbedded in the fibres of the median nerve.

These examples teach us, when we receive complaints of pain in any part, and can discover no cause of pain in the part itself, to look for some possible source of irritation in the trunk of the nerve, from which the part in question is supplied with nervous fibrils.

But the source of irritation may be farther back than this: it may depend upon a diseased state of the spinal marrow, or of the brain. Of this we have had so many examples before us already, that I need not seek for any new illustrations of it.

Sometimes, again, irritation applied in the course, or at the extremity, of *one* branch of a nerve, will give rise to pain at the extremity of *another* branch of the same nerve. The sensation appears to be reflected, as it were, along the branch which is not, directly, the subject of the irritation. Thus filaments of the phrenic nerve penetrate the diaphragm and communicate with the ganglia that lie around the celiac artery; other filaments are distributed to some of the muscles about the

shoulder; and in this way has been explained the well-known fact, that disease or irritation of the liver is very apt to be accompanied with pain in the shoulder.

Thus also we have pain in the glans penis, from the irritation of the bladder, produced by a stone there: pain of the thigh and testicle, from irritation of the kidney: pain in the left arm, from disease of the heart: pain in the feet, from stricture and irritation of the urethra.

There are many pains also, plainly enough connected with irritation of distant parts, although no other nervous connexion can be traced between the parts, except that which is afforded by the brain, or spinal cord. In such cases we must suppose that the morbid impression travels to the brain or cord, and then the sensation is referred to the part affected through another nervous channel of communication. Dr. Wollaston was accustomed to relate the following story of himself. He had eaten some ice-cream after dinner one day; and his stomach did not seem to be capable of digesting it. Some time afterwards, when he had left the dinner table for the drawing room, he found himself rendered lame by a violent pain in one ankle. Suddenly he became sick, the ice-cream was vomited, and instantaneous relief of the pain followed its ejection from the stomach. "A gentleman (says Sir Benjamin Brodie) awoke in the middle of the night, labouring under a severe pain in one foot. At the same time certain other sensations, to which he was not unaccustomed, indicated the existence of an unusual quantity of acid in the stomach. To relieve the latter he swallowed a large dose of alkaline medicine. Immediately on the acid in the stomach having been thus neutralized, the pain in the foot left him."

The lesson that we learn from all these facts is this: that when we can find no explanation of a pain in the very spot in which it is felt, we should look for some condition that may explain it in the trunk of the nerve supplying that part; or in the parts supplied by other branches of the same nerve; or (if we are still unsuccessful) we look for other indications of disease in the brain or spinal marrow: and if these be wanting, we should extend our search, and enquire whether there be any intelligible disorder or cause of irritation elsewhere, which operating through the medium of the brain or spinal cord, may have occasioned the sympathetic pain of which our patient complains.

I say we should institute this search, because, if it be successful, it may teach us, on the one hand, that the cause of the pain is fixed and irremediable; or, on the other, it may enable us by some simple and obvious expedient to cure the pain. But sometimes we shall be quite disappointed in all this seeking. We shall find nothing, either in

the living patient, or in the dead body which throws the smallest light upon the cause of the neuralgia.

Now with respect to those neuralgic pains, for which we can discover no adequate cause, either in any diseased structure, or in any diseased action of the blood-vessels, there are certain general facts observable which I will mention before I specify any particular forms of neuralgia. They occur in all parts of the body; but they are more frequent about the head than in any other part; and next of all, probably, in the abdomen. In the head, or face, the branches of the fifth pair of nerves are very frequently the seat of neuralgia; and to such pain in that situation the name of *tic douloureux* is generally given. The painful affection called *hemicrania* is another example of neuralgia of the head. Certain forms of *angina pectoris*, and of *gastrodynia*, seem to belong to the same class of disorders: and *sciatica*—which depends on different causes in different cases—is often rather a neuralgic than a rheumatic pain. I have stated that the pains sometimes follow the track of certain nerves; but this is not, I think, very common. Inflammation of the nerve, or of its investments, generally causes pain having that property: but the truly nervous pains are much better characterized by the suddenness with which they come on, and the suddenness with which they sometimes go off also; by their intermittence in many cases, and the regularity of the period at which they often, though not always, return; by the total absence (in most cases) of heat and swelling, and often of tenderness too, when they are external, and of febrile symptoms when they are internal, even although their intensity be extreme; by their apparent dependance, in numerous instances, upon sudden changes of the weather; by their occurring chiefly in persons of a nervous temperament, in whom the health is otherwise disordered; and by their frequently abating under tonic remedies, or what are called specifics, rather than under antiphlogistic treatment (*Alison's Outlines*). There is another circumstance, characteristic of these pains, which has been mentioned by Sir Benjamin Brodie, and I do not know that the same thing has been noticed by other writers. These pains are often suspended by sleep. "A person suffering from *tic douloureux* in the face may for a time be prevented from falling asleep, but, if once asleep, his sleep is likely to be sound and uninterrupted for many hours." He says that though there may be exceptions to this rule, they are comparatively rare. Now this, you will observe, is quite analogous to what takes place in certain spasmodic affections of the muscles also. The jactitations of chorea are almost always suspended during sleep. It is the same with the spasmodic

wry neck, in which the involuntary contraction of some muscle, commonly the sterno-cleido mastoideus, drags the chin round, and the head awry. Persons affected with that sort of deformity when awake, have their necks flexible enough, I believe, while they are sleeping.

I mentioned just now one character of these neuralgic pains, viz. the total absence in most cases, when they occupy the surface, of heat, redness, swelling, or tenderness; and I said *in most cases*, because there are unquestionably exceptions to this. After these pains have been long-continued and intense, they may give rise even to a moderate degree of inflammation of the part; which will become tender to the touch, manifestly vascular, and even swollen a little. "In a gentleman," mentioned by Sir B. Brodie, "who suffered for a great length of time what was regarded as a most severe tic douloureux in the face, at first the parts to which the pain was referred retained their natural appearance, but ultimately they became swollen, from an effusion of serum into the cellular texture, and so exquisitely tender that they could not bear the slightest touch. In a patient who had laboured for some time under pain in the testicle, depending on a calculus passing down the ureter into the bladder, the testicle became tender and considerably swelled."

The attacks of neuralgia may recur at intervals of a few seconds only; or they may take place daily, or every other day; or they may be separated by much longer intervals, regular or irregular. Sometimes there is continual pain, but it is wonderfully exalted and aggravated by fits. It is sometimes described as being sharp, and sudden, and like an electric shock in its momentary duration; sometimes it is attended by a feeling of constriction and cramp, although no muscular contraction accompanies it. I suppose that is one reason why such pain is so often spoken of, even by medical men, and almost always by the vulgar, as *spasm*. Whenever a patient tells me he has spasms here or there, I am obliged to request that he will explain himself farther. I want to know how he construes spasm; and nine times out of ten I find that he intends a sudden and sharpish, and generally a transitory attack of *pain*: whereas spasm really signifies, and ought to be restricted to, involuntary muscular contractions. When a medical man prints a case in which he states that spasms occurred in such or such a part, it is impossible to tell what he means, unless that term is explained by the context. Pray avoid this inexactness.

Tic douloureux.—The most common of these neuralgic pains, as I have said, is that which has been called, κατ' ἐξοχήν, *tic*

douloureux, and which is situated in the facial branches of the fifth pair of nerves, which, as you know, are nerves of sensation: and it is usually restricted to one of the three branches that emerge severally to supply the parts in their neighbourhood. Sometimes two, sometimes all of them are implicated. The middle one of these branches, the infra-orbitary, is, I believe, the most commonly affected in the severer forms of the complaint. The torture occasioned by this dreadful malady is sometimes excessive. The sufferers speak of it as anguish that is scarcely endurable; and you see, in their quivering features and restless limbs, that the acute bodily pang is, indeed, hard to bear.

When the uppermost branch of the trifacial nerve is the seat of the complaint, the pain generally shoots from the spot where the nerve issues through the superciliary hole; and it involves the parts adjacent, upon which the fibrils of the nerve are distributed; the forehead, the brow, the upper lid, sometimes the eyeball itself. The eye is usually closed during the paroxysm, and the skin of the forehead on that side corrugated. The neighbouring arteries throb, and a copious gush of tears takes place. In some instances the eye becomes blood-shot at each attack; and when the attacks are frequently repeated, this injection of the conjunctiva may become permanent.

When the pain depends upon a morbid condition, or morbid action, of the middle branch of the nerve, it is sometimes quite sudden in its accession, and sometimes comes on rather more gradually; being preceded by a tickling or pricking sensation of the cheek, and by twitches of the lower eyelid. These symptoms are shortly followed by pain at the infra-orbitary foramen, spreading in severe flashes (so to speak) over the cheek, affecting the lower eyelid, ala nasi, and upper lip, and often terminating abruptly at the mesial line of the face. Sometimes it extends to the teeth, the antrum, the hard and soft palate, and even to the base of the tongue, and induces spasmodic contractions of the muscles of the face.

When the pain is referable to the inferior maxillary branch of the fifth pair of nerves, it darts from the mental foramen, radiating to the lips, the alveolar processes, the teeth, the chin, and to the side of the tongue. It often stops exactly at the symphysis of the chin. Frequently it extends in the other direction to the whole cheek, and to the ear. During the paroxysm the features are liable to be distorted by spasmodic action of the muscles of the face, amounting sometimes to tetanic rigidity, and holding the jaw fixed and immoveable.

The paroxysms of suffering in this fright-

ful disease are apt to be brought on by apparently trivial causes; by a slight touch, by a current of air blowing upon the face, by a sudden jar or shake of the bed in which the patient is lying, by a knock at the door, or even by directing the patient's attention to his malady by speaking of it, and asking him questions about it. This was remarkably manifest in a patient who came into the hospital under my care for another complaint; but who had for some time been subject to *tic douloureux*. The necessary movements of the face in speaking, or eating, are often sufficient to provoke or renew the paroxysm. At the same time firm pressure made upon the painful part frequently gives relief, and causes a sense of numbness to take the place of the previous agony.

This fearful disorder occurs most commonly in persons who exhibit, in other respects, the signs of an unsound, or disordered, or debilitated system. It is more apt to fasten upon those who are pale, and asthenic, and upon individuals whose powers have been broken by advancing years. It is not unfrequently attended with some obvious disorder of the digestive organs, and ceases or is mitigated when that disorder is corrected. Sometimes it is clearly connected with a disposition to rheumatic affections; coming on in persons who suffer rheumatism in other parts, and even alternating with rheumatism in other textures. It is observed to be common among fishermen, and the inhabitants of marshy districts: and in some of these sufferers it may be attributable to their habitual exposure to cold and moisture; and this nerve, lying superficially, and being unprotected by any artificial covering, is more likely, perhaps, for that reason, to be affected by vicissitudes of temperature; but in many of these cases the disease seems to be produced by the *malaria*, which is prevalent in those situations. The paroxysms are then not only intermittent, but periodical, and they will frequently yield to the remedies which have been ascertained to be specific against ague and its various modifications. Sometimes the facial neuralgia is evidently dependant upon some general state of the system; for it will cease in the face, and fix itself in some other place; and in this way it may come to occupy several distant parts of the body in succession. There are other cases again in which the disease has a local origin, and results from some diseased bone, or exostosis, in the neighbourhood of the painful spot. The late Dr. Pemberton afforded a well-known example of this. He was seized with *tic douloureux* in the very zenith of his reputation, and when he was in the fullest practice of his profession in this town. It completely ruined him: compelled him to give up business. He ulti-

mately died of apoplexy. When his head was examined after death, the *os frontis* was found to be unusually thick, and on the falciform process of the *dura mater*, at a little distance from the *crista galli*, a small osseous substance was discovered, nearly half an inch long, and almost as broad. Sir Henry Hallford has recorded several other instances in which the disease was connected with some morbid condition of the bones of the head or face.

Now *tic douloureux* is one of those complaints for the cure of which there exists a number of specific remedies. But what I have been stating of this disease will suffice to convince you that, as it depends upon different causes in different persons, it is absurd to expect that any single drug—or even one plan of treatment—will always remove it. Our first care, in every example of it that comes before us, must be to investigate all the particulars of the case. We must not be satisfied with learning that the complaint is *tic douloureux*, and then go on prescribing one after another the reputed specifics for *tic douloureux*. It may happen that the origin of the disease is plain, and the remedy obvious. We must endeavour to make out whatever is amiss in the system at large, or in the state of particular functions. Very rarely, I believe, *tic douloureux* is dependant upon a condition of general plethora. Mr. John Scott gives the case of a gentleman who suffered severely from it for some time; at length he had an attack of apoplexy, and for this last disorder he was largely bled, and the bleeding seemed to cure the neuralgia. Much more frequently we find evidence of a feeble or a shattered state of the system; debility and paleness: and then we may expect to do good by the treatment so strongly recommended by Mr. Hutchinson, viz. by giving the carbonate of iron. This remedy has been put largely to the test, since Mr. Hutchinson wrote in commendation of it, by Dr. Elliotson, and subsequently by others. Dr. Elliotson states it as the result of his experience that, “in all cases of neuralgia, whether exquisite or not, unaccompanied by inflammation, or evident existing cause, iron is the best remedy.” I have already explained the manner of administering the carbonate of iron, the doses in which it may be given, and the limits within which I should be inclined to restrict the doses. Sir Benjamin Brodie thinks it probable that the carbonate of iron proves beneficial by its mechanical operation on the internal surface of the intestines: but I should rather ascribe its good effect to the well-known property of preparations of iron, to give firmness to the nervous system; apparently by increasing the quantity of red blood that circulates in it. However, it is

of the utmost consequence that the state of the digestive organs should be attended to. Mr. Abernethy used to relate, in his lectures, many instances of tic which he had been successful in curing by measures which were solely directed to the improvement of the stomach and bowels. He had a notion, that in patients who suffer under this disorder, there were always two functions wrong; those of the nervous system on the one hand, those of the digestive system on the other. And I am sure you will commonly find indications of a faulty state of both these systems. "The two," he used to say, "were the common parents of a numerous progeny of very dissimilar local diseases. In tic douloureux, you must seek to put the digestive organs right, or to soothe the nervous system, according as the one or the other may seem to be the principal and primary cause of the disease. Take away one of the parents, and there will be no more propagation."

In these cases, the unhealthy state of the digestive apparatus may be marked by obvious signs; a furred tongue, loss of appetite, costive bowels: or it may reveal itself by no other symptoms than the pain. It may depend upon the mere presence of acid in the stomach. Dr. Rigby tells us that having suffered in his own person an intense attack of tic douloureux, which opium did not assuage, he swallowed, at the suggestion of a friend, some carbonate of soda dissolved in water. The effect was almost immediate: carbonic acid was eructated, and the pain quickly abated. More often the cause of offence appears to lie in some part of the intestines; and purgatives do good. Sir C. Bell—drawing a bow at a venture—achieved the cure of a patient, upon whom much previous treatment had been expended in vain, by some pills composed of cathartic extract, croton oil, and galbanum. He mixes one, or two, drops of the oleum tiglii, with a drachm of the compound extract of colocynth; and gives five grains of this mass, with ten grains of the compound galbanum pill, at bed-time. I mention the exact proportions and dose, because other cases have been since reported, both by Sir Charles and others, in which the same prescription was followed by the same success.

When the disease occurs in a rheumatic individual, and especially when, as is sometimes the case, it alternates with rheumatism of other tissues, the remedies which have been found useful in rheumatism deserve a fair trial: guaiacum; colchicum; calomel and opium.

When all has been done that can be done towards restoring or improving the general health, we may turn our thoughts to local remedies. It is plain that these must be

inefficient when the local pain results from constitutional causes that are unredressed, or perhaps incurable. Yet even then topical measures may soothe the pain for a while.

One of these topical expedients, which promised well when first thought of, is the division of the trunk of the painful nerve, so as to cut off the nervous communication, through that main channel at least, between the painful part and the brain. This was originally proposed by Dr. Haighton, and was at first attended with some little success; but in a great number of instances it has signally failed, as indeed might have been expected. In Dr. Pemberton's case the several branches of the fifth pair were cut by Sir Astley Cooper: but in vain. When there is any reason to think that the disease has a constitutional origin, or a local *distant* origin, the division, or even the excision, of a part of the nerve must be perfectly useless: it would be as reasonable (as Mr. Abernethy has observed) to expect to cure gout by cutting the nerve that goes to the great toe: or to perform castration with the view of remedying that pain in the testicle which is apt to be produced by the passage of a calculus through the ureter. Nevertheless there are cases, in which the division of the nerve, or some other surgical operation, is required. If you can make out that there is any tumor pressing upon or adherent to some part of the nerve—or if some foreign body, as a splinter, or a shot, should be ascertained to be in contact with the surface of the nerve, or to be entangled in its substance—the tumor or the foreign body may be removed by the knife, with the strong expectation that a cure will be thus effected. And if this cannot be done, or if the nerve itself be altered in structure, either from disease or injury, (I am referring now to neuralgia in general, and not merely to that in which the facial branches of the fifth pair of nerves are implicated), under those circumstances it will become a very proper subject of deliberation whether the nerve should be divided, or the limb amputated.

In the Medical and Physical Journal there is a case described by Mr. Jeffries, of a violent facial neuralgia, cured by the removal of a small fragment of china, which had been lodging there for 14 years. And Mr. Descot mentions an instance in which a very severe affection of 10 years' standing was removed by the extraction of a carious tooth. I saw, not many days ago, a young woman whose finger had been amputated for very acute neuralgic pain which she had suffered in it; and the amputation had been successful in liberating her from that pain.

Sometimes we may hope to afford relief to the suffering patient by means which tend to remove or lessen the *exciting* cause of the

paroxysms. Of this I may mention one remarkable example, which fell in part under my own observation; although I had nothing to do with the treatment. I was asked, a few years ago, by a friend, to go with him to call upon a relation of his, who laboured, he said, under *tic douloureux*: he did not wish me to see her professionally, but was desirous that I should witness what he considered an extraordinary complaint. I saw a young girl, about 12 or 13 years old, very pale and delicate, lying on a sofa; and I learned from her and from her mother that she was subject to the most excruciating agony in one side of her face and neck. The pain came on whenever she swallowed any thing: the action of deglutition proved invariably the exciting cause of the torment. She was at that time under the care of a practitioner who had desired that she might eat mutton-chops three or four times a day. Of course this was a sentence full of misery to her; but so desirous was she to get rid of her disease, that she resolved steadily to follow the directions that were enjoined. This plan was to be tried for at least a month; after that time, if she were no better, her mother had resolved to consult another practitioner who had been much recommended to her. I should say that she had already consulted a great number of medical men; for the malady had existed nearly two years. At the end of the month she was worse than at the beginning; and the new practitioner, Mr. Pennington, was called in. He acted, like a man of sense and sagacity, upon the fact that the act of swallowing always gave rise to the pain; and he advised that she should not attempt to swallow for 24 hours. That period passed without any return of the pain; but it immediately recurred upon her eating a morsel of bread. The result of this experiment, however, encouraged him to hope that the morbid habit might be broken through by a sufficiently long abstinence from swallowing. And as she had been subjected to a great variety of fruitless treatment, he gave her no medicine, but advised that she should refrain altogether from taking food or drink by the mouth. Nourishing injections composed of beef tea, with an egg beat up in it, or of milk, were thrown into the rectum, two or three times a day. This plan was persisted in for a longer time than I should have supposed she could have endured it. No nutriment whatever was taken by the mouth for five weeks and three days, and no paroxysm of pain occurred. At the end of that period the pulse sank suddenly, from between 70 and 80, to 35 beats in a minute; and thereupon Mr. Pennington thought he had carried his experiment far enough; and deemed it advisable to administer by the mouth a

dessert-spoonful of beef-tea twice a day. This was continued for four days without producing any return of the pain. A small piece of fish was then allowed, and afterwards some chicken; and proceeding thus cautiously, in the course of a month she was able to eat and drink any thing, without the slightest inconvenience.

I should state, however, that some time afterwards the neuralgia returned in another situation, and affected the left knee: and this was remedied by a different mode of treatment. She is since dead.

When other means fail, or in conjunction with other means, local applications to the affected part may be tried. Belladonna will sometimes materially palliate the pain; so will opium: but within the last few years a new anodyne has been brought into use; and it really seems to have been of essential service in several instances of this most painful disorder: I allude to *aconitine*: the active principle of the monkshood. The property belonging to this plant, of benumbing sensation, has long been known. Sir Benjamin Brodie found many years ago that after chewing its leaves, a remarkable numbness of the lips was left, which lasted some hours. We may understand therefore the beneficial operation of the aconitine upon a part of which the sensibility is unduly exalted. It is only very recently that pure aconitine has been procured; and consequently it has not yet been very extensively employed, and the less so on account of its very high price: but what experience we have of it, as a benumber of pain, is highly encouraging. It has been of singular benefit to a surgeon who formerly lived in Charterhouse Square, and whose case is well known, I believe, to the profession. Mr. Spry had suffered greatly, for eight years, under very acute neuralgia affecting the parts supplied by the lowermost or mental branch of the fifth pair of nerves. After exhausting almost every expedient that ever has been recommended for *tic douloureux*, except that of dividing the nerve, he was induced to make trial of the aconitine. It was mixed with cerate, in the proportion of one grain to one drachm, and a small portion of this was smeared over the track of the painful nerve once or twice a day for six days. By that time he had entirely lost the pain. He states, I understand, that the application of the ointment produced a sense of numbness, which continued for 12 or 18 hours. Dr. Hue, who first told me of Mr. Spry's case, told me at the same time that he knew of two others in which the same application had been equally successful. This encouraged me to try it upon my patient, whom I mentioned before, and who happened at that time to be in the hospital. I bought five

grains of the aconitine at Morson's, in Southampton Row, where I knew it would be genuine, for ten shillings. One-third of a drachm of ointment, containing one-third of a grain of the alkaloid, was smeared two or three times a day over my patient's face, and the attacks presently diminished in intensity, and in a few days ceased altogether. He soon after left the hospital, so that I cannot tell whether the cure was permanent. I presume it was so, as he did not return. The particulars of Mr. Spry's case have been published by Mr. Skey in the 19th volume of the *MEDICAL GAZETTE*. It is now six years since the aconitine was applied, and the pain (as Mr. Skey has recently informed me) has never recurred. It used to be excited by gentle friction of the hand, or by a current of cold air, but Mr. Spry "can now face any wind or temperature with impunity." In the same paper M. Skey relates another instance of the utility of this substance in facial neuralgia. It occurred in one of his patients at St. Bartholomew's Hospital.

This is a remedy therefore which is not to be neglected. Even if it only allayed the pain for a time it would be highly valuable. But, judging from the instances now referred to, we may hope that, in some forms of tic douloureux, the aconitine may be found equal to their cure. It seems probable that the recurrence of the pain is sometimes kept up by the influence of habit; and will cease if the habit can be broken for a while. You must take care, however, to obtain a genuine article: the manufacture of aconitine is difficult, and therefore the cost is considerable. Mr. Skey, in the Bartholomew case, failed with some aconitine that had been imported into this country, but succeeded at once when he employed the same quantity as it is prepared by Mr. Morson.

A few years ago Mr. John Scott published a little book on the disease we are now considering, with the professed object of introducing to general notice a species of local treatment which he had found successful in several long standing and previously obstinate cases. It is well to be aware of these things, though probably the aconitine ointment will beat Mr. Scott's. Mr. Spry used Mr. Scott's ointment, but without benefit. It consists of the iodide of mercury, mixed with lard, in the proportion of two scruples to the ounce: and it is rubbed into, or placed in contact with, the affected surface, until some degree of irritation is produced.

There is a kind of face-ache, which cannot properly be included as a species of neuralgia, for it does not occur in short stabbing paroxysms, nor is the pain acute enough to entitle it to the name of tic douloureux; but

which is very common, very distressing, and under ordinary treatment sometimes very intractable. It is called often a rheumatic pain; it occupies the lower part of the face, the jaw principally, and the patient cannot say exactly whereabouts it is most intense. It is often thought to proceed from toothache, and bad or suspected teeth are extracted, but with no good effect. Now I allude to this for the sake of saying that some years ago I was told by an experienced old apothecary, that this face-ache might be almost always and speedily cured by the muriate of ammonia;—a medicine that we seldom give internally here, although it so much used in Germany. And I have again and again availed myself of this hint, and been much thanked by my patients for the good I did them with this muriate of ammonia. It does not *always* succeed; but it *often* does. It should be given in half-drachm doses, dissolved in water, or in almost any vehicle, three or four times a day. If the pain does not yield after four doses, you may cease to expect any benefit from it. In two or three instances of a similar kind that I have recently had to treat, I have found the iodide of potassium, in doses of five or six grains, work a speedy and permanent cure. This induces me to suppose that the pain in some of these cases is periosteal; judging from the ascertained efficacy of the iodide in other periosteal affections attended with pain.

Sciatica.—Tic douloureux is the principal form of severe neuralgia which you may expect to meet with, in regard to acuteness of suffering and difficulty of cure. Two other forms, more common, and luckily more manageable, are generally spoken off under the same head: *sciatica*, namely, and *hemicrania*. I have very little to say, in this place, of either of these. Sciatica, or pain radiating from the sciatic notch, and following the course of the sciatic nerve, is sometimes an inflammatory complaint, and yields to the remedies of inflammation—bleeding and blistering: sometimes it is plainly a part of rheumatism; and then may often be relieved by calomel and opium, or by colchicum: sometimes, again, it results from irritation within the pelvis, affecting the nerve before it emerges externally; this irritation may be connected with a disordered state of the kidney, and I suspect that it is in such cases that the oil of turpentine is of so much use: lastly, it is sometimes a purely nervous and neuralgic pain; and then the treatment applicable to facial neuralgia will, *mutatis mutandis*, be applicable to it. I had some time ago a butler under my care at the hospital, whom I am

afraid I did not manage well. He suffered severe sciatica, and I had him cupped and blistered, and gave him a variety of medicines, for some time, to little purpose: at last he got what I ought, I suppose, to have given him at first, viz. the carbonate of iron, and was presently well.

Hemicrania. — *Hemicrania* is simply headache, confined to one side, and occupying generally the brow and forehead, but sometimes affecting very exactly one moiety of the head. It is the *migraine* of the French, the *megrin* of our vernacular language; each of these terms being obviously traceable to the same Greek root. It is often attended with sickness; and in many instances it is periodical, coming on every day at a certain hour, lasting a certain time, and then subsiding. Like the other forms of neuralgia, hemicrania may be produced by various causes, which are, however, almost all of them such as tend to debilitate the system: it sometimes occurs in connexion with hysteria; sometimes it plagues women who have suckled their infants too long; sometimes it acknowledges the same cause as ague; and sometimes also it occurs independently of all other disease, and when no obvious exciting cause can be traced.

Whatever may be its origin, it is usually a very manageable complaint. When it is associated with evident anæmia, steel and the shower bath may be expected to cure it. When its visits are strictly periodical, it will yield to quinine. Arsenic is considered by many to have a specific power over the complaint; and I believe that four or six drops of the liquor arsenicalis, given three or four times a day, with due attention to the state of the bowels, will be almost sure to remove hemicrania in nine cases out of ten in which it occurs. But steel or bark, being milder and safer drugs, are, *ceteris paribus*, to be preferred.

I say this complaint often acknowledges the same cause as ague; namely, the miasm of marshes, or malaria: and as that cause, mysterious as it is in some respects, exerts apparently its primary or chief influence upon the nervous system, and as ague has no definite seat in the human body, if it be not in the nervous system, I shall not find a more convenient place in these lectures for the consideration of ague than here, at the close of the remarks which I had to make respecting the diseases of the brain and nerves. In the next lecture, then, I shall begin to speak of Intermittent Fever.

CLINICAL LECTURES,

By DR. CORRIGAN,

*Delivered at the Hardwicke Fever Hospital,
Dublin,*

During the Session, 1840-41.

LECTURE VIII.—FEVER, No. 6.

Lesion of function of respiration; two sources of the lesion. Pulmonic derangement and nervous derangement; diagnostic marks of each. Cases of Johnson and Harrington. Treatment. Lesions of pulmonic circulation; determination of its nature. Estimate of physical signs. Case of Toner. Analogy as a guide. Treatment. Blisters; their mode of action.

WE proceed now to turn our attention to the state of the functions of respiration and circulation in fever. In examining the state of the function of respiration of a patient in fever, always recollect there are two main sources from which its lesions may take their origin. First, the state of the lungs themselves, as to congestion; second, the state of the respiratory muscles. The lesion of the function may be equally great, from whichever of these sources it may take its origin; but there is a wide difference as to the amount of danger hanging over the patient. Let us suppose two patients, on the fourteenth or fifteenth day of fever, both breathing with great labour—in both the respiration as rapid as sixty in the minute; there yet shall be a far greater amount of danger in the one case than in the other. In the one the rapidity and labour of respiration are proceeding from congestion of the lung, and effusion into the air tubes; in this the danger is very great. In the other the rapidity and labour of respiration proceed from the state of the respiratory muscles, and of the nervous system presiding over the action of the muscles. We may, for the sake of distinction, call the first deranged pulmonic respiration; the second deranged nervous respiration. The distinction between those two kinds of deranged respiration is well marked: we had an instance of the first kind in the case of Johnson and of Harrington. In those two cases the respirations were from fifty-six to sixty in the minute, and very laborious: both diaphragm and respiratory muscles laboured very much. To this was however superadded a congested dark expression of face and lips, indicating that aeration of the blood in the lungs was materially interfered with; and in Johnson's case there was expectoration of mucus tinged with blood. If the stethoscope be applied in

such cases, there is mucous and crepitating rattle in the depending portions of the lungs. In this form of respiration, if the sound of it become audible to your ear as you stand at the bed-side, without approaching the patient more closely, it is always accompanied with a rattle, indicating that effusion to a considerable amount has taken place into the air-tubes. In the second form, or what I have designated as pervious respiration, the labour and rapidity of respiration are often apparently even greater than in the other; but there are some very distinctive marks. The countenance does not present the peculiar livid hue which belongs to the first form. It is sometimes pale, sometimes flushed, just as it may chance to be from natural hue or accidental accompanying circumstances; but there is no necessary connection between the state of countenance and the labour and rapidity of circulation. The respiratory action, also, is carried on principally by the intercostal and supplementary respiratory muscles; while in the first form the diaphragm and abdominal muscles are more proportionably engaged. There is another very marked character of this respiration: it is often so loud that you can hear it at a considerable distance from the bed; in some cases this amounts to a perfect blowing, so that in expiration the cheeks are distended, while the nostrils are dilated, and respond in their action to the rapidity and labour of the intercostal muscles; the mouth is often kept closed, the lips firmly compressed, unless in the act of expiration, when, from the lips not parting sufficiently, the blowing out of the cheeks I have already noticed is produced. The loudness of the respiratory murmur, audible even at a distance from the bed, is produced principally by the rapid motion of air along the internal nares in the act of inspiration, and by the blowing through the mouth in the act of expiration. The sound which is thus produced by this laboured and rapid respiration is the simple murmur produced by the rapid passage of air alone. It is very different from the rattle produced by the admixture of air and fluid noticed as belonging to the first form. It is, in fact, closely analogous to the audible respiratory sound produced by the panting respiration of a person who has made some continuous violent exertion. It is not only frequent, but quick; that is, the respirations are considerable in number — within the minute amount to fifty or more; and *each* separate act of inspiration is rapid. It is, in fact, the respiration of great exertion; and if the stethoscope be applied to the chest in such a case, we find in it very intense puerile respiratory murmur. I have said nothing of the state of the pulse as furnishing a diagnostic sign between those two kinds of respiration,

because it furnishes none. The pulse may be equally quick, or equally feeble, in both forms: the latter, the nervous respiration, never of itself proves fatal. Whenever the patient affected by it dies of a lesion of the respiratory organs, it is always by its passing into the second form, or the pulmonic respiration, in which the circulating vessels become congested, and the air-cells obstructed, and the patient finally dies from mechanical obstruction, in the air-vesicles, to the progress of respiration. You remember Johnson's case, a woman in the female ward: the ninth day of her fever the first form of respiration began, and continued up to the sixteenth. The symptoms, as detailed in Mr. Sunter's notes of the case, which are very full and accurate, are that her face became congested, her lips bluish, her lower jaw gaped, and her respiration was sixty-six, and abdominal; there were mucous rattles and sonorous murmurs over the chest; she had a symptom also which removed all doubt as to the nature of the lesion, in this case, of the function of respiration; her expectoration, when she was able to get it up, was mucous, frothy, and tinged with blood.

Another similar case was that of Harrington. On the twelfth day of her fever she had mucous rattle and sonorous murmur over the whole chest, the respiration was fifty-six, and, as in Johnson's case, abdominal and very laborious. Those cases resemble one another so much that it is unnecessary to multiply examples. In the case of Geoghegan, on the thirteenth day of his fever, and for a day or two longer, we had a good example of the second kind of disordered or nervous respiration. His respirations were rapid, his *alæ naris* distended, and his breathing resembled exactly such as you would see in a person after any violent or long-continued exertion. This form of respiration is the more unfavourable the earlier it comes on. In the case of one of the Herberts, brothers, admitted in the early part of the session, this form of breathing was well marked even on the day of admission, the second day of his illness. We have a very well-marked case of this kind in the case of Matthews, at present in hospital. He is a man of a fresh complexion, and from the tenth "up to the fifteenth" day of his fever, presented this nervous respiration very well marked. His pulse was 124, barely perceptible; his respiration fifty-four in the minute, principally performed by the intercostal and thoracic respiratory muscles. With this rapid breathing his lips were compressed; his nostrils dilated, and acting strongly; and the rapid motion of the air through the nares produced an audible sound, which, coupled with the other characters of the respiration, make his breathing resemble

very much the panting respiration of a person who is breathing quick and hard after some rapid exertion. His respiration was yesterday running into the second form, and under this complication his case is most likely to terminate fatally. This form is always indicative of debility; and it is the more dangerous the more early it sets in, for it shews the patient has to contend against much nervous debility in the progress of his fever. Still, let this second form of respiration be ever so rapid, ever so laborious, and ever so loud, there is yet a chance for the patient's life, for the rapidity and labour of respiration are dependent only on the deranged or debilitated action of the respiratory muscles, from which recovery is not only possible but probable; but let the same amount of rapid and laborious respiration arise from mechanical obstruction in the air-tubes, as indicated by mucous rattle in them, and the chance of recovery is very poor indeed.

I believe it may be laid down as a general rule, that wherever there is the nervous respiration described above present, the patient requires wine. I can remember no exception to this rule. In all the cases which I can recollect of this form of respiration we gave wine freely, and always with the best results. Wine, and the remedies, such as camphor and ammonia, which belong to the same class, seem to constitute the means best adapted to meet this derangement of the respiratory function. If it continue beyond one or two days, there gradually creeps on the complication which I have called the pulmonic derangement, or this latter may be the affection from its commencement, and as this is intimately connected with, or immediately dependent on, the derangement of the function of circulation, we must devote some observations to this, and more particularly where, as in the present instance, its derangement is occurring in so important an organ as the lungs.

There is a variety or second form of nervous or cerebral respiration, which indicates very great danger; it is when the respirations are very slow and very deep, not perhaps reaching 20 in the minute, and the chest both rising to a full height, and also sinking deeply. This form accompanies great cerebral oppression, and does not call for the administration of wine, like the other form. If we were, without due consideration, to decide on the nature of such a case as Johnson's or Harrington's, we should probably at once pronounce it, from some of the symptoms, and from all the physical signs, to be a severe and acute case of pneumonia, or of pneumonia complicated with acute bronchitis complicating fever. There is mucous and bloody expectoration, and

dulness on percussion of one or of both sides of the chest in the inferior portions, the part in which you are generally taught to look for pneumonia; and there is in the same situation crepitating rattle, with very often sonorous and sibilous respiration over the remainder of the thorax. Let us, however, first estimate the real value of the physical signs, or those signs afforded by percussion and auscultation. Those signs, recollect, only tell us of the existence of physical causes sufficient to produce the physical signs. They tell us nothing of the *vital state* of the vessels giving rise to the physical causes. Let me explain a little more in detail. There is a case of maculated fever, such as Johnson's was, or Harrington's, or many more that we have had, and on making a physical examination there is found some dulness on percussion, that indicates a certain amount of congestion in the capillaries, a state of the lung in which there is a greater proportion of blood than air in that particular portion. There is crepitating and mucous rattle over the same extent, that tells us there is in addition an effusion of fluid into the air-vessels. There is even in addition, as in Johnson's case, viscid expectoration streaked with blood, that confirms us in our opinion of there being effusion into the bronchial tubes; but we can go no further than this on the information given to us by the physical signs. It is at once obvious that the physical signs would be just the same from the most opposite vital states of the circulating and secreting vessels of the lungs. Thus, whether the distension of the capillaries arose from a state analogous to acute inflammation, or from a state of extreme debility permitting their distension, the dulness would be equal in amount, for it is their *physical* not their *vital* state that causes the dulness. In like manner of the effusion, whether the effusion into the air-tubes be from increased power of the secreting vessels, as in acute bronchitis, acute pneumonia, &c. or of a passive nature, the mixture of that effusion, whatever its nature, in the air-vesicles with air, would, it is obvious, equally give rise to crepitating rattle. The expectoration, as to containing blood, would be equally deceptive, for the transudation of blood may be owing to debility or to increased action. We dare not, then, draw our conclusion as to treatment from the physical signs alone. The physical signs tell us merely of the existence of physical causes sufficient to produce those signs: they tell us nothing of the state of the vital actions going on in the lungs, and to which the physical causes owe their origin. Dr. Stokes, in his admirable work on Diseases of the Chest, has made the following judicious and well-pointed observations on the manner in which physical

signs are to be considered and estimated :—
 “It cannot be too often repeated that physical signs only reveal mechanical conditions, which may proceed from the most different causes ; and that the latter are to be determined by a process of reasoning, &c. &c. Without this power I have no hesitation in saying that it would be safer to wholly neglect the physical signs, and to trust in practice to symptoms alone.” The cases of Gray and Connor, detailed in our last lecture, fully bear out these observations as to insufficiency of physical signs to determine on the nature of the derangement of the vital action which exists in the function of circulation in the lungs. The distension of the capillaries or circulating vessels may be either from debility of those vessels permitting themselves to distend, no longer possessing power to contract on their contents, or it may be from an opposite state. I do not mean to say that even active inflammation may not occasionally accompany our maculated fever ; but I believe it will be evident, on a little consideration, that, in the immense majority of cases, as they at present come before us in this epidemic, the state of the capillaries is one of passive distension—a state requiring the administration of wine, rather than the use of remedies of an opposite character. We have already estimated how far we dare trust to physical signs. Let us now turn to an examination of the function of circulation in other particulars. You remember the case of Toner. He was admitted on 25th February, on the eighth day of maculated fever, with suffused eyes and dark maculæ ; his pulse 108, very weak. He was put on wine. Now what was the result ? That under the administration of wine, in very large quantities, on the thirteenth day the suffusion of eyes began to disappear, and, on the sixteenth day, he was convalescent. If the vessels of the conjunctiva in this case had been congested from being in a state of active or sthenic inflammation, it is not easy to conceive how they could have returned to their healthy state under the influence of wine. Is it not rational to look upon the state of the circulation in the lungs as partaking of the same nature as its derangement in the conjunctiva ? We find, as in this case of Toner, a wretched pulse and dark maculæ. We see congestion of the vessels of the conjunctiva. It is more rational to suppose the capillary circulating vessels to be in nearly one state through the whole body, than to suppose that their state in one organ is the very opposite of their state in another. Let us look at the maculæ on the surface of the body in Matthews' case. They were very dark in colour, and his eyes, too, were congested : this was on the tenth day of his admission ; with these his pulse was 124, barely perceptible. He got sixteen ounces of wine. On the next day the

maculæ were brighter in colour ; but the pulse was still so weak that it could not be counted. The wine was continued, and, on the thirteenth day, the pulse is reported 130, skin warm, and maculæ bright in colour. We have now the state of the vessels of the conjunctiva and of the small vessels of the maculæ to guide us as to the state of the general function of circulation. Let us add to this the state of the pulse, indicating with what feebleness the blood is propelled along even the larger arterial tubes, and it is, I think, not easy to resist the conclusion that the state of the function of circulation, as a whole, is one of general debility. Let us look at what occurs to the circulation in this maculated fever in any part where it meets even slight embarrassment. It is unable to sustain the mere pressure of the hip, to struggle against even the slight oppression produced by gravity ; and hence we find patches on the hips becoming, if not carefully watched, gangrenous, and the depending portions of the body, without pressure at all, becoming dark-red from the depth of the congestion. We thus see the vessels of the conjunctiva, the star-like circle of vessels which constitute the maculæ, the vessels of the depending portions of the skin, all distending from debility, all recovering their contractile and circulating power under wine ; the eye becoming clear, the maculæ becoming bright, the depending portions of the skin regaining their natural hue : and when we see the whole function of circulation, as far as we can examine it in external parts, undergoing those changes, we are surely justified in considering that (as its lesion is a lesion of function) its changes follow the same laws, and its treatment is to be on the same principles, whether the congestion we have to treat be in the vessels of the conjunctiva, the capillaries of the maculæ, or the circulating vessels of the lung. Thus it will follow that, in considering the lesion of circulation so common in the lung in this fever, we are to look upon that local lesion but as a part of the general lesion of the function of circulation ; and that we must correct the evidence we obtain from physical signs of the state of the local circulation by symptoms drawn from every other part of the state of the function of circulation as a whole. This consideration is very important, not only in the progress but in the very commencement of our fever ; in some of our cases it has not unfrequently happened that on admission, while the patient is still under the depressing influence of the attack, the capillaries of the lungs will be congested, and the physical signs, as far as they go, indicate pneumonia. Depletion might be practised, and injuriously, in such cases. Many such instances have occurred to us where, after a few days, under a totally

opposite line of treatment, the physical signs of pneumonia have disappeared. The same observations made on moderate local depletion, when speaking of the disturbance of the cerebral system, apply to internal lesion of the function of circulation. The external and local application of a few leeches over the seat of a local engorgement is quite compatible with the internal exhibition of remedies, such as wine, and bark. The good effects of successive relays of blisters are, in a great measure, referrible to their effects on the function we are now considering—circulation. You would have a very limited idea of their use, if you suppose they act as mere counter-irritants. Looking upon the function of circulation as it were a whole, undergoing the same morbid depressions, we give increased energy by stimulation to a certain number of capillaries. On their continuance of action organic life depends; the increased energy thus given to a portion is propagated through the function; the effect on the circulating vessels generally is like that related in Gendrin's experiments, on the web of the frog's foot, when under the microscope; the circulation is seen stagnating, the fluid becoming dark, the vessels distending, and at length ceasing to carry on their action. If a stimulus be applied, the vessels are seen to contract, the circulation becomes more energetic, the colour becomes brighter, and increased vigour of circulation and of vitality is the result. So it is with the capillaries or function of circulation in fever; and hence it is, as in such a case as Johnson's or Harrington's, or many others, blister after blister was applied for days in succession; now to the chest, then to the legs, then to the thighs, again to the chest, leaving each blister on long enough only to produce increased action, but keeping it up by a continued succession. Hence, in that form of fever where the pulse is very feeble, the function of circulation feeble, and the capillaries ready to run into passive congestions, our axiom should be—quick, short, and repeated blisterings. It does not matter very much as to the parts to which we apply them.

DR. FOWLER ON GALVANISM.

To the Editor of the Medical Gazette.

SIR,

I HAVE seen in the Gardener's Magazine for this last month (April), an advertisement of a "new discovery" (a galvanic plant protector), formed of broad bands of zinc and copper, so placed in relation to each other and the stems of plants, that snails and cater-

pillars attempting to crawl over them with their moist surfaces are intercepted by the metallic sensation, probably similar to that which is felt on the tongue when in contact with zinc and silver.

The experiment which suggested this ingenious application of zinc and copper is thus stated, page 225, Gardener's Magazine for April:—

"If a snail or slug be placed on a plate of zinc, to which a narrow plate or strip of copper is fixed near the edge, and the zinc turned over it so as to form a ring of zinc, copper, and zinc, it creeps unmolested on its surface. But as soon as it touches the rim where the copper is, it receives a galvanic shock, its moistened soft body acting as moist cloth (between plates of zinc and copper in a pile), and thus forming the galvanic circle complete, and immediately recoils, twisting itself back, and rarely venturing a second time to touch the copper, to receive another shock."

In the year 1793, while yet an undergraduate in the University of Edinburgh, I published the following experiment, with many others, on the nerves, senses, and muscles:—

"I had laid a leech on a crown-piece of silver, placed in the middle of a large plate of zinc. The animal moved its mouth over the surface of the silver, without expressing the least uneasiness; but having stretched beyond it, and touched the zinc plate with its mouth, it instantly recoiled as if in the most acute pain, and continued thus alternately touching and recoiling from the zinc, till it had the appearance of being quite fatigued. When placed wholly on the zinc it seemed perfectly at its ease; but when its mouth came in contact with the silver lying on the zinc, the same expression of pain was exhibited as before. With the earth-worm this experiment succeeded still more decidedly; the animal sprang from the zinc in writhing convulsions. If, when the worm stretched itself forwards, one of its folds lit upon the zinc, it expressed little uneasiness in comparison of what it shewed when the point of its head touched the zinc. These extraordinary effects were, however, considerably different from those produced by the metals on the limbs of frogs and other animals. They had not so much

the appearance of involuntary instantaneous convulsions, as long-continued expressions of pain and disgust, such as are produced by applying zinc and silver to the tongue of a child: a strong presumptive proof, in my opinion, that these animals are endowed with a most exquisite organ of sense; and consequently not, as had been supposed, destitute of a system of nerves*."

I have been so much out of the way of all literary transactions for nearly half a century, that I am afraid I may be making an unreasonable request in begging the favour of you to publish this unvarnished statement of facts in your very correct and impartial journal.

To the useful application of the experiment I can have no claim; nor should I trouble you with this proof of priority of observation of its results, were it not one of a series in an investigation of phenomena connected with the fifth pair of nerves.

Some part of this subject I communicated to Section E, of the British Association, at Glasgow, last autumn, a short notice of which may be found in the volume of Reports, just published.

Nearly the whole of the experiments alluded to were repeated in the presence of gentlemen yet alive, and who still permit me to call them friends: Mr. Allen, Master of Dulwich College, Professor John Thomson, of Edinburgh, and Dr. A. Monro. As I was at that time a pupil of the late Mr. John Bell, the experiments were probably seen also by Sir Charles Bell. The experiments succeed best when snails, worms, and metals, are wet with rain water.

Far be it from me to impute intentional plagiarism to the author of this clever application of an experiment so similar to mine, made in 1793. Nothing is more likely to happen than that men not in communication with each other, but occupied in similar investigations, should adopt like means, and arrive at like results.

Professor John Robison, a lecturer on natural philosophy in 1793, sent me a letter, of which I inclose you a copy. As it may interest those who have not

met with it, to see how near Professor Robison was to the discovery of the pile, which immortalized Volta, you may perhaps think it worth while to republish the letter.

I have the honour to be, sir,

Your obedient servant,

RICHARD FOWLER, M.D.

Salisbury, May 10, 1841.

To Mr. Fowler.

Edinburgh, May 28, 1793.

Sir,—About a fortnight ago, my son told me of a curious experiment, with a piece of zinc and a piece of silver applied to the tongue, by which a strong irritation, resembling taste, was produced, and that a luminous flash was excited, by applying one of them to the eye. I immediately repeated them according to his directions, and my curiosity was greatly excited to prosecute them in a variety of circumstances. I understand that these experiments have originated from the curious discoveries made some time ago in Italy, of which I was informed last winter. But I have been so much out of the world for some years past, that I have had no opportunity of knowing what was going forward.

Being informed, that you have been long engaged in experiments on this subject, and are about to favour the public with an account of them, I have taken the liberty of communicating to you a few facts which have occurred to me, some of which, perhaps, may be new to you.

1. I find, that if a piece of zinc be applied to the tongue, and be in contact with a piece of silver, which touches any part of the lining of the mouth, nostrils, ear, urethra, or anus, the sensation resembling taste is felt on the tongue. If the experiment be inverted, by applying the silver to the tongue, the irritation produced by the zinc is not sensible, except in the mouth and urethra, and is very slight. I find the irritation by the zinc strongest when the contact is very slight, and confined to a narrow space, and when the contact of the silver is very extensive, as when the tongue is applied to the cavity of a silver spoon. When the zinc touches an extensive surface, the irritation produced by a narrow contact of the silver is very distinct, especially on the upper side of the tongue, and along its margin. This irritation seems to be mere pun-

* Experiments and Observations relative to the Influence lately discovered by Galvani. By Richard Fowler.

gency, without any resemblance to taste, and it leaves a lasting impression, like that made by caustic alkali.

2. If the zinc (finely polished) be applied to the ball of the eye, the brightness of the flash seems to correspond with the surface of the contact of the silver with the tongue, palate, fauces or cheek. The same thing happens when the silver is applied to the eye.

3. When a rod of zinc, and one of silver, are applied to the roof of the mouth, as far back as possible, the irritations produced, by bringing their outer ends into contact, are very strong, and that by the zinc resembles taste, in the same manner as when applied to the tongue.

4. I had been paring my toe nails with scissars, and had cut off a considerable portion of the thick skin, so that the blood began to ooze through, in the middle of the wound. I applied the zinc there, and an extensive surface of silver to the tongue. Every time I brought the metals into contact, I felt a very smart irritation by the zinc at the wound.

5. I made a piece of zinc having a sharp point, projecting laterally from its end. I applied this point to a hole in a tooth, which has sometimes ached a little, and applied the silver in an extensive surface to the inside of the cheek. When the metals were brought into contact, I felt a very smart and painful twitch in the tooth, perfectly resembling a twitch of the toothache. I thought this twitch double, and that one of them happened before the metals came into absolute contact. I am now almost convinced that this is the case, for when I make the silver rest on a dry tooth, without touching the tongue or fauces, I have no twitch on bringing the outer ends of the metals together: showing that there is not a proper communication through a dry tooth. If, while the outer ends remain in contact, I touch the silver with the tip of the tongue, still no twitch is felt in the tooth. If I now separate the outer ends of the metals, keeping the tongue applied to the silver, a slight twitch is felt in the moment of separation, when they are again brought into contact.

N.B. This twitch is prevented, by allowing the tongue or lip to touch any part of the zinc.

6. I had a number of pieces of zinc made of the size of a shilling, and made them up into a rouleau, with as many shillings. I find that this alternation, in some circumstances, increases considerably the irritation, and expect, on some such principle, to produce a still greater increase. If the side of the rouleau be applied to the tongue, so that all the pieces are touched by it, the irritation is very strong and disagreeable. This explains what I have often observed, the strong taste of soldered seams of metal. I can now perceive seams in brass and copper vessels by the tongue, which the eye cannot discover, and can distinguish the base mixtures which abound in gold and silver trinkets.

If any of the above facts can add to the stock of knowledge you have acquired on this subject, it will give me great satisfaction, and I shall not fail to communicate any thing which may afterwards occur. My indisposition hinders me from taking an active part in the researches, to which this wonderful and important discovery incites; but it is both my duty and my earnest wish, to contribute my feeble assistance to every gentleman engaged in this interesting pursuit.

I find that common silver thread makes a very good conductor, and this to any distance.

Since writing the above, I have found a very easy way of producing very sensible convulsions (I think muscular), and corroborating my opinion, that the communication (of this part of the whole effect) takes place before contact.

Put a plate of zinc into one cheek, and a plate of silver (a crown piece), into the other, at a little distance from each. Apply the cheeks to them as extensively as possible. Thrust in a rod of zinc between the zinc and the cheek, and a rod of silver between the silver and the other cheek. Bring their outer ends slowly into contact, and a smart convulsive twitch will be felt in the parts of the gums situated between them, accompanied by bright flashes in the eyes. And these will be distinctly perceived before contact, and a second time on separating the ends of the rods, or when they have again attained what may be called the striking distance. If the rods be alternated, no effect whatever is produced.

Care must be taken not to press the pieces hard to the gums; this either hinders us from perceiving the convulsion, or prevents it. I find too, that one rod, whether zinc or silver, is sufficient for the communication; and even bringing the two pieces together will do as well, or perhaps better. But the rods are easier in the management.

Asking pardon for the liberty I have taken, without having the honour of your acquaintance, I am, with great regard, sir,

Your most obedient humble servant,
JOHN ROBISON.

ON THE USE OF ERGOT.

To the Editor of the Medical Gazette.

SIR,

HAVING long been convinced of the value of the ergot of rye as a medicine, I now beg to offer the following remarks to my professional brethren, through the medium of your very valuable journal; premising, that what I am about to advance is not the offspring of any fanciful notions as to its *possible* effects, but a simple statement of the results by which its use has been followed in my practice for several years. This assurance seems to be especially necessary at the present time, when it is but too much the fashion to render facts subservient to hypotheses. Whatever, then, may be advanced in this communication has been actually observed; and I am perfectly satisfied (due discrimination being exercised) will be fully sanctioned by further experience.

To enter upon the consideration of the action of the ergot on the parturient uterus, or the very interesting and important question of its power to excite uterine action, is foreign to my present object; I shall therefore proceed at once to a statement of its efficacy as a medicine, and the diseases in which I deem it worthy of confidence. They are—polypus uteri, attended with profuse hæmorrhage; menorrhagia, where there is no inordinate action of the heart or arteries, or morbid sensibility of the uterine system; in leucorrhœa, when independent of inflammatory action; in chlorosis with amenor-

rhœa; and in dysmenorrhœa; in all of which cases I have had numerous opportunities of ascertaining its efficiency.

The first time I saw it exhibited as a medicine was by my lamented friend, the late Mr. Parr, of this town, so long ago as the year 1828. It was in a case of very large polypus of the uterus, accompanied with frequent and frightful attacks of hæmorrhage. He gave the ergot after all ordinary treatment had proved unavailing. The effect produced was not only the moderation of the hæmorrhage, but also the expulsion of large and numerous masses of the tumor, in many of which a distinct fibrous structure was perceptible. After its continued employment, the woman, who had arrived at the climacteric period, enjoyed comparative health and comfort, being freed from the repeated and alarming hæmorrhages, and experiencing no inconvenience from the small portion of the tumor which, when last examined, still remained. To this case I owe the idea that it might possibly be useful in others. How far this has been justified, the sequel must decide.

In menorrhagia, by which term I do not mean the mere increased menstrual discharge, I have found this medicine of the greatest value. In this disease, however, it is necessary to ponder well on the individual circumstances of each case, and to use the utmost caution in ascertaining the cause on which the disease depends; as where such precaution has been neglected, it has been my lot to see both the sufferings and danger aggravated by the ergot. In this respect it does not differ from other active medicines. The slightest reflection will suffice to call to mind the very different states of the system in which this disease occurs. It may, for example, happen in the most phlogistic and plethoric, or it may arise in a person of a diametrically opposite constitution. If given in the first state it is decidedly prejudicial, unless preceded by such means as are calculated to remove alike the plethora and morbid sensibility; and even when this has been done, its operation requires to be carefully observed. In the last it acts most beneficially, as it at once raises the nervous energy of the uterus, and through this medium pro-

bably imparts increased tone to the relaxed and debilitated vessels from which the exhalation takes place. The following case affords a striking example of the utility of the medicine:—

In 1834 I was requested to visit a poor woman who had for many years led a most abandoned life, and who was apparently in a dying state from extreme exhaustion. Her age might be about thirty-four. On inquiry, I learnt that for the two preceding years she had suffered from repeated and severe hæmorrhages from the uterus, and that, on the present occasion, the discharge had continued for many days, until, when I saw her, though still abundant in quantity, it was not more coloured than the serum of healthy blood. In short, she seemed to be rapidly sinking. I ordered her immediately a full dose of the acetate of lead, with opium, to be repeated every four hours; and port wine or brandy, in sago, to be given frequently. Cold was applied, both externally and internally, by means of an injection of a strong solution of alum. For a brief space of time she appeared somewhat relieved; but in two or three days the hæmorrhage recurred with increased violence, the fluid also being more sanguineous. Having, in other cases, fancied that the ergot had been of use, I immediately gave her ʒss. of the powder, to be repeated in ʒi. doses every three or four hours. From the lapse of an hour after the first dose she experienced manifest relief; and on seeing her in the evening, I directed the third dose not to be given till the sixth hour, unless any recurrence of the discharge should render it necessary. Next day she continued better; and the medicine was directed to be taken only night and morning. From this visit she went on gradually improving, and for two years after, when I had occasion to prescribe for her, she had experienced no return of hæmorrhage. She appeared quite well in health, but completely blanched in colour. To this I might add several other cases, occurring in persons of asthenic constitution, in which similar good effects were derived from the ergot, but consider that it would only be a needless occupation of your columns, and your readers' time. It may be enough to say, that seven years of

subsequent practice have confirmed my reliance on the ergot in such cases. Even where an opposite diathesis prevails it has been employed advantageously, combined with conium and hyoscyamus. In such persons it is advisable to premise a moderate abstraction of blood from the system, and to keep up depletory action by means of saline cathartics. In these cases, however, the infusion of roses with sulphuric acid and digitalis, or alum, seem more appropriate.

In leucorrhœa it has been highly useful. In this disease, as in menorrhagia, it is necessary to ascertain the state of the system, more particularly the uterine disorder on which it depends; as where any degree of inflammation is present it will be injurious. In several distressing cases of this disease, where the strongest astringent injections had been employed without any effect, except exciting inflammatory action which did not previously exist, I have found the ergot, aided by injections of simple warm water, or the decoction of poppy capsules, perfectly successful. I may observe *en passant* that from considerable opportunities of observation, it is my firm belief that in a vast proportion of cases of leucorrhœa stimulant and astringent injections are not only uncalled for, but absolutely contraindicated; at the same time, it must be acknowledged that they are occasionally of great use.

In chlorosis and amenorrhœa I have frequently experienced the good effects of the ergot, after aloes, iron, valerian, cantharides, &c. had all been employed without the slightest advantage. In these cases, when extreme nervous susceptibility exists, it may be most advantageously combined with the valerian, and where the alvine system is torpid, with aloes. When aloes are employed, I attach the preference to the Barbadoes: in doing so I may be somewhat empirical, as it is difficult to account for the superiority of the Barbadoes over the socotrine, as an emenagogue, unless it be ascribed to the larger quantity of bitter principle contained in the former.

In dysmenorrhœa the ergot has been most useful. In one case, accompanied with what may without exaggeration be termed torture at each period, it appeared almost magical in its opera-

tion. It is right to state that it was given in combination with the valerian; which medicine, however, had been previously given, but without any apparent benefit. To enter into the detail of individual cases is not only tedious, but would also occupy too much of your valuable columns. I shall therefore endeavour, with the utmost possible brevity, to explain the apparent paradox of a medicine being of use in cases at first sight so pathologically opposite.

It is well known that the same cause operating on different constitutions will be followed by effects proportionately variable. This is a fact which does not admit of refutation, whether it be applied to man morally or physically. So then it is with regard to the uterine system, as every man of moderate experience will admit that the same morbid condition will, according to peculiarity of constitution, produce a very dissimilar train of symptoms. Thus, whilst a congested state of the uterine vessels will in one person lead to menorrhagia, the same cause operating on a different constitution will be attended with leucorrhœa. Again, when the nervous system of the uterus is at fault, we in one person have chorea, in another simple hysteria, in others cardiac and pulmonic symptoms, &c. &c. This is especially true in regard to menorrhagia and leucorrhœa. These diseases may arise alike in the sthenic and asthenic diatheses, which must of course render a corresponding modification of our remedial measures necessary. Without using much discrimination, we aggravate rather than alleviate the suffering inseparable from disease, and enrol ourselves under the banner of empiricism.

I have merely to add, that I consider myself fully justified in recommending the ergot as an excellent emenagogue and anti-hemorrhagic agent. Should this recommendation be followed by beneficial results in the hands of other professional men, I will regard myself as amply rewarded for the trouble of putting together the foregoing remarks, the correctness of which can only be proved by trials more extensive than private practice; besides which, every man is liable to view with *partiality* any plan of treatment, which, without authority, he adopts and finds successful.

I have now to apologize for the length to which this communication has extended, and subscribe myself, sir,

Your obedient servant,

GEORGE FIFE, M.D.

Newcastle upon-Tyne,
May 27, 1841.

P.S. The doses in which I have prescribed it have varied from gr. x. to ʒj. of the powder, 3ss. to 3j. of the concentrated tincture. A good formula for decoction is given in Foot's Medical Almanack, although it appears to me that the quantity of ergot is rather small.

My friend Mr. Bennet, of Gateshead, informs me that he has given the ergot in a case of hæmoptysis with very satisfactory results.—G. F.

ILLUSTRATIONS

OF THE

PATHOLOGY AND TREATMENT OF THE AMAUROSES.

BY EDWARD HOCKEN, M.D.

(For the Medical Gazette.)

(Continued from p. 872, vol. i. 1840—41.)

Amaurosis from Affections of the Spinal Cord or its Membranes—Amaurosis from sudden Sinking of the Vital Powers.

AMAUROSIS is an occasional symptom in functional or organic disease of the spinal cord, but by no means a constant or invariable occurrence from any one disease, or from any particular situation it may hold. Although such is true, on a general inspection of our knowledge of affections of the spinal cord and its membranes, since disease may arise, and make progress, and terminate in any one situation again and again, and yet never occasion complete or incomplete amaurosis, yet if the recorded cases in which amaurosis has been symptomatic be examined, it will be found that all situations are not indifferently, but that disease of the cord or membranes situated within the cervical vertebræ was necessary to its production, although the same apparent derangement may have held the same situation, and presented nearly similar phænomena, a hundred times, without originating amaurosis; the cervical disease being free from com-

plication, or in connexion with a similar disease in other portions of the spine.

In objection to this statement of disease of the cervical portion of the cord being necessary, there are some apparent exceptions. I will presently mention a case where the dorsal portion was softened, whilst the cervical was healthy; yet I believe that, in such cases, traces of disease may be found in the cervical membranes, or that the cervical portion is implicated in some other way. I do not believe that lumbar disease ever does or can produce such symptoms.

Diseases of the spine, producing amaurosis, may or may not occur conjointly with disease of the brain or its membranes: it may be strictly *spinal*, and quite unconnected with cerebral disturbance. The results of pathological anatomy can, however, never have more than a limited application to the physiology of the brain. We are unacquainted with the laws according to which the different parts of the organ participate in the affections of each other; and we can only, in a general way, regard as certain, that organic diseases in one part of the brain may induce changes in the functions of other parts; but from these facts, and the results of pathological anatomy, we cannot always draw certain conclusions. Degenerations in the most various parts of the brain, which appear from experiment to have no immediate connection with the central organs of the sense of vision, nevertheless frequently cause blindness; and at this we must be the less surprised, since even in the diseases of the spinal cord, as *tabes dorsalis*, imperfect amaurosis is a frequent symptom*.

Experience has without doubt proved the occasional dependence of imperfection or loss of vision solely on spinal disease; but the explanation of such cases is by no means easy. How disease of a part apparently unconnected in function with vision, such as the spinal cord, can produce abolition of sensation in the visual nervous apparatus, partial or complete (amaurosis), is indeed curious: but what is the *modus operandi* of such exciting, or perhaps rather efficient, causes? Can the anatomical relations

of the corpora pyramidalia, corpora olivaria, and corpora restiformia, sequentially with the anterior, middle, and posterior cerebral lobes, and those of the cerebellum, explain the propagation of irritation to the origins of the optic nerves, and thus account for what may be termed *spinal amaurosis*? Be this as it may, let the following abbreviated case speak for itself:—

Induration of the spinal cord, with loss of sight and hearing.

These remarkable phenomena* occurred in the Marquis de Cousan, commencing first with pricking in the fingers and toes of the right side, which gradually extended along the arm and leg: the parts wasted, became cold, and lost their feeling; but some degree of motion was retained. After a twelvemonth the left side became similarly affected, and he then lost all power of motion in the trunk or extremities; the other functions continuing for some time in a healthy state. His sight and hearing were next deranged, being first weakened, and then gradually destroyed. He subsequently lost his speech, and the power of swallowing, in the same gradual manner; dying soon afterwards.

On a post-mortem examination the brain and all the viscera were found quite healthy. That part of the spinal cord which is included in the cervical vertebræ was so hard as to have the consistence of cartilage, and the membranes of this portion were red, as if inflamed.

REMARKS.—A more convincing proof of the occasional dependence of amaurosis on uncomplicated spinal disease, could not well be found: here its origin and progress were gradual, and its termination complete, and this in connexion with loss of the sense of hearing. We may safely conclude that a knowledge of physiology, based on experiments performed on healthy animals, by no means always explains the phenomena observable in disease; and hence that the true vital relations and sympathetic connexions of organs, or particular parts of those organs, are learnt rather from observation and reasoning, in disease, than from the apparent results of experimental reasoning during health, however

* Vide Translation of Müller's Physiology, vol. i. p. 839.

* Vide Portal, Cours d'Anatomie Médicale, tom. iv.

much we may learn from such of the immediate functions normally performed. Who, for instance, would have concluded that the senses of sight and smell held the remotest connection with the cervical spine? But pathological observation has certified what experimental physiology never could have accomplished. I myself met with a case which might perhaps be referred to the head of "spinal amaurosis." A child became affected with many symptoms resembling ordinary infantile remittent, and some tendency to spasmodic actions; such as flexing of the thumbs and great toes inwards on the palms and soles. After some days the head was drawn to the left side, and fixed in that situation, the child complaining of great pain and tenderness in the region of the cervical vertebræ. Shortly afterwards the mother asserted that the child was blind, that it never took the least notice of passing objects, but was in other respects quite sensible. The look was staring and vacant, the pupils of both eyes dilated, and but very little sensible to alternations of light and shade. This condition remained about a week, when all the febrile symptoms, the pain in the neck, and the blindness, gradually passed off, mainly by attention to the condition of the alimentary canal.

But very severe disease may exist in the cervical spine, and yet any affection of the sight or hearing be absent. I saw a patient in the Devon and Exeter Hospital, who had actually spit up through his pharynx several large carious portions of the bodies of those bones; and yet no such symptom here existed. I believe the portions of vertebræ spit up were preserved by Mr. James, of that city.

Amaurosis from hæmorrhage and rupture of the cephalic bulb of the spinal marrow, and of the annular protuberance, &c. &c.

M. D.,* a middle-sized man, with a large head, short neck, broad shoulders, and a large abdomen, very muscular, being at work in the open air, complained suddenly of ringing in the ears; some minutes after which he screamed from acute pain. He arose, commenced running, and after a short

distance fell completely insensible. His face was pale; his pupils immoveable, much dilated, and of the same size on either side; eye-lids at first half closed; immobility of the globe of the eye; mouth half open; tongue covered with arterial blood, and occasionally protruded, but without permanent deviation of its point; lips covered with frothy saliva; no perceptible tension of the mouth. The respiratory movements were irregular, with occasional stertor; the limbs in a state of rigidity, readily overcome, and not permanent: convulsive movements occurred when the skin was pinched, or cut, in bleeding. He died five hours after the first attack. On examining the body the central protuberance was found changed into a pouch filled with blood, partly coagulated, and mixed with some fragments of nervous substance, softened and coloured by this liquid. This effusion made its way laterally by a small opening, but the principal rupture existed in the fourth ventricle, the floor of which, divided transversely, had given issue to the blood which distended the parietes of the ventricle.

The brain and the cord may be simultaneously affected, and amaurosis may be symptomatic of such condition, as in the following case from Abercrombie*. A strong healthy child, aged nearly two years, after being oppressed and feverish for two days, was seized with violent convulsions. The first fit continued about an hour, and left her comatose, with distortion of the eyes. She was subsequently reattacked with convulsions, which were now attended by violent and irregular action of the heart, and a peculiar spasmodic action of the diaphragm, leaving her in a comatose condition, from which she never recovered. She took food or medicine when they were put into her mouth, but shewed no other appearance of sensibility. The eye was completely insensible, and the pulse very frequent. She had afterwards several slight attacks of convulsions, and one more severe a short time before death, which happened thirty-three hours after the first attack. Inspection shewed only in the brain slight increase of vascularity and effusion under the arachnoid; a copious discharge of bloody

* Vide Ollivier's *Traité de la Moelle Epinière*, &c. tom. ii. p. 511.

* Practical Researches on Diseases of the Brain and Spinal Cord, &c. 3d Edit. p. 356.

fluid escaping from the spinal canal when the brain and cerebellum were removed. When the spinal canal was laid open a considerable deposition of a colourless gelatinous fluid was discovered between the canal and the dura mater, most abundant in the cervical and upper dorsal regions: a small quantity of the bloody fluid also remained. The substance of the cord, at the upper part, seemed softer than natural, and very easily torn. All the viscera of the thorax and abdomen were healthy: the foramen ovale, however, remained pervious by a small opening.

Again, I would remark that simple injury from mechanical causes may be attended with dilated and insensible pupils.

A man* fell from a tree, and lighted on his back, and likewise struck the back of his head, in which a wound in the integuments took place. He lay in a condition of syncope for some minutes: on his recovery it was found that the lower extremities were entirely deprived of sense and motion. He had afterwards retention of urine, tumefaction of the abdomen, headache, dilatation of the pupil, extreme anxiety, difficult deglutition, and stertorous breathing: the pulse being as low as 38 in the minute. He recovered gradually, and was well in three weeks. Was this spinal or cerebral? Dr. Monteith, of Glasgow, reported some cases of spinal affections to Dr. Abercrombie of an anomalous nature. Beside a great variety of painful and spasmodic symptoms, there was in general a great aversion to light, and one of his patients lay in a state of almost total darkness for more than a year. In one patient there was incessant vomiting, so that she retained nothing of food, drink, or medicine for six weeks. In this case the vision was also very much impaired, and twice suspended for a considerable time. Nothing was discovered about the spine in any of these cases, and the pain in the spine was not increased by pressure, but it was very much increased by motion, or by attempting a sitting posture†. I should myself regard these rather as examples of local hysteria than as actual cases of diseased spines, especially from their history, treatment,

and relapses. (Vide author's first paper, *Lancet*, vol. ii. p. 7, and "Amaurosis from Hysteria," vol. ii. 1839-40.)

A lady became affected with numbness and partial loss of power in the right arm and leg, and some time after had slight difficulty of articulation; these symptoms being subdued by treatment, returned, affecting both sides, after some months, with the characters of chorea. After another interval of some months, she became liable to attacks of blindness, the upper eyelids falling down so that she could not raise them: when raised by the hand, the eyes were found distorted upwards. These attacks continued for weeks at a time. The case went on for two years in this manner. For four years subsequent to this period she became subject to most peculiar symptoms, apparently referrible to the spine; no doubt hysterical. At the end of this protracted period she suddenly recovered from a severe paroxysm in an instant, with a convulsive start, menstruation having, at this instant, taken place in a more full and healthy manner than it had done for years. She now recovered completely — *Abercrombie*. Dr. Abercrombie also minutely details another case, attended with very peculiar symptoms: "During these attacks she became suddenly silent and motionless; the eyes open, but fixed and insensible, with total unconsciousness of every thing." She never perfectly recovered from the affection. — *Loc. cit.* p. 412. I shall make no apology for introducing the outline of another case from the same author.

Ramollissement of the cord—Amaurosis.

A boy, aged seven, had been indisposed from the 18th to the 20th of May, 1823, but so as to attract little notice: there had been some slight headache and feverishness, but he seemed almost well on the morning of the 22d. He was seized on the same afternoon with general and severe convulsions, becoming partially comatose; the eye fixed and insensible. He became less incoherent in the course of the day, and complained of headache and impatience of light. In the evening there was a slight appearance of squinting; and in the night some convulsions. On the 24th he seemed better. Eye natural; face pale; pulse 120. The convulsions returned on the morning of the 25th;

* *Journal Universel*, tom. xxviii.

† For an account of these cases vide *Abercrombie, loc. cit.* p. 404.

after which he sank into a low oppressed state, and died early in the afternoon.

Inspection.—There was no effusion, and no appearance of disease, in the brain. On removing the brain a considerable quantity of fluid flowed from the spinal canal, and more was found on laying open the canal. The cord was healthy at the cervical portion, but was remarkably softened and broken down in the upper part of the dorsal region. This appearance extended for several inches, but varied in degree. At one place a complete separation took place in attempting to raise the cord, the part falling down into a soft diffuent pulp throughout its whole diameter. From the middle of the dorsal portion it was quite firm and healthy. The inner membrane of the cord was dark coloured, highly vascular, and showed evident marks of inflammation at the part corresponding with the softened portion*.

REMARKS.—The cases I have already detailed are quite sufficient to prove that various affections and diseases of the spine may and do frequently originate amaurotic affections, as prominent symptoms of their existence—a fact which seems to have been overlooked by all previous ophthalmological writers. In the case last alluded to the dorsal portion of the spinal cord, in its upper part, was so considerably softened as to give way completely when an attempt was made to raise it, but the cervical region was quite healthy.

As this is a solitary case, in opposition to the great number I have before alluded to, the general deduction, with which I commenced—namely, that where amaurotic symptoms were induced, the cervical portions of the membranes or cord were always affected, either separately or conjointly, does not appear to me disproved. Although, in the present case, the membranes only displayed evident marks of inflammation at the part corresponding to the softened portion, increased vascularity may have existed above during life; for post-mortem examinations never can unravel vital pathology. But on this supposition I lay little stress, since I attribute the production of the amaurotic phenomena to the accumulation of fluid, and the pressure thus exerted on the cervical spine; for it is to be

remembered that a very considerable quantity of fluid flowed from the spinal canal on the removal of the brain, and more was found on laying open the canal itself, between the cord and the external membrane.

Should these considerations appear insufficient, I own myself no bigot to these opinions, but that where the law is, that if amaurotic symptoms arise with those of spinal disease, the cervical portion is implicated, or solely affected, the exception may be that the upper dorsal region is the originating part, and that the cervical is little if at all concerned. The deductions at which I have myself arrived, from an attentive consideration of cerebral and spinal diseases, are, that the symptoms are dependent on their situation and extent, rather than on their nature, since the most opposite conditions, as regards their pathology, and every intermediate stage, may produce the same phenomena, provided that they give rise to the same extent, degree, and situation of mechanical effects; or to vital relations which originate the same impairment of function. Hence the local symptom of hyperæmia or anæmia are identical; and no difference is apparent from the pressure and deranged circulation induced by idiopathic apoplexy, a depressed skull, or the introduction of some foreign body, or whether the local accumulation be pus, or blood, or serum, provided that the rapidity of accumulation be similar, and the situation, extent, &c. be identical, as before mentioned.

Amaurosis from sudden sinking of the vital powers.—I will allude to two cases of this description, to conclude this paper. A man in the Devon and Exeter Hospital, a patient of Mr. Barnes, had his leg amputated,* his subsequent treatment devolving on me whilst a pupil at that institution. The case proceeded unfavourably; the vital powers sunk rapidly, and about a week after the performance of the operation he died. On the morning preceding his death he was calm and collected, and assured me he was perfectly blind in his right eye: the globe was sunken and flaccid. On the same afternoon the

* He was an old man, and the amputation was undertaken under unfavourable circumstances. I have alluded to it only as an example of the form of amaurosis.

whole anterior part of the eyeball appeared brown, dry, and shrivelled. He died in the evening. A lady was suddenly seized, within half an hour, with a depression of the general nervous system, to so low an ebb, that immediate dissolution was anticipated.

Although death was expected every minute, so great was the collapse, yet by the constant use of the most powerful stimuli, the action of the heart was still kept up, and the patient kept alive: with this condition of the nervous system the sight was completely lost; the eyes were completely insensible to the impression of light. These symptoms, with but little change, continued during a space of five weeks, when, for the first time, some slight symptoms of reaction occurred; within the short space of half an hour bringing hopes of recovery from a condition differing little from death. The heart continued to act of its own accord, without stimuli, for half an hour; and from that time she slowly but eventually recovered. On the induction of complete reaction, the true but previously unintelligible cause of all this alarming depression succeeded: the rash of scarlatina developed itself; and soon the disease was fully marked. She passed safely through the exanthematous fever, and eventually regained perfect health.*

Such cases as these, are, I have no doubt, known to many; and I may remark that the progress influences the result; the sinking, if slow and gradual, not giving rise to blindness, whereas amaurosis may occur if the vital powers be rapidly depressed from any cause. The second case illustrates the influence of a powerful impression on an individual, where the powers of the general nervous system were feeble, and is in accordance with a general law in the system, namely, that if any distressing influence be exerted on a part, or on the system generally, if the general tone allow, not only is it expelled, but increased action results. If, on the contrary, the local or the general tone be deficient, the vital depression is proportionate to the condition and the intensity of the cause, immediate death being the ultimatum; then death at a more remote period; and, as we gradually ascend the

scale, a more and more speedy reaction, till the reaction almost immediately follows the depression; the force of the reaction being generally proportionate to the previous depression experienced.

I trust, in my next paper, to dwell on the forms under which hyperæmia of the visual nervous textures conjointly produce amaurosis.

MEDICAL GAZETTE.

Friday, June 18, 1841.

“*Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.*”

CICERO.

APOTHECARIES *v.* CHEMISTS.

THE cause between the Society of Apothecaries and Mr. Greenough, reported in our number for June 4th, illustrates admirably well the anomalous position of medical practitioners in the present day, and might afford many good hints on the subject of medical reform.

It is, in the first place, evident that the defendant, Greenough, had every anxiety to avoid a collision with the Apothecaries' Company, for it will be seen that in all his bills he called himself *Surgeon*; knowing well enough that the only corporation in England possessed of power to make surgeons has no power to prevent any individual who chooses from assuming that title. And, indeed, it must have been a source of no less surprise than satisfaction, when this defendant, assailed by the Society of Apothecaries, whom he had so cleverly striven to avoid, found that there was yet a loophole furnished—unadvisedly (to use the mildest term)—by the Judge, through which he might escape, by stating or pretending that, while exercising all the positive functions of a physician and of an apothecary, and while calling

* From the Author's manuscript notes of Dr. Latham's lectures. Vide also Essay on Influence of Constitution, &c. p. 38.

himself a surgeon, he was, in fact, only doing the same as any chemist and druggist did, or might have done, before the passing of the Apothecaries' Bill in 1815.

Whoever will be at the pains to read the case will find, that the escape of the defendant was entirely due to the difficulty of determining what was the difference between an apothecary and a chemist before 1815. Mr. Watson, his counsel, assumed and stated that the difference was *substantially* nothing, and *nominally* no greater than the members of either craft thought fit to make it for themselves. For example, if a man chose to call himself apothecary, he became so, with the advantage of being thought, perhaps, a clever well-educated man; whereas, if he called himself a chemist—a chemist he was, with the disadvantage of being deemed, perhaps, an unlettered man; but, he might have added, the benefit of being regarded a cheap one. And thus, after 1815, they might both continue to perform the self-same functions, the one by right of the Society's license, the other by right of his own; the one necessarily well educated, the other as ignorant as he pleased; the one in a measure bound to confine himself within his own province, the other fettered by nothing but his own inclination.

Such a doctrine, being difficult of subversion by definitive facts regarding the actual and legal distinctions between chemists and apothecaries before 1815, may hold good in a court of common law (though of this we think there is very great doubt); but it certainly can never hold in a court of either equity or common sense. The Apothecaries' Act was passed because apothecaries, so called, and so calling themselves, practiced as Mr. Greenough has done, performing all the essential functions of physicians without any proof of their competency; and its object was

to prevent their doing so in future, and to give them, when an examination proved them competent, a license to discharge the same functions as before they had performed unlicensed. And the simple fact of the enactment of such a measure affords ample proof that all who did thus act, visiting patients, writing prescriptions, or dispensing medicines on their own authority, were regarded as apothecaries, and, as carrying on a practice which, without the test of an examination, they could not all be safely trusted with. The preface of the Act, therefore, exactly defines the class of persons upon whom it was intended to operate; they were those who, being unlicensed, practised medicine under any name but that of physicians. If any one, previous to 1815, calling himself a chemist, did thus practise, he became, by that very deed, an apothecary; and it was to guard the public in subsequent years against such, that the Act was framed.

What chemists really were before 1815 is easily told: some of them were a small unimportant class, who sold spices, certain articles of grocery, and some drugs; who, somewhat elevated above the ordinary grocers, trod on the heels of the apothecaries in the *counter-line*, but never ventured to do more in prescribing or recommending medicines than one neighbour might to another; the rest, and the most important class, were the wholesale chemists, who occupied the same position then as now, and who, as merchants, supplied the chief consumers of drugs, the apothecaries. That the first class interfered in no way with the management of the public health is proved as plainly as if it had been openly declared, by the fact that the Apothecaries' Act, which was intended to guard the public against unlicensed practitioners, left the customs and rights of chemists and druggists, who

at that time were not practitioners, undisturbed. For what effect could the act possibly have had, if, in 1816, a person wishing to discharge all the offices of a licensed apothecary, but not willing, or not able, to obtain the license, had only needed to write *chemist*, instead of apothecary, over his door?

Such—however the present action may terminate—is a common-sense view of the case: the fact of the Apothecaries' Act having been passed, and its evident object, are the best proofs that, previous to 1815, the practices of chemists and of apothecaries were not the same; and therefore that no man has any right to evade the provisions of the Act by pleading that he does but follow the same business as a chemist did before it was passed.

If there be any chance that such a plea will hereafter be available, it will at once virtually license the hundreds,—we might, perhaps, say thousands—of chemists who are now practising under no title, or under that of surgeons. Should this action terminate in favour of the defendant, the power of the Apothecaries' Company to protect its licentiates is gone, and with it their whole occupation of granting licenses. Gone, at least, they will be, unless chemists and druggists be placed under regulations similar to those of the Apothecaries' Act. Whatever may have been the defects in the details of that measure, or the errors in working it, it certainly has, upon the whole, done much good. It was a great public evil that any one should pretend to take charge of other men's health without a guarantee of his competency. But it will be a still greater evil now if the same lawlessness prevail; if, when competition is much closer, and money much scarcer, and cheapness often much more prized than safety, all men are alike ad-

mitted to make their market in their neighbours' sickness.

We believe that none ever doubted the propriety of passing, in 1815, some measure with the objects of this Act; and if the same, or a still greater necessity, exist now, such a measure will surely not be withheld. If chemists are determined to be apothecaries, let them be so; and we heartily wish them prosperity, if they will only be content to be so by right and license, and with an assurance to the public that they are competent to the discharge of all an apothecary's duties. Only let them not sneak into privileges which other men gain openly by hard labour; but, for their own sakes, let it be granted that any one of them, desiring to rise honestly from his shop to a respectable medical practice, may do so by obtaining the apothecary's or an equivalent license in the ordinary way—save by the serving of an apprenticeship.

It appears to us that in all schemes of medical change, the disposition and the capability of the public to pay their medical attendants is far too much disregarded; whatever enactments are made, whatever corporations are established, money will in the main guide all. If the whole body of practitioners are to be possessed of the highest amount of medical knowledge that can be obtained by a prolonged and expensive course of study, they must be paid accordingly; they must have much handsomer means than the public can or will afford to pay to a great number, and their numbers must, therefore, decrease in even a greater proportion than there is at present any prospect that they will. And if they did thus decrease, which is most improbable, even then the public would require at least one other class. There are few, except among the more opulent, who, when themselves or their

families are attacked with sickness, do not consider at least two distinct things; the chance of recovery, and, on that chance, the amount which it is worth while to pay a medical attendant to secure or accelerate that recovery. This last is in ninety-nine of every hundred cases, where there is a difference of rates of charges among practitioners, the thing chiefly looked at; and, in the end, the great majority of men, unable or unwilling to decide on differences of medical merit, but keenly alive to differences of expense, believe or hope that the former are not, after all, so great as to compensate for the evidently considerable amount of the latter. This is the case very generally even in cases of illness of considerable severity, and it is almost universal in those of less importance; any advice, it is thought, will do for them, and the cheapest practitioner is resorted to without question.

We repeat, then, the public choose to have a class of cheap practitioners, and, if they will, they always can have them. Laws, however stringently made, will be evaded; and the only chance for those who would protect the public and the profession must be to insist on those who will practise cheaply being compelled to make themselves competent to do so safely. Only let the profession be sure of this—that if they will make themselves all of one title and rank, and will maintain that unity by the most decisive measure of making their rates of remuneration all equal, they may certainly anticipate that the public will exalt a class inferior to them into the position which is at present occupied by the majority among them. The just boast of the general practitioner, that it is he who is most essential to the public service, who attends and has the confidence of the majority of the public, should make him very cautious

how he leaves the post he occupies. His professional talents are equalled by others, who, demanding higher fees, have less numerous clients: his position only is peculiar to himself, and he will make an unwise mistake if, forgetting this, he abandons that position, and, with it, his profits, to the unlicensed chemist, whose anxiety to hold them is abundantly proved, and whose capacity to satisfy the public ignorance admits of little doubt.

ROYAL MEDICAL & CHIRURGICAL SOCIETY.

Tuesday, June 8, 1841.

THE PRESIDENT IN THE CHAIR.

On the Action of Poisons. By JAS. BLAKE, Esq.

THIS paper contains a confirmation of the author's researches on the action of poisons, the greater part of which have already been before the public. After some remarks on the manner in which such researches should be conducted, the author observes that the present question contains two elements distinct in kind: viz. a dynamical and a chemical problem. The former referring to the place where the poison acts; the latter to the molecular changes which the presence of the poison in the blood gives rise to in this fluid, or the tissues with which it is brought into contact.

The former question came before him at present. It had formerly been shewn that sufficient time always elapses between the application of a poison, or its injection, and the first symptoms of its action, to allow of its being carried to the brain, and also that contact of the poison with a large surface is not sufficient to give rise to any general symptoms, as long as its diffusion through the body is presented. The author now proves, that in every instance the rapidity of action of a poison is in proportion to the rapidity of the circulation. With a view to this proof, experiments have been performed on different species of animals, in which the time required for the blood to circulate from one part to the other differed greatly; for should a poison which acts on the nervous system only produce symptoms when applied to the nervous centres, it must require a longer or shorter time before it shews evidence of its action, according as the circulation is more or less rapid, or the course which the poison has to run be longer or shorter.

Experiments were performed by the author on horses, dogs, fowls, and rabbits; these animals offering extreme differences as regards rapidity of the circulation of the blood.

It is shown, by experiments, that a substance injected into the jugular vein of a horse, arrives at the capillary termination of the coronary arteries in ten seconds; of a dog, in twelve seconds; of a fowl, in six seconds; of a rabbit, in four seconds. Having made this statement, the author points out, from experiments, that in these animals the time required for the first symptoms of the poison to present themselves bears a close relation to the rapidity of the circulation.

In these experiments he proves not only that the time required for a poison to act is in proportion to such rapidity of the circulation, but also, that in every instance sufficient time elapses, between the application of a poison and the appearance of the first symptoms of its action, to allow of its reaching the nervous centres.

The author concludes by noticing some facts which tend to establish a connection between the chemical composition of substances and their physiological action. The chemical action on the blood of such salts as destroy the irritability of the heart; the analogous effects of salts of the same base; the analogous action of isomorphous substances on the animal tissues; and the difference between the effects produced by poisons according as they are derived from organic or inorganic substances.

Mr. Ancell made some observations on the high importance of the investigations related in the paper, and on the evidence that a humoral pathology was again gaining ground, and that, in many circumstances, the results of recent discoveries had tended to confirm the ideas of the fathers of medicine.

Dr. Addison recalled to the attention of the Society, that it was but a few years since it was regarded as almost established, that there were two different modes in which poisons acted on the system. Some, it was believed, were absorbed into the blood, and being carried with it, acted directly on the tissues or organs chiefly subject to their influence; while others, it was thought equally certain, acted through sympathy; affected first the nerves to which they were immediately applied, and then, through their medium, the nervous centres. That which was considered the chief evidence for this last opinion was, however, now removed. It was said, in favour of this view, that the mode of operation of these poisons, hydrocyanic acid, the essential oils of tobacco and almonds, and others, was so rapid, so instantaneous, that it was impossible for them to have been con-

veyed in so short a time, from the part at which they were applied, through the circulation, to the organ on which they acted. But the experiments of foreign physiologists, and of Mr. Blake, had proved that the course of the blood was much more rapid than had been hitherto supposed; that a few seconds were sufficient for the whole circle to be passed through; and that, on the other hand, the action of poisons was not too rapid for it to be imagined that they might have been absorbed, and carried by the blood to the part immediately affected. These facts, then, had removed the main difficulty which formerly existed in the way of making the theory of absorption universal. But he thought still that caution would be necessary, lest the idea of the poison being always carried by the blood in its circulation should be adopted too widely. We could not, for example, be certain that when a poison was introduced into one blood-vessel, and speedily after found in another, it had been carried there by the moving of the blood, and that the same blood, or a part of it, which was, at the time of introducing the poison, in one vessel, was, in the next period, in another. It might be that the poison passed by diffusion, by the mere act of mixing with the blood; and, in that case, the velocity of the circulation would have little to do with the velocity of the poisoning. He thought, further, that there must be some fallacy in the experiments just related by Mr. Blake, from this circumstance; that if the poisons acted on an animal with a rapidity directly proportioned to the velocity of its circulation, all poisons should affect the same animal in the same time. But this was far from being the case; some poisons acted very rapidly, others very slowly, on the same animal; and this, although they must be conveyed, according to the author's opinion, with the same rapidity to the organ on which they act. And if, in answer to this, it were said that though so carried with the same rapidity, all poisons do not affect the tissue to which they are applied with equal readiness, then all argument must cease; because it would not be possible, in any given case, to say how much of the time was expended in the conveyance of the poison through the circulation, and how much in the action upon the organs to which it at length arrived. Again, it would appear from the paper that the rapidity with which the same poison affected different animals, depended upon, and bore an exact proportion to, the respective rapidities of their circulations; that a poison affected a horse in so many seconds, a dog in fewer, a rabbit in yet fewer, and a fowl in fewer still. But surely this might also result from the different degrees of susceptibility of these animals, which he supposed were in about the

same ratio. These seemed to him some of the circumstances connected with the subject which would render it advisable to pause before completely admitting the theory of absorption and direct action of poisons. He had no doubt that one theory only was correct; that in whatever way one poison acted, all would be found to act in the same; it was absurd to suppose that they had different modes of producing their effect; but he thought the question still open for discussion, and that although the theory of absorption had of late years gained much ground, it was still not yet fit to be entirely admitted. Mr. Blake's experiments were certainly very striking, and though they were opposed to the view which he (Dr. A.), in consequence of experiments performed by himself and Mr. Morgan, had been led to adopt, they had given him very great pleasure. One of the most remarkable among them he had lately repeated, though not under very favourable circumstances, and therefore not with sufficient clearness to enable him to draw a very positive result. The experiment which he meant was that in which Mr. Blake put a ligature around the vena porta of an animal, and then introduced some poison into its stomach. Now it might fairly be said, if the poison does act in the ordinary time, it must be by sympathy, for it cannot have passed into the blood; but if it do not so act, then it will prove that absorption is essential to its action, because the nerves of the stomach are there uninjured, or at least not materially injured, though of course the nerves of a part in which the circulation is much interfered with cannot be regarded as in a perfectly healthy state. Now Mr. Blake had found that in these cases the poison does not act. His (Dr. A.'s) experiments had not been so conclusive. The first, on a rabbit, failed by some of the poison being, by the sudden action of the stomach, forced on the organs around it; from these it might, perhaps, have been absorbed; at any rate the experiment was vitiated, and the influence which the poison did exert could not certainly be ascribed to its action on the stomach. The second experiment was a still more humiliating failure; for the poison acted rapidly, but on examination it was found that the vena porta was not included in the ligature. The third had been more accurately performed, and again the poison acted; but this also was open to a source of fallacy, which he since found Mr. Blake had anticipated, for the œsophagus of the rabbit had not been tied as in Mr. Blake's experiments, and it was therefore possible that some of the poison had passed out of the stomach into the œsophagus, and been absorbed from its surface. From these experiments therefore he could draw no

definite conclusion. Another point which he wished to state was the propriety of endeavouring to render the theory of absorption clear by experiments similar to those which he and Mr. Morgan had performed on two dogs at once, but which, as far as their present evidence went, were altogether opposed to this theory. Some of these experiments were the following:—Two dogs were so connected that the blood of the carotid artery of one might pass into that of the other, *i. e.* the upper part of the carotid of one was made to communicate with the lower part of the carotid of the other, and then some strychnine poison was inserted into the back of this last. Now it seemed certain that if the blood of this dog were poisoned, it should, by passing as it would to the brain of the other, poison *it* also. However, no such effect was produced; the dog that was inoculated was destroyed in the ordinary time, but the other remained altogether unaffected. This experiment was frequently repeated with the same result, and it did seem to him to render it very probable that the blood was not the medium of poisoning. Another appeared still more conclusive. Two dogs were similarly connected by their jugular veins, and then some of the same poison was inserted into the face of one of them, so that all of it that passed into the blood would be carried into the circulation of the other dog; and that the blood from the face of the first dog *was* carried to the jugulars of the other was proved by their becoming empty as often as those of the first were compressed above them, and again becoming full when that pressure was removed. However, in this, as in the previous cases, the first dog only, that is, that which was inoculated with the poison, suffered from it; the other into which its blood passed remained quite unaffected. Now these experiments were so opposed to the theory of absorption, that he thought it could never be received till they had been disproved, or shewn in some way which he (Dr. A.) could not imagine, to be favourable to it. Another subject on which he must remark was the use of the hæmadynamometer, to which Mr. Blake had so frequently resorted: he must confess that he thought the numerous fallacies to which this was subject, from the continued disturbance of the circulation by the fright and excitement of the animal, and from other circumstances, were too great to allow of much confidence being placed in its use; and this had been his opinion from the time when he first read M. Magendie's account of it, and saw the very numerous instances in which its use had been followed by results different from those which had been anticipated. Again, with regard to the conclusion at which Mr. Blake had arrived, that the action of a poison was brought about with the greater

rapidity in direct proportion as it was introduced into the circulation nearer to the brain, this was contrary to what he had observed in his experiments; for they had shewn that poison introduced into the femoral artery produced its effects a few seconds sooner than when introduced into the carotid. On the whole, then, he (Dr. A.) must state his opinion to be, that the question between the two theories of sympathy and of absorption was still open for discussion; that it was not in the least degree probable that both modes of explanation were true—one for one set of cases, and the other for another; and that, on the whole, the researches of Mr. Blake and others had rendered the balance in some measure favourable to the theory of absorption.

Mr. Blake said, in answer to the objections of Dr. Addison, that he had found all poisons act on the same animal with almost exactly the same rapidity, provided they were all administered in sufficiently large doses. He believed that when this was the case, the only source of delay in the action of some among them was due to their being retarded in their passage through the capillaries of the lungs; by their belonging to that class of substances of which, in another paper, he had proved the existence of a considerable number, which, when introduced into the blood, prevent it from passing freely through the capillary vessels. This retardation might cause a delay of a few seconds in the action of these poisons, but except for this he believed all acted with very nearly equal rapidity. With respect to the experiments in the two animals at once, he believed no conclusion at all could be drawn from them, for this reason; that the force of blood in the lower part of the carotid, in the one animal, was not greater, nor, perhaps, so great, as that of the downward current through the upper part of the other; so that, in these experiments, the heart of the inoculated dog did not send any blood to the brain of the other, but might even receive healthy blood from it through its carotids bringing back blood sent to the brain through the vertebrae: and this would be the more evident as soon as the poison began to act on the inoculated dog; for then, the action of its heart becoming weaker, it would be quite unable to drive the blood with sufficient force *up* its carotids, to resist the current coming *down* those of the other. As to the use of the hæmadynamometer, he could only say that for all the purposes for which he had employed it, it was barely possible for it to lead into any error; and for the question whether a poison introduced near the brain produced its effects more rapidly than one introduced at a distance from it, his results had been derived from a

series of several experiments in which he had no reason to believe there was any fallacy.

A few more observations, of less interest than the above, having been offered by Dr. Addison, and answered by Mr. Blake, the Society adjourned.

REPLY TO DR. BURNE.

To the Editor of the Medical Gazette.

SIR,

As nothing can be more revolting to any mind associated with a particle of honest or honourable feeling, than an accusation of snatching surreptitiously hard-earned laurels from the brows of others, I feel myself called upon, unwillingly, to notice a charge of plagiarism by implication, preferred against me in the last number of your journal, in a note appended to a paper entitled, "An Account of a New Plesser, &c. by John Burne, M.D."

Did not the comparison of dates recorded on the one hand, and assumed on the other, with the very unnecessary importance attached to them by Dr. Burne, convince me that he has done me the honour of perusing the paper to which he has referred, I should scarcely have imagined that he had read more than the title; for had he done so he would have seen that I no more denied the priority of using a flexible acoustic tube, for the purpose of auscultation, than I did the invention of the various forms of wooden stethoscope, known long before my name was connected with the profession to which I belong. In my remarks it will be seen that I merely offered an explanation of the *modus agendi* of the solid, perforated, and flexible stethoscopes, in accordance with the laws of acoustics; and, in conclusion, stated that I first used the latter form of instrument at Guy's, in June, 1840; and I distinctly stated that Dr. Clendinning had "for some time past employed the common snake hearing trumpet" for the purpose of auscultation; and up to this moment I am not aware that that accomplished physician has ever published an account of it. For aught I know to the contrary, many other physicians may have used a similar instrument; and the reason why I did not mention Dr. Burne's name in my paper arose simply from my utter ignorance of his ever having dreamt of using a flexible tube; for I certainly never had the pleasure of seeing the Doctor's name associated with auscultation until the last week.

I will take this opportunity of remarking, that bent or flexible stethoscopes have been long used; those which I have seen are, 1st, one used by Dr. Babington, curved in the figure of a segment of a circle, of large

radius, and extremely convenient for auscultating the posterior surface of the chest, in patients who are too weak to be raised in bed with safety; 2d, Dr. Granville's, shewn at the Newcastle meeting of the British Association. In this instrument the ear-piece is capable of being inclined at any angle, from 0° to 45° , by means of a perforated ball and socket joint; and 3d, one which Dr. Chowne lately put into my hands at Charing Cross Hospital, and which he has long used there, is made of wood, and bends at the middle at any angle, by means, I think, of what is termed a cradle joint. I have employed all these instruments, and they all answer the purpose of conducting sound exceedingly well.

With regard to Dr. Burne's observations on the principle of auscultation, I would ask any candid reader whether they can be regarded as any thing else than a meagre repetition of my own, published in the paper at the appearance of which he felt so much "surprised." There are some points, however, which I certainly did not refer to: thus my own powers of observation led me to recommend a short tube, because sound becomes checked by lengthening the conductor; they certainly did not induce me to notice that a tube eighteen inches in length would wear out at one end, and then, if an inch or two were cut off, that it would become shorter. I certainly did not allude to the advantages of the flexible tube to ladies in enabling them to avoid the "proximity" they dreaded, especially, I presume, after being assaulted by the Doctor's plesser; and I was equally remiss in endeavouring to make auscultation comfortable to "corpulent" practitioners.

In conclusion, sir, I beg to assure Dr. Burne that I am ready to give him credit for having used the flexible tube for any length of time he may think proper to claim, and telling him that I am not anxious to be regarded as one of those

"Whose eye turns green at merit not mine own." I sincerely wish him all the honour that can accrue from the *inuendo* contained in the note affixed to his paper, and remain, sir,

Your obedient servant,

GOLDING BIRD, A.M. M.D.
M.R.C.S.

22, Wilmington Square,
June 15th, 1841.

SESQUI-IODIDE OF IRON.

M. OBERDOERFFER, of Hamburg, has proposed to substitute the sesqui-iodide of iron for the iodide, as being less subject to decomposition than the latter. It is made by forming, in the first place, an iodide in the ordinary way, by the union of sixteen

parts of iodine and six of iron in thirty-two of water. This is filtered, diluted with 128 parts of water, 8 parts of iodine added, and the whole quantity of water made up to 320 parts. If the above parts are made grammes, eight grammes of the solution are equal to forty-five centigrammes of iodine. It produces the same effects as the iodide, but ought to be given in smaller doses, being more active.—*Journ. de Pharm.* Sept. 1840; and *American Journal*.

NEW TREATMENT OF HYDROCELE.

By M. JOBERT.

THIS mode of treatment, which is founded on the same principles as that proposed by M. Velpeau for the cure of inguinal hernia, has already been put in practice by M. Jobert in several cases, and with every appearance of success. The following are the steps of the operation, as described in the report of the first case in which M. Jobert had followed the practice.

A small and very narrow bistoury was introduced at the middle and anterior part of the tumor, its cutting edge being directed inwards, and its back outwards. When the tunica vaginalis was pierced M. Jobert depressed the handle of the bistoury, and carried it on in a direction parallel with the cord. Having reached with its point the summit of the tumor, he turned the cutting edge forwards as if to incise the integuments. This done, he withdrew the bistoury, dividing with its point the tunica vaginalis from the upper end of the sac to the point where the skin had been punctured. The bistoury was again immediately introduced by the same puncture, and the inferior part of the tunica vaginalis incised in the same manner. The fluid was then evacuated by the small puncture and compresses soaked in a solution of muriate of ammonia were applied. The patient suffered little during the operation, and nothing afterwards.

The day after the operation a small longitudinal depression was felt through the scrotum, corresponding with the point where the tunica vaginalis had been divided.

The operation was performed on the 22d of June 1840, and the patient left the hospital about the middle of July, to all appearance cured.

In a case on which M. Jobert has since operated, in addition to the longitudinal incision he made likewise a transverse one, with the view of giving greater certainty of success.—*Edinburgh Monthly Journal of Medical Science*.

NOTE FROM MR. BOWMAN.

To the Editor of the Medical Gazette.

SIR,

MAY I beg the favour of your correcting an inaccuracy which I have but this moment observed in the report of the discussion on Dr. Addison's paper at the *Med. Chir. Society*, on April 29th, contained in the *MEDICAL GAZETTE* of May 7th, p. 286. I did not state on that occasion that the epithelium lining the *air-cells* was ciliated; but that I had traced cilia as far as the minute tubules opening into the air-cells. The epithelium in the cells themselves I am inclined to believe to be a simple pavement of scales, though further examination is wanted on this point. I also stated my belief that the capillary vascular network is situated on the *parenchymal surface* of the homogeneous basement membrane of the air-cells, and not in the substance of that membrane.

I am, sir,
Your obedient servant,
W. BOWMAN,
Dem. Anat. King's Col. London.

Paris, June 2d, 1841.

DISLOCATION OF THE THUMB.

At the last meeting of the Hunterian Society on the 9th of June, Mr. Adams directed the attention of the members to a novel proceeding which he had successfully employed at the London Hospital in the reduction of a dislocation backwards of the first phalanx of the thumb. In this case much extension in the ordinary manner had been employed, but without relief. The method consisted in drawing backwards, or extending, as far as possible, the thumb, so as to incline the back of the first and second phalanx on the back of the metacarpal bone: by this the proximal end of the first bone was more closely approximated to the distal end of the metacarpal bone. The thumb was then gradually brought forwards over the end of the metacarpal bone, at the same time that the end of the first phalanx was firmly held in its position. By this means the thumb itself becomes converted into a considerable lever, the fulcrum of which is the proximal end of the first phalanx: the reduction was accomplished with great ease.

UNIVERSITY OF GLASGOW.

DR. J. H. BALFOUR has been appointed Regius Professor of Botamy in the University of Glasgow; vice, Sir W. Hooker, who has been appointed to take charge of the Kew Botanic Garden.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday June 3, 1841.

Henry Marsh, Aldborough, Yorkshire.—George Bell Irving, Sussex.—Richard Payne Cotton, Kensington Square.—Henry James, 4, City Road.—John Thomas Mould, Alford, Lincolnshire.—William Potter.—Nathan Burlinson, Sunderland, Durham.—Joseph Langridge Lowdell.—Henry Cape.—Matthew George Painter, Cornwall.—William MacIise.—William Wall Pearce, Measham.—John Little.—James Rogers, Swansea.—Richard Robert Bowles Norman, Yarmouth, Norfolk.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 5th June, 1841.

Measles	8
Small Pox	22
Scarlatina	14
Hooping Cough	35
Croup	6
Thrush	2
Diarrhoea	3
Dysentery	0
Cholera	1
Influenza	0
Typhus	20
Erysipelas	4
Syphilis	2
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	143
Diseases of the Lungs, and other Organs of Respiration	243
Diseases of the Heart and Blood-vessels	20
Diseases of the Stomach, Liver, and other Organs of Digestion	48
Diseases of the Kidneys, &c.....	5
Childbed	7
Ovarian Dropsy	0
Diseases of Uterus, &c.	7
Rheumatism	1
Diseases of Joints, &c.	2
Ulcer	0
Fistula	0
Diseases of Skin, &c	0
Diseases of Uncertain Seat	93
Old Age or Natural Decay.....	45
Deaths by Violence, Privation, or Intemperance	31
Causes not specified	1
Deaths from all Causes	763

METEOROLOGICAL JOURNAL.

June.		THERMOMETER.		BAROMETER	
Wednesday	9	from 46 to 63		29.90	Stat.
Thursday	10	42	67	29.79 to 29.66	
Friday	11	44	56	29.65	29.74
Saturday	12	45	61	29.75	29.92
Sunday	13	37	67	29.94	30.01
Monday	14	35	68	29.98	29.95
Tuesday	15	52	65	29.85	29.99

Wind N. and N.W. from the 9th till 14th; W. and N. N.E. on the 15th.
Except on the mornings of the 9th, and three following days, generally clear; a little rain on the 11th and following day.
Rain fallen, .015 of an inch.

CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, JUNE 25, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XL.

*Intermittent Fever. Phænomena of an
Ague Fit. Species and varieties of In-
termittents. Predisposing causes.*

I AM now to enter upon the consideration of that disorder of which the trivial English name is *ague*, and which is called by nosologists *intermittent fever*. This is one of the diseases which is known to us only in its group of symptoms. Before we can enquire successfully into its history, it is necessary that we have the group of symptoms which characterize the complaint set fairly before us. I must first, therefore, describe the *phænomena* of *ague*.

You will observe that *ague* resembles several other maladies that essentially belong to the nervous system, in being *paroxysmal*. A certain series of symptoms occurs, and then the patient reverts to a state of health: but this alternation commonly happens (or would happen if the disease were left to itself) a great many times. You may therefore look upon this succession of attacks as so many repetitions of a short distemper; or you may regard the whole period during which the attacks continue to recur at short intervals, as being occupied with one single disease.

An *ague fit* is composed of three distinct stages; and they are named, from the *phænomena* that respectively characterize them, the *cold*, the *hot*, and the *sweating* stages.

A person who is on the brink of a *paroxysm* of *ague*, experiences a sensation of debility and distress about his epigastrium;

becomes weak, languid, and listless, and unable to make any bodily or mental exertion. He begins to sigh, and yawn, and stretch himself; and he soon feels himself chilly, particularly in the back along the course of the spine; the blood deserts the superficial capillaries; he grows pale, his features shrink, and his skin is rendered dry and rough, and drawn up into little prominences, such as may at any time be produced by exposure to external cold, and presenting an appearance somewhat like the skin of a plucked goose: hence it is called *goose's skin*, and in Latin *cutis anserina*. Presently the slight and fleeting sensation of cold, first felt creeping along the back, becomes more decided and more general; the patient *feels* very cold, and he *acts* and *looks* just as a man does who is exposed to and subdued by intense cold; he trembles and shivers all over; his teeth chatter, and sometimes so violently that such as were loose have been shaken out; his knees knock together; his hair bristles slightly, from the constricted state of the integuments of the scalp; his face, lips, ears, and nails turn blue; rings which before fitted closely to his fingers become loose; his respiration is quick and anxious; his pulse frequent sometimes, but feeble; and he often complains of pains in his head, back, and loins: all the secretions are usually diminished; he may make water often, but generally he voids but little, and it is pale and aqueous; his bowels are confined, and his tongue is dry and white.

After this state of general distress has lasted for a certain time, it is succeeded by another of quite an opposite kind. The cold shivering begins to alternate with flushes of heat, which usually commence about the face and neck. By degrees the coldness ceases entirely; the skin recovers its natural colour and smoothness; the collapsed features and shrunk extremities resume their ordinary condition and bulk. But the reaction does not stop here; it

goes beyond the healthy line. The face becomes red and turgid; the entire skin hot and pungent and dry; the temples throb; a new kind of headache is induced; the pulse becomes full and strong, as well as rapid; the breathing is again deep, but oppressed; the urine is still scanty, but it is now high coloured; the patient is exceedingly uncomfortable and restless. At length another change comes over him: the skin, which, from being pale and rough, had become hot and level, but harsh, now recovers its natural softness; a moisture appears on the forehead and face; presently a copious and universal sweat breaks forth, with great relief to the feelings of the patient; the thirst ceases; the tongue becomes moist; the urine plentiful but turbid; the pulse regains its natural force and frequency; the pains depart; and by and by the sweating also ceases, and the patient is again as well, or nearly as well, as ever.

This is surely a very remarkable sequence of phenomena: and they would appear still more remarkable if they were less familiar to us. The earlier symptoms are all indicative of debility, and of a depressed state of the nervous functions. There is the same sensation of exhaustion, and incapacity of exertion, which are produced by fatigue. The sighing, yawning, and stretching, are all indications of debility. The paleness of the surface, and constriction of the skin and collapse of the features, are all owing to the retirement of the blood from the superficial capillaries. The skin shrinks, but the parts containing the bulbs of the hairs cannot contract so much as the other parts, and therefore the surface becomes rough, and the hairs bristle up, or become erected in some degree. *Horripilatio* is the learned term for this state of the surface. The coldness of the skin is another consequence of the emptiness of its blood-vessels, and the tremors, which are always indicative of debility, seem to depend upon the coldness: the chattering of the jaws has been said to be so violent as to fracture the patient's teeth. This you can believe or not as you please, but certainly the whole bed is often strongly shaken by the shiverings of the patient. The necessary accumulation of the blood in the larger and internal vessels offers a reasonable explanation of the distressed and anxious breathing.

In their attempts to render a "ratio symptomatum," authors have sometimes spoken of the hot stage as though it were a necessary consequence of the cold. But if the latter, the cold fit, be in any sense or degree the cause of the hot fit, it can only be so partially; there must be some other cause, for these reasons: the cold stage may occur and never be followed by the hot; or the hot stage may come on without any

previous cold stage; and when they do both happen, they are not by any means proportioned to each other. When we thus see that a supposed cause is not always followed by the effect, or that the effect is produced without the agency of the supposed cause, and also that the supposed cause and effect are not proportioned to each other, we cannot but conclude that the supposed cause is at most but a partial and accessory cause. We can more easily conceive how the hot fit may conduce to bring on the sweating stage: the stronger action of the heart and the more forcible propulsion of the blood will fill the superficial vessels, and in this way the natural secretions may be restored. We see exactly the same thing happen when the force of the circulation is increased by exercise: the extreme vessels seem to relax, and sweat ensues.

There are many curious facts to be observed in respect to the paroxysm of an intermittent, such as it has been now, in general terms, described. In the first place the paroxysm *returns*. Cullen makes this a part of his definition; and quibbling objections to his statement have been made, which are scarcely deserving of mention. Thus it is said that this circumstance should not have been introduced into the definition, because it is not *necessarily* or *universally* true; that the patient *may die* in the *very first* paroxysm; or that he may be cured by the proper remedies of ague, *before a second* paroxysm has time to shew itself. But all this is trifling. The paroxysms, if the disease be left to itself, will recur for a certain length of time; and, unlike the paroxysms in many of the spasmodic diseases which we have lately been speaking of, they recur at regular periods, and often with singular exactness. This is a circumstance which we should waste our time in attempting to account for. Dr. Cullen has tried to explain it on the principle of some diurnal *habit* of the body; but the truth is, that no *satisfactory* explanation of it has ever been given, and we must be content, for the present at least, to receive it as an ultimate fact; and doubtless a very strange and interesting fact.

In distinguishing some equally curious varieties of these successions and alternations of disorder and health, certain terms have, by common consent, been adopted by pathologists, which terms it is necessary that I should explain. The period which elapses between the *termination* of one paroxysm and the commencement of the next is called an *intermission*; while the period that intervenes between the *beginning* of one paroxysm and the beginning of the next, is called an *interval*. As the paroxysms are liable to vary in length, the intermissions may be very unequal, even when the intervals

are the same. When the intermissions are perfect and complete, the patient resuming the appearance and sensations of health, the disorder is an *intermittent* fever. When the intermissions are imperfect, the patient remaining ill and feverish and uncomfortable in a less degree than during the paroxysm, then the complaint is said to be a *remittent* fever.

But, confining ourselves for the present to intermittents, it is another curious property of this complaint that, although the intervals are commonly constant in each case, and quite regular, they differ in duration in different cases. Upon this circumstance is founded a division of agues into species. When the paroxysm occurs at the same hour *every day*, the patient is said to have *quotidian* ague. When it comes on at the same hour *every other day*, appearing and remaining absent day by day alternately, he is said to labour under *tertian* ague. The paroxysm, strictly speaking, repeats itself every *second* day; and if the species I first mentioned be fitly termed *quotidian*, that in which the fits occur on alternate days ought to be styled *secundan*. But nosologists have chosen to reckon the day on which the preceding fit happens as the first; and then the day on which the succeeding fit will happen, in the species now under consideration, is the third. In the same way, when a paroxysm absents itself for two whole days, and then recurs, the complaint is called a *quartan* ague. These are the three principal species or types of intermittent fever. Of course it follows, from what I have been stating, that in the *quotidian* type the interval is 24 hours; in the *tertian*, 48; and in the *quartan*, 72.

Each of these types has some other characters peculiar to itself. Thus, the paroxysms of the *quotidian* ague begin in the morning; those of the *tertian*, at noon; those of the *quartan*, in the afternoon. These are the *rules*. You are not to expect to find them always or rigidly observed; for the most part you will find that they *are* observed. It is probable that *quotidian* paroxysms, occurring at noon or at night, have sometimes been ascribed to ague, when they were merely symptoms of some local disease or inflammation; or perhaps accessions of *hectic* fever. It is observed also of the paroxysms, that when the disease is about to yield, they often occur later day after day, before they take their final departure. This is called *postponing*; and when they occur earlier than their stated hour, the paroxysms are said to *anticipate*. Now a postponing *quotidian* may be deferred till noon. But when the disease is pursuing its regular undisturbed course, the rule is such as I have mentioned.

The three principal types differ from each

other, not only in their respective intervals, and in the periods of the day at which the paroxysms severally commence, but also in the *duration* of the paroxysms; and in the proportions which the stages of these paroxysms bear to each other. The average duration of the paroxysm in the *quotidian* is ten or twelve hours; and of course the average duration of the intermission is nearly as much. The *tertian* paroxysm commonly begins at noon, and is finished the same evening; its average duration may be estimated at six or eight hours. And that of the *quartan* does not exceed four or six hours.

You will observe also that while the *quartan* has the longest interval and the shortest paroxysm, it has the longest cold stage; while the *quotidian* has the shortest interval and the shortest cold stage, but the longest paroxysm. To express these facts in mathematical language, the length of the paroxysm varies inversely as the length of the cold stage; inversely also as the length of the interval.

Of these three principal types or species the *tertian* is by much the most common; but the *quotidian* and *quartan* are neither of them unfrequent where ague is rife.

I should tell you that there are other types also spoken of, as *quintans* and *sextans*; but they are scarcely worth our attention. It is probable that when they are observed (and that is very rarely) they are merely irregular *quartans*, postponing perhaps for a day or two. They never prevail epidemically. Galen describes one of these; so does Van Swieten. Boerhaave talks of a *septiman*, and even *octavans* are mentioned; or if you want still more of the marvellous, Pliny, the naturalist, informs us that a certain *Improvisatori* was in the habit of having a paroxysm once a year, and that exactly on his birth-day; yet he died at a good old age.

There are, however, some curious modifications of the three principal types; or rather of two of them, the *tertian* and the *quartan*. For instance, a paroxysm may occur daily, and yet the ague not be of the *quotidian* type, but of the *tertian*. The paroxysm of one day will differ from the paroxysm of the next, but exactly resemble that of the third day; while the paroxysm of the second will be like that of the fourth; and so on alternately. And these differences will be decidedly marked; the paroxysms of two consecutive days will come on at different hours, and will differ in duration and severity. This form of ague is called the *double tertian*. One case of this kind, very distinctly characterized, was some time ago under my care in the hospital.

There is another form of *double tertian*. Two fits will occur on the same day—Monday for example, one in the morning, the

other in the evening; on Tuesday there shall be no fit; on Wednesday again two; on Thursday none; and so on. The Latin nomenclature is more precise than the English in denoting these variations. The form I have last mentioned, in which two dissimilar paroxysms occur every other day, is called *tertiana duplicata*; while the other form, in which there is a fit every day, but those on the alternate days resemble each other, is called *tertiana duplex*.

In the same way you may have a double quartan. In that case, a paroxysm occurs on two days in succession, and leaves the third day free; then it returns on the fourth day as it did on the first, and on the fifth as it did on the second, and leaves the sixth day free like the third, and so on. This is the *quartana duplex*. But two fits may happen on one day: say on Monday; none on Tuesday or Wednesday; and two again on Thursday. This is the *quartana duplicata*. Nay, the paroxysm of quartan ague may recur every day, and so far resemble a quotidian; but the fit of the first day will differ from those of the second or third, and resemble that of the fourth; the fit of the second day will be dissimilar from that of the first or third, and like that of the fifth; and the fit of the third will be unlike that of either of the two preceding days, and find its counterpart in that of the sixth. This is a triple quartan; and where three paroxysms occur on the first day, which we will again suppose to be Monday, and none on Tuesday or Wednesday, but three again on Thursday, corresponding respectively to the first three, we have the *quartana triplicata*. And there are other complications still, with which I need not trouble you. In Dr. Cleghorn's book on the diseases of Minorca, you may find a very good and authentic account, evidently drawn from nature, of the irregular types and varieties of ague. They are well worthy of the attention of any among you who may be likely to practise abroad.

Some physicians have used the words *double tertian*, and so on, in the literal sense, and have supposed that two or more distinct agues co-existed. This savours a good deal of the error that I formerly warned you against, of looking upon diseases as separate entities, and not merely as modes of being and of acting different from those which are proper to the state of health. I refrain from speculating on so doubtful and obscure a subject, and limit myself to the exposition of well-ascertained facts.

Besides these varieties in *type*, some other deviations from the normal and regular paroxysms require to be noticed.

Sometimes the paroxysm is *imperfect*: it is shorn of one or more of its stages: the heat and sweating occur without any previous

rigors; or the patient shakes, but has no subsequent heat; or the sweating stage is the only one of the three that manifests itself. These imperfections are often noticeable when the complaint is about to take its departure; but they may also occur at other periods of the disease. Sometimes there is no distinct stage at all; but the patient experiences frequent and irregular chills, is languid and uneasy, and depressed. This state is commonly known among the inhabitants of our fenny and aguish districts as the *dumb ague*, or the *dead ague*; the patient is said not to *shake out*.

Again, there is often observed a tendency to a change of type in the course of the same disease affecting the same person. The quotidian will be transformed into a tertian; a tertian into a quartan; or, on the other hand, a quartan into either of these. I have already noticed the fact that the paroxysms will also alter their time of invasion, sometimes coming later and later in the day, at each recurrence, sometimes earlier and earlier. When the paroxysm thus *postpones*, the disease is growing milder; when it *anticipates* its usual period of attack, the disease is increasing in severity. The postponement or anticipation, therefore, of the fit, has a close relation to the prognosis.

There are yet other cases, in which from first to last no determinate type or order of succession is observed by the paroxysms; and these cases authors speak of as *erratic* forms of ague.

There are also many modifications or complications observable in the symptoms which constitute the fits. Occasionally each paroxysm is attended by violent delirium; this is most common, I believe, in the hot stage. This symptom has been known to be almost constant throughout an epidemic. Sometimes the patient is convulsed in the paroxysms; or syncope comes on; or tetanic symptoms; or petechiæ take place on the skin, and disappear with the paroxysm. These deviations from the common and regular kind and order of the symptoms may sometimes depend upon the constitutional predispositions of the person affected; but there is another way also in which they may be explained. I shall presently have a good deal to say upon the one grand—I may say *sole*—exciting cause of intermittents. Now exposure to that cause, a residence in aguish districts, will sometimes impart a periodic character to *other diseases*: and I apprehend that this explanation will apply to many of the instances which have been observed of hysterical, tetanic, or other paroxysmal complaints, occurring at perfectly regular intervals.

The duration of ague—of the whole disease, and not merely of a separate paroxysm—it is not easy to estimate. If persons

who laboured under it were always removed at once from the influence of the exciting cause, and were always suffered to remain without treatment calculated to check the malady, we might then find materials for determining its average natural duration. But we have not these data. In point of fact, agues sometimes consist of a very few paroxysms only, half a dozen, or four, or three, or even of one fit; and on the other hand, they may be protracted over a space of several weeks, or months; nay of many years.

An ague may attack a person at any time; but they are much more common in spring, and in autumn, than in the other seasons of the year: so that you will hear and read a good deal of *vernal* intermittents, and of *autumnal* intermittents. Now the autumnal agues are, *cæteris paribus*, the more severe and dangerous. The quotidian is most common in the spring; the quartan in the Autumn; and the tertian is frequently met with both as a vernal and as an autumnal ague. You will bear in mind that in all this I am stating the prevailing *rules*; which are liable to numerous exceptions.

Ague is one of those disorders of which (like common inflammation) all persons, at all periods of their existence, seem to be susceptible, when submitted to the influence of the specific exciting cause. Individuals of all ages, from sucking infants to persons of four score, are liable to it, but they are not *equally subject* to it. It is less likely (*cæteris paribus*) to affect the very young, and the aged, than those of middle life. However, the very old are by no means exempt from the operation of the cause of ague: and with respect to the very young, some extremely curious statements have been made. It is said that persons have had ague before they were born. We know that the period of intra-uterine life is obnoxious to many forms of disease; for we trace the consequences of such disease, in visible changes of structure, immediately after birth. Pulmonary tubercles constitute one malady to which the foetus in utero is liable: hydrocephalus is another: acute inflammation of the peritoneum a third. And there can be no doubt that various specific poisons influence, occasionally, the included being, even although they may have no sensible effect upon the parent. The foetus may thus contract small-pox, which sometimes proves fatal to it, sometimes not. The daughter of my bed-maker at Cambridge had a child ill of whooping-cough in the house with her while she was in the last months of pregnancy; and the infant in the womb must have caught the disease, for I was assured that he whooped the very day he came into the world. The sins of the parent are thus visited often upon the child, when, before its first breath is drawn,

its frame is contaminated by the virus of syphilis. And in like manner unborn infants are capable of being affected by the poison that produces ague. One case in proof of this is recorded by Dr. Russell, in his *History of Aleppo*. The woman had tertian ague, which attacked her, of course, every other day: but on the alternate days, when she was well and free, she felt the child shake; so that they both had tertian ague, only the paroxysms happened on alternate days. Bark was prescribed for her; and it cured the little one first, and afterwards it cured the mother.

One probable reason why ague more commonly affects persons about the middle period of life, than those near its extremes, is, that the former are much more likely to be exposed to the primary exciting cause. And the same reason may be given, I presume, for another fact; viz. that the complaint is much more frequently seen in men than in women.

Among the circumstances which predispose to ague, debility has a powerful influence. It is important to be aware of this, as it concerns the prophylaxis, and the management of the patient after the disease has been subdued. Soldiers have been exposed to the exciting cause, without becoming affected by it, while strong and in good health; and have fallen ill of intermittent fever upon being weakened by exertion and fatigue. When I have told you that debility, any how produced, constitutes a predisposition to intermittent fever, I need scarcely add that all the multiform causes of debility may also be regarded as predisposing causes of this same disease; as they are of so many others.

But the strongest predisposing cause of all is an actual occurrence of the disease itself. The effect of former intermittents upon the system is such that the complaint may be reproduced by agencies which under any other circumstances would be quite inoperative in exciting ague. I have stated already my persuasion that, strictly speaking, there is but one exciting cause of intermittent fever; but in making that statement I refer to its *first production*. The disease leaves the body in a condition in which other injurious influences may, of themselves, be sufficient to renew it. It brings into play a new order of exciting or rather of re-exciting causes. If a person were never exposed to the malaria, he could never, as I believe, have ague: but, having once had ague, he may many times have it again, although he should never again be subjected to the direct influence of the malaria. The late Dr. James Gregory, of Edinburgh, had a brother-in-law who illustrated well in his own person the effects of predisposing circumstances in respect to ague. This gentleman was a strong active man, and commanded a battalion in the

West Indies; and he escaped for a long time, while others were falling down around him in remittent fever. At last he was wounded by a musket-ball which passed through his shoulder. He insisted, much against the will of the surgeon of the regiment, on resuming his duties before his strength was completely re-established; and the consequence was that he was immediately attacked by a remittent fever of such violence, that his life for sometime was despaired of. But this was not all. The remittent disease assumed by degrees a distinctly intermittent form, and became a tertian; and at last he got well, and strong, and came over to this country. But for a long while, though to all appearance his health was re-established, ague fits would from time to time occur; and they came precisely at the day and hour on which they would have happened if the tertian had continued with its original type; and slight causes were sufficient to reproduce them. He had marked, in an almanack, the days of the expected accession; and on those days it recurred, for some time, whenever the *east wind blew*. This very circumstance, the east wind, is a *common* re-exciting cause in such cases; exposure to cold in any way is another.

LECTURES
ON THE
FUNCTIONS OF THE NERVOUS
SYSTEM.

BY W. B. CARPENTER, M.D.

*Special Functions of the Spinal Cord.
Their independence of Sensation. Res-
piratory Movements.*

IN the general fact, that the Spinal Cord constitutes a distinct centre, or rather a collection of centres, of nervous influence, and that its operations are carried on through the nervous trunks with which it is connected, all physiologists are now agreed. It is further generally admitted that its functions are independent of the will; and that they are in effect frequently opposed to those of the brain, which operates on the muscles either by a *volitional* or an *emotional* impulse. And, lastly, its actions are always (except when excited by a physical irritation directly applied to itself) entirely of a *reflex* character: that is to say, the motor impulses which originate in it are not spontaneous, but result from the stimulus of impressions, conveyed to it by the afferent trunks, and operating upon it, to use the expression of Prochaska, according to certain "peculiar laws written, as it were, by nature on its medullary pulp." It is not, however,

universally admitted that these actions are independent of *sensation*; and some eminent physiologists, among whom may be named Dr. Alison, still hold that the intervention of sensation is necessary,—in the cases, at least, of the ordinary associated movements, which have definite ends in view, and follow one another in regular succession, as those of respiration,—for an impression to give rise to that organic change in the spinal cord which shall terminate in a muscular motion*. It will be desirable, therefore, to consider the evidence upon which the statement rests, that reflex actions are independent of sensation, though ordinarily accompanied by it.

In the first place, then, it has long been well known that, in the human being, the spinal cord does not itself possess in the remotest degree the attribute of sensibility; since, when its lower portion has been severed from the brain by injury or disease, there is complete anæsthesia of all the parts of the body which derive their nerves exclusively from it. Hence it might be inferred, that, throughout the Vertebrated classes, the spinal cord is equally destitute of sensibility; and that whatever movements may be produced by stimuli acting through it, are the results of a physical, and not of a sensorial change. This inference, however, has been disputed; and, if unsupported by other evidence, it would not, perhaps, be entitled to rank as an ascertained truth. The very performance, by decapitated animals of inferior tribes, of actions which had not been witnessed in man under similar circumstances, was held to indicate that the spinal cord in them had an endowment which his did not possess. The possibility of such an explanation—however unconformable to that analogy throughout organized nature which, the more it is studied, the more invariably does it guide to truth—could not be disproved. Whatever experiments on decapitated animals were appealed to in support of the doctrine that the brain is the only seat of sensibility, could be met by a simple denial that the spinal cord is every where as destitute of that endowment as it appears to be in man. The cases of profound sleep and apoplexy might be appealed to, as examples of reflex action without consciousness; and these might be met by the assertion, that in such conditions sensations are felt though they are not remembered. It is difficult, however, to apply such an explanation to the case of anencephalous human infants (in which all the ordinary reflex actions have been exhibited with an entire absence of brain), without supposing that the medulla oblongata is the seat of a sensibility which we know that the lower part of the spinal

* See Outlines of Physiology, 3d edit. p. 211.

cord does not possess; and of this there is no evidence whatever.

Experiments on the lower animals, then, and observation of the phenomena manifested by apoplectic patients and anencephalous infants, *might* lead to the conclusion that the spinal cord does not possess sensibility, and that its reflex actions are independent of sensation. At this conclusion, Prochaska, Sir G. Blane, Flourens, and other physiologists, had arrived; but it was not until special attention was directed to the subject by Dr. M. Hall, that facts were obtained, by which a positive statement of it could be supported. For the question might have been continually asked,—If the spinal cord in man is precisely analogous in function to that of the lower Vertebrata, why are not its reflex phenomena manifested when a portion of it is severed from the rest by disease or injury? The answer to this question is twofold. In the first place, simple division of the cord with a sharp instrument leaves the separated portions in a state of much more complete integrity, and therefore in a state much more fit for the performance of its peculiar functions, than it ordinarily is after disease or violent injury; and as the former method of division is one which the physiologist is not likely to meet with in man, and cannot experimentally put in practice, the cases in which reflex actions are manifested are likely to be comparatively few. But, secondly, a number of such instances *have* now been accumulated, sufficient to prove that the occurrence is by no means so rare as might have been supposed; and that nothing is required but patient observation, to throw great light on this interesting question from the phenomena of disease. A most valuable collection of such cases, occurring within his own experience, has been published by Dr. W. Budd, in the *Medico-Chirurgical Transactions*, vol. xxii. The leading facts observed by him will be now enumerated.

In the first case, paraplegia was the result of angular distortion of the spine in the dorsal region. The sensibility of the lower extremities was extremely feeble, and the power of voluntary motion was almost entirely lost. “When, however, any part of the skin is pinched or pricked, the limb that is thus acted on jumps with great vivacity: the toes are retracted towards the instep, the foot is raised on the heel, and the knee so flexed as to raise it off the bed; the limb is maintained in this state of tension for several seconds after the withdrawal of the stimulus, and then becomes suddenly relaxed.” “In general, while one leg was convulsed, its fellow remained quiet, unless stimulus was applied to both at once.” “In these instances, the pricking and pinching was perceived by the patient; but *much*

more violent contractions are excited by a stimulus, of *whose presence he is unconscious*. When a feather is passed lightly over the skin, in the hollow of the instep, as if to tickle, convulsions occur in the corresponding limb, much more vigorous than those induced by pinching or pricking; they succeed one another in a rapid series of jerks, which are repeated, as long as the stimulus is maintained.” “When any other part of the limb is irritated in the same way, the convulsions which ensue are very feeble, and much less powerful than those induced by pricking or pinching.” “Convulsions, identical with those already described, are at all times excited by the acts of defecation and micturition. At these times, the convulsions are much more vigorous than under any other circumstances, insomuch that the patient has been obliged to resort to mechanical means to secure his person while engaged in these acts. During the act of expulsion, the convulsions succeed one another rapidly, the urine is discharged in interrupted jets, and the passage of the *fæces* suffers a like interruption.” The convulsions are more vigorous the greater the accumulation of urine; and involuntary contractions occur whenever the bladder is distended, and also when the desire to relieve the rectum is manifested. “In all these circumstances, the convulsions are perfectly involuntary; and he is unable, by any effort of the will, to control or moderate them.” The patient subsequently regained, in a gradual manner, both the sensibility of the lower extremities, and voluntary power over them; and as voluntary power increased, the susceptibility to involuntary movements, and the extent and power of these, diminished.

This case, then, exhibits an increased tendency to perform reflex actions, when the control of the brain was removed; it also shows that a slight impression upon the surface, of which the patient was not conscious, was more efficacious in exciting reflex movements than others more powerfully affecting the sensory organs. This is constantly observed in experiments upon the lower animals; and it harmonises, also, with the important fact, that when the *trunk* of an afferent nerve is pinched, pricked, or otherwise irritated, the reflex function will not be nearly so strongly excited, as when a gentler impression is made on a *surface* supplied by the branches of this nerve. The former produces *pain*, whilst the latter does not; the amount of sensation, therefore, does not at all correspond with the intensity of reflex action, but rather bears a converse relation to it. Mr. Grainger found that he could remove the entire hind leg of a salamander with the scissors, without the creature moving, or giving any expression of

suffering, if the spinal cord had been divided; yet that, by irritation of the foot, especially by heat, in an animal similarly circumstanced, violent convulsive actions in the leg and tail were excited.

It should be added that, in the foregoing case, the nutrition of the lower extremities was not impaired, as in most cases of paraplegia. The rationale of this occurrence, which is a phenomenon to be constantly observed when the reflex actions of the part remain entire, will be hereafter noticed.

In another case the paralysis was more extensive, having been produced by an injury (resulting from a fall into the hold of a vessel) at the lower part of the neck. There was at first total loss of voluntary power over the lower extremities, trunk, and hands; slight remaining voluntary power in the wrists, rather more in the elbows, and still more in the shoulders. The intercostal muscles did not participate in the movements of respiration. The sensibility of the hands and feet was greatly impaired. There was retention of urine, and involuntary evacuation of the fæces. Recovery took place very gradually; and during its progress several remarkable phenomena of reflex action were observed. At first, tickling one sole excited to movement that limb only which was acted upon; afterwards, tickling either sole excited both legs, and on the 26th day, not only the lower extremities, but the trunk and upper extremities also. Irritating the soles, by tickling or otherwise, was at first the only, and always the most efficient method, by which convulsions could be excited. From the 26th to the 69th day, involuntary movements in all the palsied parts continued powerful and extensive, and were excited by the following causes:—In the lower extremities only, by the passage of flatus from the bowels, or by the contact of a cold urinal with the penis; convulsions in the upper extremities and trunk, attended with sighing, by plucking the hair of the pubes. On the 41st day, a hot plate of metal was applied to the soles, and found a more powerful excitor of movement than any before tried. The movements continued as long as the hot plate was kept applied; but the same plate, at common temperature, excited no movements after the first contact. The contact was distinctly felt by the patient; but *no sensation of heat* was perceived by him, although the plate was applied hot enough to cause vesication. At three different intervals the patient took one-eighth of a grain of strychnia three times a day. Great increase of susceptibility to involuntary movements immediately followed, and they were excited by the slightest causes. No convulsions of the upper extremities could ever be produced, however, by irritating their integument; though, under the influence of strychnia, pulling the hair of the

head, or tickling the chin, would produce violent spasmodic actions in them. Spontaneous convulsions of the palsied parts, which occurred at other times, were more frequent and more powerful after the use of strychnia.

On the first return of voluntary power, the patient was enabled to restrain in some measure the excited movements; but this required a distinct effort of the will, and the first attempts to walk were curiously affected by the persistence of the susceptibility to excited involuntary movements. When he first attempted to stand, the knees immediately became forcibly bent under him; this action of the legs being excited by contact of the soles with the ground. On the 95th day this effect did not take place until the patient had made a few steps; the legs then tended to bend up, a movement which he counteracted by rubbing the surface of the belly: this rubbing excited the extensors to action, and the legs became extended with a jerk. A few more steps were then made; the manœuvre repeated, and so on. This susceptibility to involuntary movements from impressions on the soles gradually diminished; and on the 141st day, the patient was able to walk about, supporting himself on the back of a chair which he pushed before him; but his gait was unsteady, and much resembled that of chorea. Sensation improved very slowly: it was on the 53d day he first slightly perceived the heat of the metal plate.

This important case suggests many interesting reflections. Common sensation was not so completely abolished as in the former instance; but of the peculiar kind of impression, which was found most efficacious in exciting reflex movements, no consciousness whatever was experienced. Not less interesting was the circumstance, that convulsions could be readily excited by impressions on surfaces above the seat of injury; as, by pulling the hair of the scalp, a sudden noise, and so on. This proves two important points: first, that a lesion of the cord may be such as to intercept the transmission of voluntary influence, and yet allow the transmission of that reflected from incident nerves. Secondly, that all influence from impressions on incident nerves are diffused through the cord; for, in the instance adduced, the reflected influence was undoubtedly not made to deviate into the cord by the morbid condition of that organ, but followed its natural course of diffusion, being rendered manifest in this case by the convulsions which were excited in consequence of increased activity of the motor function of the cord. It is obvious, therefore, that when we consider the spinal cord as an organ complete in itself, the terms *direct* and *retrograde* have no application; these being

justly applied only when there is a determinate direction for the transmission of a change in the condition of the nerves : as when a sensory impression is propagated along an afferent trunk, or upwards to the brain in that part of the spinal cord which is a mere conductor ; or when a motor impulse is propagated downwards from the brain through the conducting portion of the spinal cord, or by an efferent trunk. If, in either of these cases, the course of transmission could be reversed, it might be fairly said to be *retrograde* ; but the true spinal cord is not a conducting organ, but a ganglionic centre for the reception and reflexion of impressions, which are diffused through it in all directions alike. It is further interesting to remark that, in the foregoing case, the reflex actions were very feeble during the first seven days, in comparison with their subsequent energy ; being limited to slight movements of the feet, which could not always be excited by tickling the soles. In another case of very similar character, it was three days after the accident before any reflex actions could be produced. It is evident, then, that the spinal cord must have been in a state of concussion, which prevented the manifestation of its peculiar functions, so long as the effect lasted ; and it is easy, therefore, to perceive that a still more severe shock might permanently destroy its power, so as to prevent the exhibition of any of the phenomena of reflex action.

It seems well established, then, by such cases, that the spinal cord or small segments of it may serve in man as the centre of very energetic reflex actions, when the voluntary power exercised through the brain over the muscular system is suspended or destroyed. And it is further evident that these movements are produced by a mere physical change in the nervous centres, the consciousness of the individual not being affected in their performance, and sensation having therefore no necessary participation in them. The movements witnessed in the lower animals under the same circumstances being altogether of a similar character, there seems no good reason to attribute to the spinal cord in them an attribute of which it is certainly destitute in man. There is no essential difference either in structure, or in the nature of the actions performed by them, between the spinal cord and the medulla oblongata, which can warrant us in assigning to the latter a function which the former does not possess : and if the reflexions of the spinal cord do not involve sensation, there is good reason for concluding that this change does not necessarily participate in those of the medulla oblongata. It is perfectly true that it always *accompanies* in us the greater number of actions to which that division of the centre is subservient ; for example, those of respiration and deglutition. And it is scarcely possible for such an accident to occur in the

human being, as the separation of the medulla oblongata from the brain, without the destruction of the independent functions of both : it is not likely that we can ever have the power of ascertaining by the testimony of a patient so affected, that the respiratory movements are performed without the necessary intervention of sensation, as we have been able to do in regard to other reflex movements ; but the general facts being, that there is no positive ground whatever for regarding any part of the spinal cord as a *sensorium* independent of the brain, and that the respiratory movements certainly correspond in all their conditions with the actions denominated reflex, there would seem no good reason for maintaining that sensation is an element to *their* production, whilst it is admitted to be not essential in the case of the less regular convulsive actions already described. The character of *adaptiveness* to a designed end, in regard to their combination and succession, which the movements of respiration and deglutition exhibit, is clearly no proof of their dependence on sensation ; since an equally perfect adaptiveness is witnessed in the actions of the heart, alimentary canal, &c. which are still further removed from the control of the will. And, further, it does not appear by any means evident what end or purpose could be answered by the production of sensation as a part of the chain of phenomena of reflex action. The question is, are these movements guided in any way by the *mind* ; or do they necessarily result from certain physical conditions of the nervous system ? If their adaptiveness is the result of mental guidance, then not only sensation but judgment and volition must necessarily be involved ; since it is impossible that sensation can guide to the choice of one out of many modes of action, without the exercise of these faculties. On the other hand, if it be said that certain movements are necessarily associated with certain sensations, it is difficult to see why they should not be equally associated with the impressions by which the sensations are produced. Sensation is a *psychical* phenomenon. It is the communication to the mind of a certain organic change in the nervous system. It is the first step in the train of purely mental operations ; and these terminate in the formation of an emotional or volitional impulse, which reacts on the body. But we have no reason to believe that sensation can itself react on the body ; or that, if it could, it would be a better guide than the impression which produced it. Thus—

Impression *a* produces
sensation A, with which is associated
motion α ;
and in like manner,
impression *b* produces
sensation B, with which is associated
motion β .

There seems no valid reason, then, to assert that a motion may not have an equally close connexion with the impression, as it is asserted to have with the sensation resulting from it.

The question has been often put to those who advocate this view,—why the sensation should be so constantly associated with the changes, if not essential to produce motion? An objection might fairly be made to any reasoning from final causes in a question of facts; but the inquiry may be easily answered. In many instances the production of sensations is the stimulus necessary for the excitement of other actions required for the continued maintenance of those in question. Thus, a cistern filled with water may be speedily emptied by a cock occasionally opened at the bottom; but if it communicate with a reservoir, by means of a valve opened by a ball floating on the surface of the water it contains, it may be kept constantly full. Now here the action of the ball-cock at the top is not essential to the flow of water at the bottom, but is rather consecutive upon it. The channel is opened, and the water flows out; and, in consequence of the lowering of the surface thus produced, the floating valve is opened, and the cistern refilled from the reservoir. Just so is it with regard to those movements of animals, which are concerned in the ingestion of their food. The muscular contractions required to propel it along the alimentary canal, from the stomach downwards, are provided for without even the intervention of the nervous system. To bring it within reach of these, a muscular apparatus is provided, by which anything that comes within its grasp is conveyed downwards by a reflex operation, originating in the impression made upon the surface of the pharynx. Now this, in the ordinary condition, may be considered as attended with sensation, in order that those other movements may be performed which will bring food within the reach of the apparatus of deglutition. The polype is dependent for its supplies of aliment, upon what the currents in the surrounding fluid, or other chances, bring into its neighbourhood; but anything which touches its tentacula is entrapped and conveyed into its stomach. The anencephalous infant, again, can swallow, and even suck; but it can execute no other movements adapted to obtain the supply of food continually necessary for maintenance, because it has not a mind in which sensations could awake into activity. The sensation connected with reflex actions has not only this important end, but it frequently contributes to enjoyment, as in suction and ejaculatio seminis. Now there is evidence that the latter of these processes, involving though it does the combined action of a number of muscles, and dependent as it

seems upon sensation of a very peculiar kind, may take place without consciousness on the part of the individual. Brachet mentions a case of this kind in the human subject, in which the patient's own testimony could be adduced; and he ascertained that emission could be produced in dogs, in which the spinal cord had been divided in the back, and in which, therefore, it can scarcely be doubted that the sensibility of the genital organs was destroyed. Such cases, it might be thought, are sufficient to prove that the power of the reflex function, operating independently of sensation, is not confined to such irregular convulsive movements as are seen in man after disease or injury, but is exercised in producing the regular combined actions which are necessary for the maintenance of the organic functions. The sensation accompanying these actions, moreover, frequently affords premonition of danger, or gives excitement to supplementary actions destined to remove it, as in the case of respiration; for where anything interferes with the due discharge of the function, the uneasy sensation which ensues occasions unwonted movements, which are more or less adapted to remove the impediment, in proportion as they are guided by judgment as well as consciousness. Again, sensation often gives warning against inconvenience, as in the excretory functions; and here it is very evident that its object is not only (if at all) to excite the associated muscles necessary for the excretion, but actually to make the will set up the antagonizing action of the sphincters, as will be hereafter explained. There is one unequivocal case, in the ordinary condition of the human body, of reflex action without sensation; this is the muscular contraction by which the food is propelled from the bottom of the pharynx to the stomach. Unless the morsel be very bulky, so as to press on the surrounding parts, or be very different in temperature from the surface it touches, or have any peculiar irritating quality, we are not more conscious of its presence whilst it is passing down the lower part of the oesophagus, than when it is being propelled along the intestinal tube; and yet, as Dr. J. Reid's experiments have shown, this contraction is of a reflex character, not being stimulated by direct contact, but requiring the completeness of the nervous circle for its performance.

We shall now separately consider the chief operations in which the spinal cord and its system of nerves are usually concerned, in the ordinary course of the vital actions of the human body. Upon taking a general survey of these, it will be found that their ordinary function is, to supply the conditions requisite for the maintenance of the various organic processes. Thus, the aeration of the blood, which takes place whenever that fluid is placed in relation with the atmos-

phere, can only be carried on by the regular exchange of the small quantity of the gas contained in the lungs; if this cease, the circulation is soon brought to a stand, and loss of vitality of the whole system speedily results. Hence this is most constantly necessary for all the actions of the spinal cord; and we find its maintenance, in spite of accident or disease of the spine, remarkably provided for, in the position of the centre of the respiratory movements, which occupies a position where it receives the greatest possible amount of protection. The supply of the digestive apparatus, again, is immediately dependent upon the spinal system; and this, being another essential function, has its centre equally protected. The outlets of the cavities are also controlled by the spinal system; but this function, although essential to the comfort of life, is less necessary to its maintenance; and we find it dependent upon a portion of the cord which is more liable to lose its powers by disease or injury. It is possible, as will hereafter be shown, that several actions, which are at first voluntary, are effected, when so frequently performed as to become habitual, through the medium of the spinal system: of this kind are the movements of locomotion, which are continued involuntarily, when the whole attention of the mind is given to other objects, but which the will can check at any time. This is an illustration of the superior degree in which the spinal system (or the parts answering to it) is concerned in the maintenance of the actions of the lower classes of animals. It will be remembered that in the *dytiscus*, whose head had been removed, the stimulus of the contact of water immediately excited regular and continued locomotive actions, which lasted for some time. So in the cases already quoted, when the control of the will over the lower extremities was lost, powerful muscular actions were excited through the spinal cord alone. In the healthy condition of the human system, when the will is controlling all the movements which are not immediately concerned in the maintenance and regulation of the organic functions, no such actions can be excited; but in proportion as its control is lost, does the independent power of the spinal cord manifest itself. Hence we can understand that, when the whole attention of the mind is given to other objects, but a certain train of muscular actions has been voluntarily begun, that train will continue under the influence of the constantly renewed stimulus, without anything more than a *general* sustaining and directing energy on the part of the will. This appears to me the only sound manner of explaining the combination of two sets of actions,—one habitual, the other requiring close attention,—which we so frequently meet with. According to some metaphysicians, an action, which

is originally voluntary, must ever remain so; and thus the movements of locomotion, which all admit to be in man at first the results of distinct efforts of the will, are regarded as equally dependent upon it, however habitual they may have become. Now we all know that, in walking along an accustomed road, we frequently lose even our consciousness of our situation, from the close occupation of the mind upon some train of thought, and yet our limbs continue to move under us with regularity, until we are surprised by finding ourselves at the place of our destination, or peradventure at some other which we had not intended to visit, but to which habit has conducted us. A musical performer will frequently, in the same manner, play through a piece, when the mind is occupied upon an entirely different subject: and I have known a good musician carry on an uninterrupted conversation (requiring, therefore, a distinct set of muscular actions) whilst playing at sight from notes before him. Now in such a case it would be said by some metaphysicians (acknowledging, as all do, that the mind cannot *will* two different things at the same time) that volition is in a sort of vibratory condition between the two sets of action—now prompting one, and now the other. But, independently of the manifest complexity which must attend the operations of the mind so employed, it may be shown that it is perfectly conformable to admitted facts, to refer the habitual series of actions to the shorter train afforded by the spinal system of nerves, whilst the cerebral system is concerning itself with the other. For we have many examples in which *perceptions*, which are *intuitive* in the lower animals, have to be *acquired* in us by mental operations, frequently of a complex nature; and which yet become at last as constantly and readily associated with the sensations that excite them, as they are in the lower animals. Such, for example, is the perception of the distance of an object, which guides the hand in endeavouring to seize it. It is not difficult, then, to understand that the locomotive actions may, by habit, and by the temporary suspension of the particular direction of them by the will, become nearly of the same reflex or excited character as they obviously are in the lower animals; and the more these actions are of a simple rhythmical character, similar to those of respiration, the more does it seem that they may with probability be referred to the spinal system; and if we attribute to this (as we can scarcely help doing) the rapid vibration of the wings of insects, there seems no reason why we should not extend the same view to the wings of birds. Such an explanation of their movements will account for their occasional continuance, during a period through which no

known voluntary effort can endure, without apparent fatigue; for it is one of the attributes of the spinal system of nerves, well pointed out by Dr. M. Hall, that the exercise of the muscles excited by it does not occasion fatigue, the sense of which is cerebral only.

Whether or not this last speculation be correct, it is certain that the lower we descend in the animal scale, the more important is the share performed by the reflex function in the general movements of the body; that is to say, the larger is the proportion of muscular actions performed in direct response to a stimulus, and the smaller is the proportion of those which originate in the spontaneous will of the being. And this observation, founded upon physiological phenomena alone, perfectly harmonizes with the facts supplied by anatomical research; since in the Vertebrata we may trace the gradual diminution of the cerebral hemispheres relatively to the spinal cord, until at last they come to be as it were a mere appendage to it; and among Invertebrate animals we find a similarly varying relation between the size of the cephalic ganglia, and of the scattered centres which collectively resemble the spinal cord of Vertebrata. We shall commence our survey of the reflex movements in man, with the consideration of those of respiration, which are particularly well adapted for illustrating their general nature.

Respiratory movements.—The centre of the respiratory movements is the upper part of the medulla oblongata, into which may be traced the excitor nerves that convey the stimulus on which the movements are dependent, and from which proceed, either directly or indirectly, the motor nerves by which they are carried into effect. The chief excitor of the respiratory movements is unquestionably the par vagum. When this is divided on both sides, according to the experiments of Dr. Reid, the number of respiratory movements is considerably diminished, usually about one-half. Now if this nerve excite the actions of respiration by its powerful action in producing sensation, we should expect to find its trunk endowed with considerable sensibility, which is not the case; for all experimenters agree in stating that, when its trunk is pinched or pricked, the animal does not exhibit signs of pain nearly as acute, as when the trunks of the ordinary spinal nerves, or of the fifth pair, are subjected to similar treatment. It cannot be questioned, however, that its power as an excitor of respiration is very great, since, besides the fact of the diminution in the number of inspirations which occurs immediately on section of it, irritation of its trunk in the neck is instantly followed by an act of inspiration. It is evident that this power must arise from impressions made

upon its peripheral extremities. The impression is probably due to the presence of venous blood in the capillaries of the lungs; or, as Dr. M. Hall thinks, to the presence of carbonic acid in the air-cells. Either or both may be true. The pneumogastric nerve, however, is not the only excitor of the respiratory movements; since, when the nerve is cut on each side, they still continue. Dr. Reid has satisfactorily shown that the statement of many experimenters, that the inspirations are *increased* in frequency after this operation, is erroneous; and that it has originated in their very prolonged and laborious character. The removal of the encephalon, also, diminishes the frequency of the respiratory movements, whether it be performed before or after the section of the vagi. Dr. Reid found that, in a kitten of a day old, in which the inspirations were 100 a minute, they fell to 40 when the encephalon was removed; and on subsequently cutting the pneumogastriacs, the number of inspirations instantly fell to between three and four in the minute, and continued so for some time. Hence it appears that the respiratory movements are partly dependent upon cerebral agency or volition; and this may also be learned from the prolonged and laborious character of the inspirations during sleep or profound attention, when the influence of the cerebrum is more or less suspended. But why do the movements continue, when the pneumogastriacs have been divided, and the encephalon has been removed? It is evident that there must be other exciters to the action of the respiratory muscles. Amongst these, the nerves distributed to the general surface, and particularly to the face, probably perform an important part; and in exciting the first inspiration, the fifth pair seems the principal agent. It has long been a well-known fact, that the first inspiratory effort of the new-born infant is most vigorously performed when the cool external air comes into contact with the face; and that impressions on the general surface, such as a slap of the hand on the nates, are often effectual in exciting the first inspiratory movements, when they would not otherwise commence. Dr. M. Hall relates an interesting case in which the first inspiration was delayed, simply because the face was protected by the bed-clothes from the atmosphere; and, on lifting up these, the infant immediately breathed. Every one knows the fact that the first plunge into cold water, the first descent of the streams of the shower-bath, or even dashing a glass of cold water in the face, will excite inspiratory efforts. That the nerves of the general surface are concerned in this appears from an experiment of Dr. Reid's. After dividing the pneumogastriacs, and removing the brain and cerebellum, he divided the spinal cord high up in the neck, so

as to cut off the communication between the spinal nerves and the medulla oblongata; and he found that the frequency of the respiratory movements was still further diminished, although they were not even then entirely suspended. It may be surmised, however, that the sympathetic nerve, which derives many filaments from the cerebro-spinal system, and especially communicates with the pneumogastric nerves, is one of the exciters to this function; and this, perhaps, not only through its ramifications in the lungs, which are considerable, but also by its distribution on the systemic vessels; so that it may convey to the spinal cord the impression of imperfectly-arterialized blood circulating in these, such as the pneumogastric is believed to transmit from the lungs. It will hereafter be shown that an impression of a corresponding kind is more probably the cause of the sense of hunger and thirst, than any which originates in the stomach alone.

The motor or efferent nerves concerned in the function of respiration are those which Sir C. Bell has grouped together in his respiratory system. The most important of these, the phrenic, arises from the upper part of the spinal cord; the intercostal much lower down; whilst the facial nerve and the spinal accessory, to the latter of which, as will presently be stated, the motor powers of the par vagum are chiefly due, evidently takes its origin in the medulla oblongata itself. But we must not decide upon the connexion of a particular nerve with a particular segment of the spinal cord, simply because it diverges from it at that point. It has been shown that, in the Mollusca, a nerve passing to, or proceeding from, one ganglion, frequently passes through or over another which lies in its course; and in the Articulata, this is a still more constant occurrence. It is by no means improbable, then, that the connexion of the intercostal nerves is really in part with the grey matter of the medulla oblongata; at any rate, such a connexion has not been disproved: and as this is unquestionably the chief if not the only centre of the respiratory movements; and as the white columns of the spinal cord consist of fibres which bring the spinal nerves into connexion, not only with the brain, but also with other segments of the ganglionic portion of the cord, there seems no reason why it should not be so in this instance.

That the respiratory movements, as ordinarily performed, are essentially independent of the will, appears not only from our own consciousness, but also from cases of paralysis, in which the power of the will over the muscles has been lost, whilst the movements have been kept up by the reflex action of the medulla oblongata or respiratory ganglion; whilst in others, some of the respiratory muscles have been motionless during ordi-

nary breathing, and yet have remained under the power of the will. Such cases are mentioned by Sir C. Bell, in the Appendix to his work on the Nervous System. It is difficult to ascertain from his account of the respiratory system of nerves, how far he recognized the necessity of a stimulus or impression conveyed to the medulla oblongata through afferent or excitor nerves, as the real source of the respiratory actions. He was well aware that the motor power originated in the medulla oblongata, and he clearly separated it from that of the brain, resulting from an effort of the will; and he speaks of this important function as being placed under the guidance of a sensation more certain and constant in its effects than the capricious will. But he was certainly not aware that sensation need not intervene. Of this we are enabled to judge from the phenomena presented by the human being in sleep and coma, by anencephalous foetuses, and by decapitated animals. Further, Dr. Ley has put on record a case which confirms this particular inference, just in the same manner as the cases related at the commencement of this lecture confirm the general doctrine of the non-existence of sensibility in the spinal cord. He had under his care a patient in whom the par vagum appeared to be diseased; the lungs suffered in the usual way in consequence, and the patient had evidently laborious breathing; but he distinctly said that he felt no uneasiness in his chest.

Our own experience informs us that respiratory movements are partly under the control and direction of the will, though frequently unrestrainable by it. In ordinary circumstances, when the blood is being perfectly aerated, and there is a sufficient amount of arterial blood in the system to carry on the functions of life for a short time, we can suspend the respiratory actions during a few seconds without any inconvenience. If, however, we endeavour to prolong the suspension, the stimulus conveyed by the excitor nerves to the medulla oblongata becomes too strong, and we cannot avoid making inspiratory efforts; and if the suspension be still further prolonged, the whole body becomes agitated by movements which are almost of a convulsive nature, and no effort of the will can then prevent the ingress of air. I have heard it stated, though I know not the authority, that no person ever committed suicide, though many have attempted to do so, by simply holding the breath; the control of the will over the respiratory muscles not being sufficiently great to antagonize the stimulus of the "besoin de respirer," when this has become aggravated by the temporary cessation of the action. But such persons have succeeded better, by holding the face beneath the surface of water; because here another set of muscles is called

into action, which are much more under the control of the will than are those of respiration; and a strong volition applied to these will prevent all access of air to the lungs, however violent may be the inspiratory efforts. It is easy to understand why, in the higher animals at least, and more especially in man, the respiratory actions should thus be placed under the control of the will, since they are subservient to the production of those sounds by which individuals communicate their feelings and desires to each other, and which, when articulate, are capable of so completely expressing what is passing in the mind of the speaker. If the respiratory muscles of man were no more under his control than they appear to be in the insect or molluscous animal, he might be provided with the most perfect apparatus of speech, and yet he would not be able to employ it to any advantage.

The motor power of the respiratory nerves is exercised, however, not only on the muscles which perform the inspiratory and expiratory movements, but on those which guard the entrance to the wind pipe, and also on certain other parts. The movements of the internal respiratory apparatus are chiefly, if not entirely, effected through the medium of the motor fibres, which the par vagum contains. These motor fibres exist in very different amount in its different branches. For example, the pharyngeal and oesophageal branches, by which (as will hereafter appear) the muscles of deglutition are excited to contraction, possess a much larger proportion of them, and exhibit much less sensibility when irritated, than do other divisions of the trunk. Between the superior and inferior laryngeal nerves, again, there is an important difference, which anatomical and experimental research have now very clearly demonstrated. The superior laryngeal branch is almost solely an afferent nerve, its motor endowments being limited to the crico-thyroid muscle, to which alone of all the muscles its filaments can be traced, the remainder being distributed beneath the mucous surface of the larynx. The sensibility of this nerve is very evident when it is pinched or irritated during experiments upon it. On the other hand, the inferior laryngeal nerve is almost entirely one of motion, as is shown by its very slight sensibility to injury, its nearly exclusive distribution to muscles, and its influence in exciting contraction of these when its separated trunk is stimulated.

The functions of the par vagum at its roots have been made the subject of particular examination by Valentin; and he has arrived at the very interesting and unexpected result, that it *there* possesses no motor power, but is entirely a sensory or rather an afferent nerve. He states that, if the roots be carefully separated from those of the glosso-

pharyngeal, and (which is a matter of some difficulty) from those of the spinal accessory nerve, and be then irritated, no movements of the organs supplied by it can be observed. On the other hand, if the roots be irritated whilst in connexion with the nervous centres, muscular contractions, evidently of a reflex character, result from the irritation; and strong evidences of their sensibility are also given. On the other hand, again, when the roots of the spinal accessory nerve are irritated, no indications of sensation are given; but the muscular parts supplied by the par vagum, as well as by its own trunk, are made to contract, even when the roots are separated from the nervous centres; so that these roots must be regarded as the channel of the motor influence transmitted to them from the medulla oblongata. When the par vagum swells into the jugular ganglion, an interchange of fibres takes place between it and the spinal accessory; but many more fibres can be traced from the latter into the former, than from the former into the latter. Hence it results that, of the branches into which the par vagum subsequently divides, many enjoy a high degree of motor power; whilst those of the spinal accessory do not appear to possess any great share of sensibility. The pharyngeal branches are among the most decidedly motor of all those given off from the pneumogastric; and these may in great part be traced backwards into the spinal accessory. Hence the idea of Arnold and Scarpa, that the par vagum and spinal accessory are together analogous to a spinal nerve, the former answering to the posterior roots, and the latter to the anterior, appears sufficiently probable. We shall for the present restrict ourselves to the consideration of the influence of this compound nerve on the respiratory organs. It has long been known that section of the par vagum in the neck, above the inferior laryngeals, was frequently followed by suffocation, resulting from closure of the glottis; and hence it has been inferred that the office of the inferior laryngeals was to call into action the dilators of the larynx, whilst the superior laryngeals were supposed to stimulate the constrictors. This view, however, is incorrect. It is inconsistent with the results, previously stated, of anatomical examination into the respective distribution of these two trunks; and it has been completely overthrown by the very careful and satisfactory experiments of Dr. J. Reid, which have established that, whilst the inferior laryngeal is the motor nerve of nearly all the laryngeal muscles, the superior laryngeal is the excitor or afferent nerve, conveying to the medulla oblongata the impressions by which muscular movements are excited. It has been ascertained by Dr. R. that, if the inferior laryngeal branches be divided,

or the trunk of the par vagum be cut above their origin from it, there is no constriction of the glottis, but a paralyzed state of its muscles. After the first paroxysm occasioned by the operation, a period of quiescence and freedom from dyspnœa often supervenes, the respirations being performed with ease so long as the animal remains at rest; but an unusual respiratory movement, such as takes place at the commencement of a struggle, induces immediate symptoms of suffocation, the current of air carrying inwards the arytenoid cartilages, which are rendered passive by the paralyzed state of their muscles; and these, falling upon the opening of the glottis like valves, obstruct the entrance of air into the lungs. The more effort is made, the greater will be the obstruction; and accordingly, it is generally necessary to counteract the tendency to suffocation, when it is desired to prolong the life of the animal after this operation, by making an opening into the trachea. Dr. Reid further ascertained that the application of a stimulus to the inferior laryngeal nerves, when separated from the trunk, would occasion distinct muscular contractions in the larynx; whilst a corresponding stimulus applied to the superior laryngeal occasioned no muscular movement, except in the cricothyroid muscle. But when the superior laryngeals were entire, irritation of the mucous surface of the larynx, or of the trunks themselves, produced contraction of the glottis and efforts to cough; effects which were at once prevented by dividing those nerves, and thereby cutting off their communication with the medulla oblongata. There can be no doubt, then, that the superior and inferior laryngeal branches constitute the circle of incident and motor nerves by which the aperture of the glottis is governed, and by which any irritation of the larynx is made to close the passage so as to prevent the entrance of improper substances; whilst the superior laryngeal nerve also excites the muscles of expiration, so as to cause the violent ejection of a blast of air, by which the offending gas, fluid, or solid, may be carried off. The effect of carbonic acid in causing spasmodic closure of the glottis is well known, and affords a beautiful example of the protecting character of this system of nerves.

The mucous surface of the trachea and bronchi appears, from the experiments of Valentin, to be endowed with impressibility, so that stimuli applied to it produce expiratory movements; and this evidently operates through the branches of the par vagum distributed upon the membrane. Here, as elsewhere, we find that a stimulus applied to the surface has a much more decided influence than irritation of the trunk of the nerve supplying it. Valentin has succeeded

in producing distinct contractions of the rings of the trachea, by irritating the par vagum in the rabbit; and he thinks it probable that a similar action might be induced in the bronchi and their ramifications; but this he has not succeeded in procuring. The phenomena of asthma, however, leave little room for doubt that spasmodic contraction of the air passages takes place as a reflex action, excited by various causes; and no other nerve but the par vagum can be concerned in producing it.

SOME
PHYSIOLOGICAL REFLECTIONS
ON
THE NATURE AND TREATMENT OF
ANGINA PECTORIS,
AND OF ANALOGOUS STATES.

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[For the Medical Gazette.]

[Continued from p. 342.]

What angina is not.—Symptoms, distresses, pains in the heart, thorax, cardiac plexuses; remoter pains.—Nerves involved: tingling; tumefaction.—Feelings of suffocation; in the absence of dyspnœa, constriction, globus, alarm; syncope.

IN the endeavour to enforce correct opinions of angina it has occurred to us, that it may serve an useful purpose to reflect for a moment on what angina is not.

It is not hydrops nor adhesion of the pericardium. It is not hypertrophy of the heart, nor dilatation, nor a mixed state. It is not a *permanent* contraction or openness of the aortic orifice, nor of the mitral. It is not a merely wasted nor softened heart. It is not a paralysed nor an empty heart. It is not a disease of nervous young women.

These negative assertions require no serious proof, but we may observe that the statements of our several chapters will hardly leave one of them doubtful.

A summary of our previous views was scarcely necessary, but our readers may, we think, safely accept the above as certain, and as a good introduction to the continuation of our inquiry.

In pursuance of the method proposed in the outset of these reflections, it now becomes necessary to take up some of the prominent symptoms of angina; to study their nature, and inquire how far

they may be probably or possibly dependent on the physical conditions to which we have referred; and thus we may hope to make the way clear for the other considerations of angina, and facilitate the further investigation of cardiac diseases generally.

The distress in various forms is doubtless the main as well as the mainly intricate character of angina; and where shall we pretend to locate this very protean incubus?

We shall hope to find advantage in making separate and successive considerations of pain of different kinds and in different situations; of the sense of suffocation; of the simple anxiety; of the dyspnœa; and lastly of palpitation; and although our remarks will not often appear definitive or positive, we conceive they must always be more or less available, and even indispensable, in explaining the signs to which they are applied.

First, of the pains of angina, in the heart and elsewhere. Our readers must not expect to find more than a general examination of the present difficulty. We shall endeavour to point out sources of error and right views to be taken of different cases, and shall hope that in the end a more extended view of all symptoms will serve to place even the pains in a clearer light, and render them often very satisfactory indications of internal states.

Inflammations of the heart, its lining or its outer covering, may go on violently without pain, and severely often, with little or no distress up to a certain amount, or rather perhaps up to a certain period, when it seems evident that graver changes still will at least assist to explain the uneasiness. The extension of inflammation to more sensitive parts exterior to the pericardium* would seem to be the first explanation of common pain in pericarditis. Material deteriorations of the substance of the ventricles generally will quickly give rise to some *distress*, and not less will be produced by the

sudden disorganizations of the left valves. We must however confess, considering all the severe affections of almost every kind which different parts of the heart undergo without the indications of pain, that we are rather unwilling to locate the cause of anginal pains in the heart itself. May it not be that the sudden fulness and severe action of the whole apparatus operates on more sensitive adjacent parts, as we have supposed the pain of pericarditis is only felt when the inflammation extends beyond the reflected serous membrane? Although we know next to nothing of the sensibility of the heart, and countless negative facts are on the side of its little sensibility, yet we shall admit for the present that the sudden and sharp agony may be in its proper substance at least in certain instances. Besides this, however, there is very much that still remains to become the subject of research and rational explanation.

We shall not pretend to have witnessed, and still less think of accounting for, all the varieties of uneasiness described by writers as attendant on different anginal attacks. The sufferings are manifestly of widely different kinds, though it is by no means clear that pain in the proper sense is always present; and it is much less certain that the pain is always in the same part and of the same kind.

Mr. Hodgson describes the case of an aged lady who was subject to severe dyspnœa and syncope, and pain at the heart. Six months after an hemiplegic attack, she had a sudden aggravation of the thoracic symptoms, and in a few hours she became collapsed, with a rupture of the left ventricle. There was no evidence of undue dilatation; the rupture presented softened edges (probably from a form of recent inflammation,) and the artery of this particular part seemed more obstructed by senile changes than the coronaries generally, which, however, were much diseased.

Now here, as in many cases, it may be questioned if the ancient pain were in the heart at all, for with dyspnœa and with the absence of distension in the heart (rather wasting) there seems to be the least reason to accuse that organ; indeed, it is not clear that in even the last attack the pain was in the part affected with rupture.

The affection is too complicated, and

* This often appears equally probable with respect to the pleuræ and peritoneum. It is most evident that pericarditis, pleuritis, and peritonitis occur in one case without and in another with pain. The constitutions producing, and the morbid products of these two events, seem to us to coincide with the opposite ideas of action confined to the serous membrane, and of inflammation which involves adjacent and more sensitive tissue.

the patient's perceptions of internal disorder are too uncertain, to warrant a positive assertion in this case, and in by far the great majority of instances.

We cannot omit to notice, that ordinary palpitations are free from pain, as is also a very considerable hydropericardion; which, however, as relates to pressure, certainly acts more equally in all directions, and probably is more gradually produced.

It is almost needless to state that chronic changes and settled disorganizations do not produce any truly painful sensations in the heart, generally or necessarily. Hypertrophy, dilatation, adhesions, disordered valves or ossifications, are found unattended with pain. These remarks may avail us in our further reasoning, as well as the recollection that asthma is often painless, though bronchitis is not equally so; being sometimes as it is said, sore, cutting, tight, &c. The pains which affect the walls of the chest are not to be lost sight of in our course.

We have carefully and amply noted, in the case of gradual decline, various and casual pains about the chest, which it was evident, from day to day, were owing to irregular efforts or constrained positions, as in sleeping. These truly muscular pains, which come and go, though sometimes especially affecting one part, as the pectoral muscle, are chiefly indications of weakness, but their importance, as distinct from the pain of pleuritis, which they much resemble, demands strict attention.

On the subject of dyspnœa we may return to these considerations, for it is particularly in connexion with the presence of constriction, or suffocation, or dyspnœa, that costal or sternal pains are to be examined. We may, however, here remark, that the pains here spoken of, and which are sometimes inconsiderately called neuralgic, are very closely analogous to the pain of pleuritis. Perhaps the two may be identical in kind. We have seen that pleuritis and various products occur without pain, as do also pulmonary disorganizations, but the extension of the inflammation external to the pleura costalis is closely enough allied to reparative reaction in the deeper muscular layers.

It is, we think, to be regretted, that hitherto sufficient distinction has not been made with respect to the different sources of the nerves of the heart, and

again to the different proportions in which the particular nerves are distributed.

The auricles and ventricles may be supposed to be supplied like the bowels for contraction. But the largest proportions of supply seem devoted to the *great* pulmonary artery and *ascending* aorta; so that whether for sense or motion, or mere nutrition, considering the thickness of their tubes, they would seem to possess more nerves than any other vessels (one set perhaps excepted—those of the head, with the *nervi molles*), and indeed than most other parts of the body.

If we were desirous too hastily to surmise, we might perhaps consider the pulmonary artery as the seat of much of the suffocative sense, and a little more probably suppose the aorta to be the seat of the severe pain which is sometimes referred to the upper part of the sternum. And if again we were to touch on final causes, we might suspect that the sensibility of the ascending aorta is destined to warn us of the approaching inundation of too great continued exertion, and that of the pulmonary artery, of the dangers of sudden efforts, and perhaps of the need of a freer respiration.

To appreciate the indications of pains adjacent to the heart will require much patience and precision.

Lieutaud connected pain in the left shoulder with rupture of the heart; Hildanus, with pericarditis; Riverius, with carditis. Another, with blood in the pericardium*. Sir Charles Bell seems decidedly to implicate the intercosto-humeral nerves in some anginal pains. The pains felt in the mamma, in such cases, will depend probably on the nerves from above the clavicle.

The pressure which the heart may suddenly produce on the phrenic nerve, especially on the left side, may be indicated by the pain over the deltoid through superficial branches of the third cervical nerve, &c.

The remoter pains, which seem all to occur with or without anginal paroxysms, we are disposed more confidently to refer to specific or local causes.

Those who are familiar with the descriptions of the pains which attend pretty constantly in kind, though very

* Med. Obser. vol. iv. p. 330. See a series of Engravings of the Nerves, by Charles Bell, 1816.

variously in degree, on internal aneurisms, will, we think, be most ready to concur in the opinion, that much of the pain of angina is most probably referable to the unusual arterial distensions. Some pains may have a very different cause, as for instance venous fulness, and while these may occur with a feeble heart, the aneurismal may only be expected under a more vigorous circulation. It is probably true in numbers of these cases, that, the less the pain, the nearer the heart is to failing irremediably, and vice versâ.

The portion of the first dorsal nerve which is closely apposed to the left subclavian artery, seems to supply just those parts of the arm which are most often affected in common angina. On the right side the same parts are not often affected, but a similar relative arrangement obtains.

The suboccipital nerve is similarly confined between the vertebral artery and the atlas, and indeed this vessel is in pretty close contact with all the cervical nerves, which, however, are very rarely affected in connexion with angina*.

The numbness or tingling which accompanies the affections of particular nerves, seems still more indicative of pressure on their trunks.

The swelling of the hand which is sometimes spoken of, if we may attach any import to it, seems to bespeak transitory pressure on, or obstruction of veins; if, indeed, it is anything more than an extreme effect of gravitation in the pendant arm, unassisted by the influence of motion on the venous valves. Walking long, and especially in warm weather, and in a state of repletion, will commonly produce much tumefaction of the pendant and inactive hands, but if the right or either hand be more employed, as with a walking-stick, it will become less swollen, or not at all so.

Now, should it be said that we have only confused the subject of anginal pains, we cannot but plead guilty; but we may safely lay all the blame with those who have so long called different cases by the same name, and our only extenuation must be, that we have invited the practitioner, with any kind

of anginal seizure under his observation, to notice, as he may at a glance, if the case be one of arterial distension or not, and then we predict that he will almost as soon discover whether the case be one of impeded or only of wide inspiratory effort;—whether the thoracic walls may be the seat of pain, or that there may be venous fulness. This is not all that will be made apparent; but our future comments will again bring us near to this topic.

The next consideration that presents itself in the symptomatology of angina relates to the uneasiness which patients refer more generally to the chest; but here again we must remark, that the general notions of the sufferer have been too little analysed by the medical observer, and we may again claim some attention for our statements if they should only serve justly to enforce discrimination.

It is essential to understand the differences between the severe and even terrible constriction of true angina, and the worst asthma; the oppressive suffocative sense that may occur in delicate or feeble health or advanced life, with or without palpitation (as in walking); the globus of young females; the depressed anxiety in the enfeebled; and lastly, the more lively, but not less impressive alarm of true angina.

We prefer to glance at these topics successively before approaching that of dyspnœa, which appertains only to the later or more complicated attacks of angina; but should the reader feel at a loss for definite notions of dyspnœa, it will be desirable for him to study this subject in the first place.

Writers appear clearly and forcibly to concur in the statement of cases in which, with the severest suffocative sensation, there is no dyspnœa—a fact which is probably of the first importance in thoracic pathology.

For the present we shall take constriction at the glottis to represent dyspnœa, and it will be at once evident that, as relates to the inspiratory effort, we have a very different case when we take the instance of sudden obstruction in the left heart; as, for example, in the aorta or in the left auricle; and again, it is no excessive refinement to distinguish two forms of the later case: first, that in which there is, from habit or predisposition, much tendency to pulmonary turgescence; and secondly, that in

* In the first Volume of the Guy's Hospital Reports, Dr. Bright has pointed out some probable relations between occipital pain and disease of the vertebral artery.

which there is a remarked absence of this feature.

Simple angina is a case in point. There is every reason to conclude that the pulmonary circulation is free, copious, and forcible, as that the respiration has been easy and exalted. The pulmonary vessels generally have as yet no disposition to unnatural yielding. In the early seizures we have mostly no dyspnœa, no catarrh, no cough, and no evidences of obstruction. These present themselves only very gradually in the course of later and repeated attacks, and they appear most analogous to the pulmonary deteriorations which are more rapidly produced in consequence of acute or other lesions in the left heart.

Dr. Hunter, in the sixth volume of the Medical Observations and Inquiries, communicated a case which seems to illustrate thoracic distress independent of dyspnœa. The orifice of the pulmonary artery would scarcely admit a probe; the ventricles communicated most freely, and opened together into the aorta. The boy died when thirteen years of age. His figure was remarkably slender and delicate—like a “greyhound,” or a “wading bird,” and he was very dark. He was subject to very various paroxysms of chest oppression, faintness, and increased liver, with even insensibility. Slight excitement brought on attacks. His remedy was to *lie down at once* on his left side, and remain motionless for about ten minutes; and this would always prevent or terminate the attack. Sobbing and yawning continued a little while afterwards, with a feeling of fatigue. This is one of a very various series of examples of distress without dyspnœa which are to be met with in books, and we shall have occasion to adduce others hereafter, as well as to shew that dyspnœa is often unconnected with suffocation, if not with actual constriction, *i. e.* not with a material sense of constriction.

The distinctions to be drawn between the feelings of suffocation and of constriction are less clear, but they may not be less important. Let us suppose the tightness to be connected with the inefficient or extreme effort of inspiration, and this is certainly the case at times, and let us imagine the suffocative feeling to be the excess of that hidden sense which excites inspiration,

(though this may be more than doubtful), and it is thus explained how the two symptoms may exist separately or together in various degrees and proportions.

The sense of suffocation must, we think, be considered as distinct alike from constriction and dyspnœa. Indeed it is pretty manifest that it is a peculiar internal sensation. In young women, in connexion with palpitation, or occurring suddenly in sleep, or rather in waking, it may be much more prominent than dyspnœa, while constriction is even absent. This suffocation is speedily remedied.

In its character it approaches to a feeling of fulness, and (speaking from a very imperfect personal experience), its slightest form is scarcely unpleasant. It is not, we think, widely separated from globus, and still less from an hysterical feeling, which many have doubtless experienced in moments of sudden joy, or the like.

This feeling is only an occasional attendant on dyspnœa and asthma, and palpitations; as on angina, and even very simple circumstances of sudden exertion.

It accompanies sometimes the fluttering palpitation, and we suspect it may even itself appear to flutter.

True globus may seem located in the œsophagus, or rather in the pharynx, but we are not sure that vascular fulness is unconnected with it. The suffocative sensation we are inclined to refer to severe or sudden accumulations about the right side of the heart. With respect to both, peculiar sensibility or unusual susceptibility alike in the constitution and the heart, seem necessary for their manifestations.

The anxiety depicted in the countenance, or the alarm expressed by the sufferer in angina—the fear of impending death, are doubtless, as signs, not without meaning, although at present extremely obscure.

In the case which we have before detailed at length, the gentleman is said to have possessed a strong mind, yet the idea of immediate dissolution seized him at times most forcibly. Now this kind of feeling does not belong to syncope or to pain, but it does, we think, belong sometimes to severe dyspnœa or asthma. It may be an effect of the constriction or of suffocation, or it may be thought to attend

depression, as in cases of poisoning, or the worse forms of peritonitis and injuries.

We have thought it needless to dwell on the subject of faintness. Pallor, cold perspirations and a failing pulse, when they occur in angina, should be too well understood to require much comment, as well as their opposites; but it may be well to remark that the observer can scarcely make too much use of all these signs in forming his judgment of different cases, and deciding on the principles of treatment.

Certain forms of syncope are more fearful than angina. Death may thus occur instantly in the first hours of returning health after severe diseases, as also after delivery, and even in extremely delicate persons, and we ought not to expect to find any quantity of blood in the heart on dissection. The sudden death of persons with settled heart diseases or original malformation, however it may seem allied to angina, is still the case of an enfeebled heart, and the fatal attack in an old case of angina is the same; but we must attend continually to the fact, that in every case the proportions of the fullness and the power of resistance must vary.

[To be continued.]

ON THE USE AND ABUSE OF BROWN BREAD.

To the Editor of the Medical Gazette.

SIR,

SHOULD the following remarks be thought by you applicable to the present day, perhaps you will give them a place in an early number of your GAZETTE.

I am, sir,

Your obedient servant,
W. P. BRODRIBB.

12, Bloomsbury Square.

There are no remedies, however valuable in themselves, which are not attended with mischievous effects when indiscriminately or excessively used. And this is the more likely to be the case with such as become popular or fashionable; for it is then that they are sure to be abused. I believe that a very little observation will show this remark to be strictly applicable to the employment of brown bread as an article of diet. For a long time past

it has been known, and deservedly praised, as a substitute for medicine, to obviate that constipated state of the bowels which so many persons have to lament. As such it was known to the ancients, and we read in Celsus, "at alvum movet, panis fermentatus, magisque si cibarius vel hordeaceus est," &c. But it was probably reserved for the present day to witness such a general and unwise use of it, as to render it questionable whether it is not more frequently productive of evil than of good. The only difference between brown bread and white is, that the former contains a portion of bran, which is merely the cuticle or covering of the wheat, and which, from its firm and insoluble nature, remains undigested. By thus augmenting the bulk of the refuse of the food, it acts mechanically as a stimulus to the bowels, and in this manner excites them to action, and not by any aperient quality residing in the bran itself. It may be macerated in boiling water, or even boiled, for several hours, but the strained liquor will not be found to possess the slightest aperient property. That the use of brown bread frequently has the effect of ensuring a comfortable and natural daily evacuation, and with that, all its happy results, may be fully admitted. But the great evil of its general use as an article of diet is, that if it fail to do good, it is sure to do harm. There is a favourite saying with many persons, of things which are considered simple and innocent—namely, that "they can do no harm, and may do good." No mistake, however, can be greater than this in the present instance; for the reverse will follow; and I should say that it must do harm if it fail to do good. It is well known that any insoluble substance, by remaining in the alimentary canal, is likely to become blended with its mucus, and so give rise to intestinal concretions. Magnesia, chalk, the sesquioxide of iron, Ward's paste, or the Confect. Piper. Comp., all of these, when taken in large quantities, or even in small quantities and continued for a long time, have produced the same results; namely, the formation of intestinal concretions. Horses that are fed much on bran frequently have them; so also have cows from licking their hairs, which they swallow, and which afterwards, by getting mixed with the

mucus in the stomach, are formed into hard balls. I have seen such nearly as large as the double fist, the soft hairs giving the outside of them a velvety or woollen appearance. Many physicians and surgeons are so aware of these facts, that they never permit their patients to persevere for a long time together in the use of such medicines as I have named, without prescribing an occasional active aperient to ensure their being carried down by, and discharged from, the bowels. Any insoluble substance taken daily, either in the shape of food, or as medicine, may give rise to these hardened masses. Such concretions once formed in any part of the canal, it is easy to foresee the consequences. Increased constipation, colic-like pains, sickness, with many of the attendants of indigestion, are sure to follow, varying in degree according to the nature and circumstances of the individual case. A lady whom I attend once narrowly escaped imminent danger from this source. As a consequence of a general want of power throughout the system, the uterine function had been suspended for very many months; the circulation was so feeble and languid as to cause extreme parts of the body to be purple or blue; the hands and feet being so cold as to be swollen and covered with chilblains, even in weather by no means severe. To this was added an habitually torpid state of the bowels. For this last symptom she was advised to use brown bread. At first no inconvenience was found, but in a short time the constipation was increased, bringing with it many additional sufferings. In this state one morning her family were alarmed by her shrieks from the closet, where she was enduring the greatest pain in her fruitless efforts to get rid of an enormous collection of hardened fæces. The rectum was choked up and distended with the accumulated mass of bran and fæculent matter, and it was only by the aid of others that she could be freed from the cause of her suffering. It may be asked, does such a result frequently follow the employment of brown bread for food; and ought it never to be taken, or if taken, under what circumstances? My reply to such questions is, I believe that it does frequently lead to such a result, and always may, if persons are inattentive to its effects. However, in a large proportion of per-

sons who take it, it answers the object in view, and may then be safely persevered in, yet with many individuals it fails of its purpose; and in all such cases its further use should at once be relinquished, and an aperient immediately taken to get rid of any that may possibly remain behind. In many who take it, the bowels are only scantily and imperfectly acted on, occasioning scybala to a greater or less extent, followed by further intestinal disorder, and paving the way for dyspepsia, with its numerous evils. Such persons may feel assured that brown bread is not adapted for them, and should seek a remedy for their complaint in other and less objectionable measures.

PUERPERAL CONVULSIONS.

ARTIFICIAL DELIVERY OF TWINS.

To the Editor of the Medical Gazette.

SIR,

IF the following case of puerperal convulsions be considered worthy of introduction into the pages of your useful journal, you will by its insertion oblige

Your obedient servant,

EDWARD AUGUSTUS CORY, M.D.

Cannon Street Road,
June 1841.

A German woman, named Zimmerman, of a leucophlegmatic constitution, during the process of her second parturition, was attacked with convulsions early in the morning of the 8th of June. She had been under the care of her midwife for some hours previously, and had complained during the preceding day of intense headache, for the relief of which her friends had very improperly recommended her copious potations of gin and water. On the supervision of the convulsive attacks, it was deemed expedient to procure the assistance of a medical practitioner, and Mr. Beale, surgeon, of Bedford Square East, was accordingly sent for, who immediately and very properly bled her to a considerable extent, and had also recourse to the usual secondary remedial agents. The os uteri at that time shewed no signs of sufficient dilatation to permit the artificial evacuation of the uterine contents, and she almost immediately sunk into a state of complete coma. Mr. Farrer, surgeon, of

the Commercial Road, had also very kindly lent his assistance. I was requested by the above gentlemen to see her about eight o'clock on the same morning. I found her in a state of complete insensibility, with no interval of consciousness. The breathing was stertorous, and the pulse so feeble, as entirely to preclude any further depletion, which appeared to have been carried to its fullest extent. The os uteri was at this time dilated sufficiently to admit the careful application of the forceps, and I accomplished the delivery of the infant without difficulty. Another child was now detected in the uterus under head-presentation, which I immediately delivered by the operation of turning. The uterus showed no disposition to contract after the expulsion of the children, but by the employment of compression externally, it contracted tolerably, and one large placenta was expelled. There was no hæmorrhage: both children were still born. The woman died in about an hour after delivery.

Sectio cadaveris, 24 hours after death.

—The dissection of the body was performed by Mr. Beale, assisted by Mr. Charles Bell, one of my pupils. On the removal of the dura mater, considerable vascular turgescence was observable on the surface of the left cerebral hemisphere, and some patches of imperfectly formed lymph were also evident. The tunica arachnoides presented a remarkable degree of dryness. On the superior portion of the anterior lobe, some extravasated blood was discovered, and on extending the dissection, the left ventricle was found to be completely filled with a coagulum. The effused blood, when collected, weighed altogether about three ounces. The right hemisphere partook but slightly of the increased vascularity which had been observed on the opposite side, and was comparatively healthy throughout its whole structure. Nothing further of interest was noticed in the dissection of the brain. The intestines were much distended with foetid gas. The uterus and its appendages were in a healthy condition, and presented the appearances usually observed in a woman recently delivered.

REMARKS.—The above is a case of the apoplectic form of puerperal con-

vulsions, which may be considered the most fatal of all the varieties of that formidable and frightful affection. I think it may very reasonably be assumed, that if the patient, in the first instance, had been under the care of a medical practitioner, instead of a midwife, her life might have been saved—I mean, had she been copiously bled and purged on the accession of the intense headache, of which, it will be recollected, she complained during the preceding day, instead of having been plied with ardent spirits by her ignorant friends, the more formidable stage of the disease might have been effectually prevented. I do not intend to trespass on your valuable columns, by detailing the symptoms and pathology of the disease under consideration, for they can be fully comprehended by a reference to any of the standard works on obstetric medicine; I shall merely observe that some writers, among whom may be mentioned Baudelocque and others, have described several varieties of puerperal convulsion. I am, however, of opinion that the division of Dewees into hysterical, epileptic, and apoplectic, is pathologically correct, and sufficient for ordinary practical purposes. It appears that primiparous women, and those having more than one child in utero, are the most liable to puerperal convulsions, and that these attacks are more common and dangerous during parturition than at any period of uterogestation or after delivery. Mauriceau had 42 cases of the above disease, of which 7 occurred during pregnancy, 3 of which proved fatal; 19 during labour, of which 11 died; 16 after delivery, of which 5 died. Merriman cites 48 cases: 6 occurred after delivery; 3 during labour with twins, of which one died. The rest were attacked during labour, of which 11 were delivered by the forceps; 9 by cephalotomy, of which 2 died; 4 by version, of which 2 died; 1 died undelivered; 14 were delivered by the natural efforts, of which 5 died. Of these, 36 were primiparous. At the Maternité of Paris, under the surveillance of Madame Lachapelle, in 15,652 women delivered there, 40 were the subjects of puerperal convulsions—12 of these were delivered by the forceps; 5 by version. 23 of these cases occurred before delivery, of which 9 died. M. Pacoud, at the

Maternité of Bourg, in 11,208 women, had 47 cases of this disease—18 of which occurred during pregnancy, 20 during labour, and 9 after delivery. The number of deaths is not stated in the report. M. Desjardins relates 7 cases, 5 of which happened during labour, and 2 after delivery, all of which recovered. M. Champion had 10 cases, all of which were primiparous; 7 recovered, 3 died; 5 of the children were born alive. Velpeau gives us an account of 21 cases—7 took place during pregnancy, of which 2 died; 5 during labour, of which 2 died; and 9 after delivery, of which 4 died. Collins records 19 cases, which occurred in the practice of Dr. Joseph Clark, of Dublin, of which 16 were first births. He also mentions 30 cases of his own, of which 29 were primiparous. Dr. Ramsbotham, senior, mentions 22 cases, of which 15 were first births. Of 59 cases attended by Dr. Ramsbotham, junior, 17 occurred before the commencement of labour, 28 during the process, and 14 after parturition. There were 3 cases of twins; 45 were first births; 13 of the women died. Of the children, 41 were expelled naturally by the head, 6 delivered by craniotomy, 6 by the forceps, 5 by turning, and 4 presented the breech; 23 of these only were born alive. The convulsions took place after delivery in 12 of these cases. One patient was attacked nine days after labour, another ten, and another seven.

The treatment of puerperal convulsions can be comprised in a few words. Bleeding, not to ounces, but to pounds, according to the state of the patient, and delivery as soon as it can be safely accomplished. The immense quantity of blood which may be taken in this disease, with the most beneficial results, is truly astonishing. Active purgation, refrigeration of the head, counter-irritation, &c. may be regarded as useful, although of secondary importance. Opium is decidedly injurious in whatever form it may be administered, unless in the hysterical variety of the affection, where I should consider it of doubtful efficacy. Some observations appeared in a late number of the *Lancet*, from the pen of my able and ingenious friend, Dr. Maddock, of Judd Street, Brunswick Square, in reference to a case of puerperal convulsions, which occurred in the practice of a pro-

vincial practitioner, in which he (Dr M.) strenuously recommended the use of opiate injections. Experience has taught me the utter uselessness, nay danger, of all the preparations of opium in puerperal convulsions; and if any practitioner should be sceptical on this point, I recommend him to peruse the interesting cases detailed by Dewees, and other eminent authorities, in relation to this subject.

The treatment of puerperal convulsions has been so graphically and impressively delineated by the late Dr. Gooch, that I cannot refrain on the present occasion from quoting his own words. "The remedies (says he) commonly recommended are antispasmodics, bleeding, and delivery. The first, general experience shows to be useless. Bleeding is then our sheet-anchor. Dr. Hamilton says, take away forty ounces at once, and if in two hours the patient is not satisfactorily better, take away forty ounces more. When I first heard Dr. Hamilton, in his lectures, deliver these instructions, I felt not a little astonished, but I can now conscientiously declare, that I have never had a patient die of puerperal convulsions, where the disease had been thus boldly treated; those who have died have been bled with a sparing hand, and to an insufficient amount. A little woman, about 18 years of age, of a spare habit, was seized with pain in her head and trembling, on which she fell down senseless. I was sent for, and soon after my arrival she became convulsed. This was the first case of the kind I had ever seen; and though the patient was not of a plethoric habit, I bled her to the amount of twenty ounces: before the bleeding was stopped, she opened her eyes and the convulsions ceased. I ordered her head to be shaved, directed cold applications to the scalp, and prescribed some brisk aperient medicine. Notwithstanding the favourable impression produced by the bleeding, which was followed by the action of the purgative, in a short time the convulsions returned: the bandage slipped off, and she lost about eight ounces of blood. The husband tied up her arm, and in great haste ran for me without his hat, and with his hands covered with blood. I went immediately, and took away about twenty ounces of blood more, and the

convulsions ceased, but still the patient remained insensible. At ten o'clock at night I went to see her again, and just before my arrival she had a convulsive fit more violent than any preceding one. She had since nine in the morning lost forty-eight ounces of blood, and I now again bled her to the amount of thirty ounces: the convulsions ceased: in the morning she was decidedly better: in the course of the day uterine pains came on; she was delivered of a dead child, and gradually recovered. Give me the lancet, and deprive me of all other remedies, and I will do more good with it singly than with all others, deprived of this, put together."

ON THE
PATHOLOGY OF THE NERVOUS
SYSTEM.

To the Editor of the Medical Gazette.

SIR,

IF you should consider the following remarks relating to the pathology of the nervous system interesting, I shall feel obliged by their insertion.

I am, sir,

Your obedient servant,

SAMUEL SOLLY, F.R.S.

Lecturer on Surgery, and Assistant Surgeon
to St. Thomas's Hospital.

June 19, 1841.

Some years ago, in my treatise on the Human Brain, I adverted to the fact that reports were frequently given of cases which during life had exhibited symptoms of disease of the cerebro-spinal axis, without any corresponding lesion being discovered after death. I added that I thought the circumstance arose, not from the real absence of morbid changes cognisable to our senses, but from want of sufficient attention to *minute* alterations of structure, either in the bones of the skull, the membranes, or the brain.

Still more extended observations have confirmed the opinion, that there is a post-mortem appearance corresponding to the symptoms of disease exhibited during life. I am also convinced that in the colour of the cineritious neurine of the hemispherical ganglia, and the state of the membranes in contact with those ganglia after death, will be found some indication of the state of the intellectual faculties previous

to its occurrence. Paleness or absence of colour, indicating atrophy of these important instruments of the mental operations, is to be found in old standing cases of mental imbecility unaccompanied with maniacal excitement; while, on the other hand, a deep purple mottled appearance, indicating increased vascular action of the ganglia, is to be found in cases of mania, where the intellectual powers have been worked up to their highest pitch: the increased quantity of blood acting, as in the first stage of intoxication, so as to produce an exaltation of the function of the part, though the subsequent effect is, that the instrument of mind is broken by an over-degree of exertion. The following case ought to encourage the pathologist never to weary in his search for the cause of disordered function:—

Edward Brown, ætat. 42, working jeweller, intemperate habits, both in regard to intoxicating liquors and venereal excitement, but more especially the latter, applied to me for the relief of pain in the nape of the neck, extending partly up over the head. From the character of the pain, and other circumstances connected with the case, I conceived that there was a slow inflammatory action going on in the dura mater and membranes surrounding the cerebellum. By the means of local depletion, and the free use of the vinum Colchici, I entirely removed the pains for a short time; but they soon returned, when finding that he had not abandoned his dissolute habits, I declined giving him any further advice.

In January 1841, he was admitted into St. Thomas's Hospital with symptoms of disease of the lungs and heart, and on the sixth day after his admission he had a convulsive fit, which lasted about a quarter of an hour, after which he remained tranquil for about an hour, and then suddenly died.

In our post-mortem examination we readily found thoracic disease sufficient to account for the thoracic symptoms; and the immediate cause of death was equally evident from the presence of effused blood in the motor tract as it passes through the pons varolii; but there appeared to be nothing to account for his venereal tendency, and the morbid pain in the region of the cerebellum, for this organ presented no morbid appearance, nor did the membranes

covering it, as examined *in situ*. But feeling satisfied that a cause for such an effect must have existed, I carefully stripped off the dura mater from the occipital bone, and then discovered a thin layer of very firm lymph, clearly the effect of old-standing inflammation.

Through the kindness of a physician, whose knowledge of the pathology and medical treatment of insanity is only equalled by the humanity and determination with which he is carrying out his plan of soothing instead of irritating by restraint the poor creatures entrusted to his care (I need not say that I allude to Dr. Conolly, of Hanwell), I have had the opportunity of observing many post-mortem appearances following mental derangement, from which I shall select two in illustration of my views.

The first case was one of melancholia of some years' standing, and the immediate cause of death was fever. At first sight neither the brain nor the membranes exhibited any morbid appearance, and the absence of that serous effusion and opacity of the arachnoid, which is almost always met with in the insane, arrested our attention; and the fact was peculiarly interesting to me, as there had been no exhibition during life of maniacal excitement, which is, I believe, the invariable effect of acute inflammation of the hemispherical ganglion, usually designated inflammation of the membranes of the brain; the fact being overlooked that the symptoms of inflammation of the membranes, such as mania, &c. result, not from the inflammation of a serous or a vascular membrane, no such effect being produced by inflammation of a similar membrane in other regions of the body, but from the effect of the inflammatory action on that portion of the brain which is in contact with them.

The examination of the brain not having afforded a satisfactory explanation of the cause of the melancholia, we directed our attention to the cranium, and its appearance in my opinion was quite sufficient to account for the mental depression. It was enormously thickened, especially in the frontal region: the greatest thickness was nearly half an inch, as shewn by perforating it with a gimblet, and all the arterial fossæ were deepened.

In this case I consider that the disease must have commenced with sub-acute inflammation of the dura mater,

producing, as in ordinary cases of chronic periostitis, thickening of bone, which from its slow progress gradually altered the texture of the hemispherical ganglia, and thus smothered the intellectual faculties without exciting them. The cineritious neurine of the anterior convolutions was of rather a darker colour than natural, and much darker than the posterior, which colour, as it must have arisen from some increase in the quantity of blood flowing through the vessels of this part during life, seemed at first at variance with the previous mental imbecility; but this is perfectly accounted for by the fact that during the febrile excitement preceding her death she answered questions distinctly, and protruded her tongue when desired, not having exhibited so much intelligence for a long period before: inasmuch as the improvement in the intellectual faculties was occasioned by the increased vascular action consequent upon the fever. The following case supports this view; for here the symptoms during life were very similar, and were not broken in upon by any improvement immediately preceding death:—

John Buckingham, æt. 48; insane 10 years; mental imbecility supervening on anxiety.

Death from ascites and hydrothorax. Post-mortem 39 hours.—*Head*: Skull generally thick at the anterior part; arterial fossæ numerous, and rather deep; thickness in the centre of the frontal bone, 4 French lines, (8-20ths of an English inch); dura mater strongly adherent to the skull.—*Brain*: Cortical substance or hemispherical ganglion pale; softening of that portion of longitudinal commissure which is called the septum lucidum.—*Thorax*: Hydrothorax; atrophy of the right ventricle of the heart.—*Abdomen*: Ascites; chronic disease of liver.

The last case is one of mania, shewing the effect of inflammatory action of the hemispherical ganglion.

Joseph Smith, insane four months. Alleged causes—intemperance, abuse of mercury, and venereal excitement. Fifty hours before death he became comatose, and gradually sunk.

Post-mortem: *Head*.—Considerable effusion of blood and serum on the surface of the brain; the pia mater greatly congested; the hemispherical ganglion exhibited a deep-pink colour, mottled with red spots.

Without lengthening this communication further by the detail of more cases, I will merely add that I have not unfrequently found small rough ossific deposits on the internal surface of the dura mater by passing the finger carefully over the whole, when they had previously escaped the eye; also a lengthening of the clinoid processes into complete spines, which perforated the substance of the brain.

REMARKS UPON
THE TREATMENT OF UTERINE
HÆMORRHAGE;

WITH A RECOMMENDATION FOR THE USE
OF A TOURNIQUET IN SUCH CASES.

To the Editor of the Medical Gazette.

SIR,

IF the accompanying remarks and observations are considered worthy of a place in your journal, I shall feel obliged by their insertion.—I am, sir,

Your obedient servant,

WM. PRETTY, M.R.C.S.

3, Camden Road Villas,
June 17, 1841.

In casual conversation with the late Mr. Walford, teacher of midwifery in Aldersgate Street School, upon the treatment of uterine hæmorrhage after delivery, he suggested to me the application of pressure by means of the tourniquet as certain of success; and such was his confidence in this means, that he emphatically declared that no woman ought to die of uterine hæmorrhage; and that any practitioner of midwifery losing a woman from this cause, ought to suffer the punishment due to the crime of manslaughter.

From having had some experience in these frightful cases, and more especially from its occurrence twice in my own domestic circle, each time producing all but fatal syncope, notwithstanding every care was taken to guard against such a lamentable result, by the attendance of a kind and talented physician-accoucheur; and knowing the great difficulty of successfully treating and managing such cases, they have been a subject particularly interesting to me. It is in cases of flooding after delivery that I have found the use of the tourniquet so highly satisfactory, and that I strongly recommend its employ-

ment to all accoucheurs. I know not how far the members of the profession generally are acquainted with the use of this apparatus for such a purpose, but finding those with whom I have conversed ignorant of its great value as a means of saving life, I have thought that my humble testimony to its merits might not be without its utility.

To witness the death of a woman from uterine hæmorrhage after delivery is an appalling sight, and a sad calamity, which it becomes every medical man to do all in his power to avert. I think also that we ought to aim at something more than the preservation of life; I mean, if possible, to prevent those many and great evils which generally follow perilous cases of flooding—such as protracted debility, incapability to nurse the offspring, and its consequent suffering from the want of natural nourishment; causing wet-nurses to be sought for, who cannot always be found or easily paid for, and are often very troublesome besides: there is here also a large amount of domestic anxiety and trouble only fully known to those who have experienced it. I believe that the tourniquet will not only arrest the violent and large discharges of blood from the uterus, but will likewise prevent that slow draining away of it, which, without producing syncope, is oftentimes the cause of great exhaustion and a long convalescence. Its use will also diminish the amount of after-pains, as exemplified in a case to be related, of a woman confined of her seventh child. It will likewise relieve the practitioner of much bodily exertion, and materially abridge the period of watchfulness; for feeling assured that his patient is safe, all painful anxiety is removed from his mind.

Many are the means employed to prevent or restrain uterine hæmorrhage; and pressure has, I believe, been justly most valued by the accoucheur. Cold applications, in a variety of ways, have proved serviceable; but here I beg to remark, that I have known the use of the lower limbs taken away for several months by too long a continuance of cold cloths, yet not longer than the appearance of discharge seemed to warrant. The period of time during which cold cloths were applied was in one instance half an hour, in the other somewhat longer, yet I have known

this period considerably exceeded, without the same bad consequences arising. Perhaps the temperature of the weather, and the peculiar constitution of the patients, might have influenced the effects; but to the impression of cold chiefly, after enduring for many hours severe labour pains, do I attribute the weakness which was experienced by these patients in their lower limbs. One was unable to stand alone for the space of three months.

The ergot of rye is a medicine of considerable utility in producing uterine contraction; but it must be given before fainting comes on, to prove serviceable. I have often administered it immediately after the birth of the child, in anticipation of flooding, where past experience has given me too much reason to expect a return: for flooding is habitual to some constitutions, — generally the weak and irritable. With scarcely an exception I have found the ergot of rye effectual in promoting uterine action. I generally administer the tincture, which has this to recommend it—it is very easily taken in any thin fluid, and is not readily injured by keeping. The emptying of the vagina and uterus of coagula is sometimes necessary, but without pressure from a bandage, which I always afterwards apply, it would not prove satisfactory. The necessity for the removal of coagula, and the redistension of the uterus, which now and then occurs, may, I think, both be prevented by the timely use of a tourniquet. The introduction of one hand within the uterus, and the application of the other without, to compress the bleeding vessels and stay hæmorrhage, has always appeared to me to be attended with much uncertainty, and some risk. Plugging the vagina is not to be depended upon, and the transfusion of human blood has not now many advocates. As immediate relief must be given, the administration of medicines, whether acids, sugar of lead, or any other drug, are of doubtful efficacy. I once saw a patient to whom a full dose of opium had been given prior to my assistance being requested by the gentleman in attendance. It was the worst case for the duration of alarming symptoms I ever witnessed, in which recovery took place. The long-continued state of exhaustion, and the frightful prostration of power, I attribute, in a very great degree, to the sedative effects

of laudanum. I have often given laudanum, to allay irregular and ineffectual uterine action early in labour, with beneficial results, but I have never seen it produce uterine contraction in cases of flooding. To surround the abdomen of every recently delivered woman, with a bandage capable of giving some temporary support, is in general practice, and much to be recommended, but the bandage or napkin ordinarily provided for this purpose is far from being efficient in cases of flooding. It is very easily displaced by a slight change of posture, however carefully applied, and although I have always in addition put a compress of some kind or other over the uterus, generally a pin-cushion or a small firm book enclosed in a napkin, pressure made by the hands is absolutely necessary in dangerous cases, and that to a degree painful and tiresome, before you can abate your watchful attentions and services. A bandage wide and deep, made of strong calico, with tapes attached to each side at different distances, so as to be made capable of affording support to a woman either before or after delivery, was strongly recommended about twelve or fourteen years since, by I believe Mr. Gaitskill, of Rotherhithe. This I have found very useful, previously placing the compress before described over the uterus. But this comes far short of the good and efficient pressure so readily to be obtained by the application of the tourniquet. This instrument is not easily displaced, as the band is passed beneath the back and over the ilia, and by turning the screw placed with the compress over the uterus, pressure is effected directly downwards upon it, and such a compression of the bleeding vessels or sinuses takes place, that uterine hæmorrhage must be restrained in almost every case: indeed, as Mr. Walford said, it might be carried to such an extent as to suspend the circulation in the abdominal aorta. In thin persons no doubt it might effect this object; and if so, few women ought to die of flooding after delivery. I recollect, when a pupil, having been sent for the late Dr. Batty, to assist in a case of flooding, and he being unable immediately to attend, directed me to inform the gentleman in attendance that if it continued he was to place the whole weight of his body upon the

patient's abdomen, by sitting astride her till he came; but the patient did well without this extraordinary help.

Mr. Walford recommended the tourniquet to be made at least double the size of the one in ordinary use, with a proportionably wide band, and this must have obvious advantages over a smaller one. The one I have hitherto used has been taken from an amputating case. I removed the pad, and by increasing the length of the fellet sufficiently to encircle the hips, with this and the compress I have obtained a power far exceeding any hitherto obtained by other means. I am disposed to think the most eligible compress might be made of a piece of cork, about an inch in thickness, covered with soft leather, and shaped somewhat to the inferior and anterior aspect of the abdomen; this would be sufficiently firm to yield uniform pressure over the uterus without giving any pain, which I have found the corners of a book apt to do: or something softer might be applied first upon the body if thought desirable. Twice I have tried the tourniquet on the same patient in flooding labours, with satisfactory results. The first time I attended this patient, which was of her fourth child, she flooded dreadfully, and so perilous was her condition that I did not think it possible she could recover. The debility, as usual in such a case, was long continued, and so debilitating to the poor mother, that to nurse the child was out of the question. When called upon to assist at her next confinement, I was prepared with a tourniquet, and *tr. secale cornut.* She began to lose blood very freely after the birth of the child, the placenta being detached and lying in the vagina. I gave immediately a dose of the *secale cornutum*, and applied the tourniquet, shortly after which I gently removed the placenta, and all further hæmorrhage was quickly restrained by increasing the pressure; fainting did not ensue, and I was relieved of all that bodily exertion which had been so fatiguing to me upon the former occasion.

At her subsequent labour I had to encounter the same danger of flooding, which commenced as before, and which, by the use of the tourniquet and compress *alone*, was most satisfactorily suppressed. This labour was of twelve hours' duration, and the latter pains

were very severe: its progress was regular, the head of the child gradually advanced, and after its extrusion, several pains were required to bring the shoulders, body, and hips, into the world. The placenta was found loose in the vagina. All this was very favourable for the non-supervention of flooding, but it did appear; and at this time I removed the placenta, which gave a momentary facility to the free discharge of blood, before I turned the screw of the tourniquet, and quickly stopped all further flow. The patient has made a much better nurse, and has been altogether stronger and better after this confinement than any former one, except her first, which occurred in the country about twelve years since. Sleep soon followed the delivery, and I left my patient with a soft compress and the tourniquet upon the body for two hours, when I returned and saw her placed comfortably in bed. She had not one bad symptom, nor yet an after pain, neither did I give one drop of laudanum.

This excellent recovery is further remarkable from the circumstance that she was seized with a very severe attack of influenza a month only before labour commenced, and when daily expecting to be confined; this seizure was attended with such an acute pain in the right side of the abdomen, upon coughing, that she was obliged herself to press a book upon the part to make it endurable. Opiates, blue pill, and Dover's powder, salines with antimony, were administered with only partial relief; and after suffering a few days, the fever being considerable, and the coughing and pain very distressing, I felt myself compelled to bleed her, which was of great service, and by continuing the antimony, in increasing doses, and the sedative at night, she was in a few days more convalescent, and gradually recruited her usual degree of strength before her accouchment. Her pregnant state, her daily expecting to be confined—indeed, she thought her labour had commenced when she sent for me, when suffering such acute pain in the side, and knowing the disposition to flood after delivery—made me most anxious to avoid taking away blood if possible. The cause of flooding in these cases is considered to be a torpor of the uterus, an exhaustion of its energy, and the consequent inefficient

contraction of its fibres, and imperfect closure of the mouths of vessels where the placenta was attached: the immediate object of the accoucheur is to put a stop to the flow of blood, and this I have succeeded in doing by the use of the tourniquet and compress; and if contraction of the uterine fibres be not at the same time induced, it must soon follow, for in proportion as the patient recovers from that state of exhaustion, be it more or less, which usually succeeds to delivery, the uterus will acquire contractile power. Time and safety are gained, and relief from much bodily exertion and anxiety of mind, though not of all watchfulness. A cool room, and a quiet state of mind, if possible to be induced, will contribute to the patient's welfare, and the attendant may administer any medicine he approves, according to circumstances, or give none if he prefers.

I hope that I have not overrated the good effects of the application of the tourniquet and compress in cases of flooding after delivery, and sure I am, that if only a portion of the evils attendant upon and following these cases can be in future prevented, a great good will be obtained for many parents and infants.

I feel some desire to state, that early in my professional practice I met with rather an extraordinary occurrence, viz. the expulsion during the last pains of labour of the whole of the uterus entire—the child, placenta, and membranes, unbroken upon the bed. At first I was a little astonished, but the momentary surprise yielded to my doing something towards saving the life of the child; I therefore immediately ruptured the membranes, discharged the liquor amnii, and separated the infant, which soon began to cry, and both it and the mother did well. I had been in the house only a very short time before I was summoned to give this assistance. The labour was quick, and took place at the full period of utero-gestation. The mother had borne one child only before, which was then twelve years old.

In the month of October last year, I had the pleasure to see the very good effects of a new midwifery instrument (the whalebone vectis), in the hands of Dr. Conquest, in a case of protracted

labour under the care of a lamented friend. A woman, ætat. 35, strong and healthy, in labour of her first child, went on favourably for some hours; the head presented, and the os uteri became fully dilated; but notwithstanding the pains were frequent and forcible for several succeeding hours, the labour made no progress; and as various means had been tried, such as bleeding, secale cornutum, purgatives, and salines, and I think a dose of laudanum, without any increased probability of it being soon brought to a termination, it was decided in consultation that the patient should be delivered by the aid of instruments. The os uteri, I have stated, was fully dilated, the ear could be felt above the brim of the pelvis and to one side of the symphysis pubis; the fore-head was the presenting part, with the face towards the pubes; not much of the head had entered the superior aperture of the pelvis, and in the absence of pains the finger could readily be passed round it; and the vagina was so dilatable that a small hand was easily passed within it. Dr. Conquest considered the situation of the head too high up for the safe or favourable employment of the forceps, and that in all probability we should be obliged to resort to the operation of cephalotomia.

Dr. Conquest exhibited to us the whalebone vectis, which he said was the invention of a gentleman whose name I do not remember, but the maker's name is Maw, of Aldersgate Street. He strongly recommended it as a safe and useful instrument, capable of affording all the aid to be obtained by the forceps without increasing the danger. It is formed of a thin loop of whalebone affixed to a handle. This instrument was passed over the occiput with the greatest facility by Dr. Conquest, and traction was made by him and myself alternately during the presence of pains. After a few efforts the head descended a little; by persevering in the traction it was brought without the os externum, in half an hour from the time instrumental assistance commenced. Dr. Conquest had some reason to expect that, as the occiput was brought down, the face might ascend, but this did not happen: the instrument being applied to that part of the occiput adjoining the vertebræ, traction brought down the head as it presented,

face and forehead first. The child was dead, as was expected, no motion having been perceptible for many hours, and the patient had been thirty in labour. The placenta was adherent to the uterus, and required manual assistance for its removal. The recovery of the patient was perfect and uninterrupted. I was much pleased to see the delivery effected so easily and by such simple means, and I cannot do otherwise than join Dr. Conquest in recommending this very valuable instrument to those of the profession who may not have seen or heard of it.

ON THE
PATHOLOGY, PHYSICAL SIGNS, AND
TREATMENT, OF VALVULAR DIS-
EASES OF THE HEART,
IN CONNECTION WITH THE SOUNDS OF
THIS ORGAN.

BY T. H. MOORE, M.R.C.S.

Late Secretary to the Dublin Medico-Chirurgical
Society.

[*For the Medical Gazette.*]

THE following case, which has lately come within the range of my observation, I have been induced to give, not merely from the intrinsic value it possesses as being linked with the others, by the symptoms, physical signs, and morbid appearances, but also on account of other particulars, which cannot fail to sanction its introduction here.

A woman, advanced in years, the mother of four children, was brought into hospital on the morning of the 16th May, with both legs, feet, and thighs, immensely swollen, presenting a florid red, glossy, erysipelatous hue, nearly as high up as the knees; pitting on pressure; of such weight that it was with pain and much difficulty they could be moved. There was an expression of great distress and anxiety in her countenance; lividity of the lips and cheeks; a tinge of bile on the conjunctiva; turgescence of the jugular veins, with distinct undulatory or pulsatory motions, very remarkable for the regularity of their succession in those of the right side of neck. No degree of uniformity between them, the cardiac impulses, and the arterial pulsations, could be ascertained with precision, or in a satisfactory manner; their persist-

ence, whether in erect or recumbent posture, was noted; with a diminution in their strength, and almost total disappearance on the left side, in the former position. Her breathing is laboured to an extreme degree, accompanied with extraordinary action of the thoracic muscles; rattling in the trachea and thorax, sensible to the ear and touch. She sits propped up in bed, and obtains but short interrupted intervals of sleep; has lost her appetite; passes but a small quantity of urine, and is irregular in her bowels.

Physical signs of chest and heart.—Percussion over and beyond the limits of cardiac region elicits a very dull sound, extending beyond sternum to the right side; the heart's impulses can be felt in cardiac region, not violent, nor forcible; on the contrary, weak, but tolerably extensive, the shock being communicated to the touch over a less circumscribed space than that observed in health. A strong vibratory thrill is imparted to the palm of the hand, proceeding from the vibration of air and mucus in the bronchial tubes, intermixed with that resulting from the blood's passage through the cavities of the heart. During the act of suspending the respiration, the sounds of this organ, previously inaudible, and altogether obscured by the respiratory phenomena, became so far distinct, that their action was reported to be extensively heard. Beneath the mammæ, and within the precincts of the præcordial region, the first and second sounds, the entire of the heart's rhythm, are so much confused, masked, or replaced by a constant loud whizzing or rasping murmur, that it is impossible to distinguish between them individually, or recognize them collectively: with each impulse and ventricular contraction it commences, and progresses with briskness, roughness, harshness; and at the moment it should cease with the diastole of the heart, and the second sound succeed, a repetition of these physical signs takes place: a retroceding, regurgitating, whizzing murmur, less vigorous, less forcible in its intensity, and possessing less of those characteristic features of the former, becomes developed, and is communicated to the ear; at the termination of which the heart's impulse succeeds, the ventricular contraction follows, accompanied by the

rushing or whizzing murmur, not unlike the sound produced by the rasping of the crust of bread, occupying the entire of first sound, masking the second, and obliterating the interval which naturally intervenes. At the upper part of sternum, and under the clavicles, the second sound is audible, though feeble; not so clear, having lost much of its energy, and not possessing that sharp, well-defined "claquement" so peculiar to it. Each arterial and valvular "clack" is preceded by a bellows murmur, regular in its succession, and constant in its intensity, but less distinct than that heard in præcordial region; seemingly continuous with, or a prolongation of, these sounds, and gradually diminishing as we approach the fourchette of the sternum. Pulse 96, full and regular, counted in carotids; it cannot be felt at wrist on account of the œdema; percussion over the posterior part of right side is dull; the physical signs indicate, in addition to acute bronchitis, extensive congestion and œdema of the pulmonary tissue, with an accumulation of fluid in the left pleural cavity.

The abdomen is swollen; from the quantity of fluid in the peritoneal sac, a sense of fluctuation is afforded on percussion, and the true condition of the abdominal viscera rendered difficult to ascertain. The liver feels hard, enlarged, indurated; its sharp edge thickened and rounded. The intestines are distended with flatus.

History.—For two years she has been subject to a chronic catarrh, and has for many months suffered from pains in the left side, palpitations, flutterings, and other symptoms of deranged circulation; the distress occasioned by the dyspnœa, violent palpitations excited by very trivial causes, agonizing pains, and præcordial oppression, has been much increased within the last few months. The swellings appeared for the first time six weeks since, in the feet and legs, spreading upwards towards the thighs and abdomen, finally extending over the upper extremities, and producing that state of misery in which she was brought into hospital.

Two days after this report was taken she died.

Autopsy.—The serous cavities of the abdomen and chest, left pleura in particular, contained light straw-coloured

fluid; the quantity in the former exceeding four or five quarts; that in the latter, a pint.

The heart, when exposed by laying open the pericardium, occupied the mesial line, and encroached considerably on the right pleural cavity, in consequence of its enormous dimensions from the increased capacity of the right ventricle and auricle; on its anterior surface is one of those white patches so frequently seen, occupying in its extent the circumference of half-a-crown. The veins are enormously turgid, those on posterior aspect, traversing from base to apex, particularly so. The heart is very flabby. The exterior of right ventricle, which is considerably augmented in its transverse and perpendicular diameter, presents a yellowish marbled colour, traversed by turgid blood-vessels; whilst the left, comparatively small, affords a specimen of two rare pathological alterations. As the heart lay in the pericardium, two circumscribed purplish or livid patches attracted our notice; one situated about an inch and a half, the other a few lines, from the apex. The superior one larger, in close proximity to the septum, does not exceed the size of a sixpence; whilst the smaller of the two might be covered by a fourpenny piece. The pericardium corresponding to each is more opaque and dense than that covering the rest of the ventricle. Two distinct indentations, similar to those which may be produced by punching in the muscular substance of the ventricle with the knuckle of the forefinger, were visible on the anterior part of the left ventricle; corresponding to which, and the dark-coloured patches, the muscular fibres of this cavity were so thin, and attenuated, that the interior of the ventricle could be felt by the introduction of the finger into either of these depressions or indentations. On first inspection, it was supposed a perforation had occurred near the apex; such, however, was not the case. By examining from the interior of the ventricle, it was evident that, from the absorption and attenuation of the muscular fibres of the ventricles interposed between the endo- and pericardium, two pouches had formed, which, when distended with blood, or protruded by means of the finger, constituted two circumscribed true aneurisms of the

ventricle; in one of which was contained a dark-coloured fibrinous coagulum, being entangled between the fleshy columns encircling the sac, and distinct from other coagula in the ventricle.

The pericardium can be detached with the greatest facility, and removed from the surface of both ventricles, by exerting a slight degree of traction. The muscular substance of the ventricle, excepting the two places described, is hypertrophied, but of a pale colour, separable into distinct laminae by making a transverse section, raising the fibres, and drawing them from apex to base; thus three, four, or even more plates, or layers of fleshy fibres, can be removed, pale and flabby, but increased in thickness.

It seems probable that the cellular tissue separating the muscular fibres had become infiltrated with serum, participating in the œdematous condition of the cellular tissue diffused over the body; such a supposition becomes justified, from the colour of the right ventricle appearing to originate in a sub-pericardiac infiltration into the cellular tissue, scattered amongst and serving to connect the muscular fibres together—from the muscular fibres themselves being flabby and attenuated—the fleshy columns being thin and numerous—and from these not being sufficient to account for the tumefied appearance and obvious augmentation in the volume of this viscus.

The auricles, right and left, more especially the former, are increased in the capacity of their chambers to double or treble their usual size. Venæ cavæ dilated, gorged with blood; coats somewhat thickened; parietes of auricles hypertrophied; appendices enlarged, and muscoli pectinati lengthened; the endo-cardium of the left is increased in density, opaque, and easily detached from the muscular fibres.

The right auriculo-ventricular aperture is of enormous size, dilated to nearly double its general measurement. The tendinous ring is sufficiently distinct. The tricuspid valves are inadequate to effect the closure of the orifice, two-thirds being thickened, in their perpendicular measurements, with fibrous prominences on their surfaces and borders. The left auriculo-ventricular aperture and mitral valves are

similarly affected, but not to the same amount.

The pulmonary valves are healthy; those of the aorta of a deeper red in their colour, but retaining their transparency; a slight increase in their density was noticed. The calibre of the arch is dilated. Steatomatous depositions pervade the interior of the aorta, and may be seen in greatest number at some distance from the valves. Not far removed from one of these there has been deposited a quantity of fibrin beneath the serous, between it and the muscular coat. Advancing from the ascending and transverse portions of the aorta, the coats were found more healthy.

The superior parts of the pharynx and larynx are of a deep red, approaching to a purple hue. The epiglottis less elastic than natural; its mucous membrane of a violet colour, thickened, and velvety feel. The rima glottidis appears narrowed in its antero-posterior diameter; no ulceration could be detected.

Head. — From three to four ounces of limpid serum was effused between the arachnoid and dura mater, also between arachnoid and pia mater; abundant at the base of the brain, at the superior part of the spinal column, and in the vertebral theca.

In the floor of the third ventricle, anterior and inferior to the orifice of the *iter a tertio ad quartum ventriculum*, a second orifice, distinct, smooth, and circular, attracted my attention in this, as well as on two previous occasions, in brains not affected by disease. Into this orifice the point of an ordinary sized director can be introduced, which, taking the course of the canal, proceeds upwards and backwards, beneath the floor of the aqueduct of Silvius, and that of the fourth ventricle, on the superior surface of the pons varolii. Passing downwards and backwards between the processus a cerebello ad testes, still preserving the mesial line, it gradually contracts in size, and seems to terminate in a species of cul-de-sac, at a short distance above the posterior spinal fissure, with which it was at first supposed to be continuous; but more minute examination did not ratify this supposition, as the continuation of the canal could not be satisfactorily ascertained. On three separate occasions have I been foiled in tracing a direct

communication between the two, and establishing an immediate connection of one with the other, but have succeeded in observing that a minute fissure leads from the apparent termination of the canal. This canal, when laid open in its entire extent, from the floor of the third ventricle, posterior to the infundibulum, to its termination behind, describes a curvilinear course, is circular in its calibre, and presents a smooth, polished, glistening internal surface, being lined throughout its entire extent by serous membrane; its parietes, varying from one to two lines in thickness, consist of a layer of cineritious and medullary substance, inclosed between two laminæ of serous membrane.

OBSERVATIONS.—It is unnecessary to remark, that we possess in this case more than an ordinary amount of instruction; but in commenting on some of the leading features during life, and the pathological changes observed in the autopsy, it will be more profitable to inquire in a cursory manner—

1st. Into the similarity existing between the symptoms, physical signs, and pathology of this case, and those noticed in others, as illustrative of the disease under consideration.

2d. Into the probability of those physical signs having originated in, and being produced by, the aneurisms of the ventricle, and the liability of the one being mistaken for, and confounded with the other, from such coincidence.

3d. Whether the attenuation of the parietes of the ventricle in those two circumscribed spaces, which was conducive to the formation of the aneurismal pouches, resulted from an inflammatory attack of the muscular structures, preceded, was coeval with, or consequent upon the morbid changes, progressing in the valvular structures.

From the combination of two circumstances, permanent patency of the auriculo-ventricular apertures resulted—first, from a dilatation, an increase in the circumferences of the orifices; secondly, from retraction of the valvular tissues and consequent deficiency in their length; diseases in themselves sufficient to account for the presence of the physical signs—the masking of the entire of the heart's rhythm, heard in the præcordial region, by a loud

whizzing murmur or constant buzz. But herein we are enabled to detect a very striking dissimilarity between the auscultatory phenomena and those recorded in the preceding cases; inasmuch as, at no period of the examination, nor by resorting to those manœuvres and experiments, as far as her deplorable condition permitted, were we able to observe a remission or intermission in the vigour, the intensity, the regularity and asperity of those phenomena, so obvious and so constant in the others, as to be considered one of the characteristics of this disease, and deemed almost pathognomonic of its existence.

We have recorded, it is true, a marked difference between the harshness and loudness of the abnormal murmur accompanying the ventricular contraction, and that heard during its diastole. In the description given, it has been noted, that the sound communicated to the ear gave origin to the idea that “the retroceding, regurgitating whizzing murmur, less vigorous, less forcible in its intensity, and possessing less of the characteristic features of the former,” (*vide* report of physical signs, &c. &c.) proceeded from a reflux of blood through the auriculo-ventricular orifices.

That this description, though virtually correct, must be at variance with our physiological knowledge of the succession of phenomena composing the heart's rhythm, and if not commented on, must lead to confusion, requires not a second consideration; instead, therefore, of being led astray by the sounds communicated to the ear, and attributing this train of phenomena to the regurgitated current produced during the ventricular contraction, and in conjunction with the aneurismatic condition of the left ventricle, chiefly instrumental in causing the loud rasping murmur, we must in preference ascribe it to the succeeding column of blood, passing over an uneven, roughened surface, immediately after the subsidence of the muscular contraction; continuing during the diastole of the ventricles; occupying the heart's interval of repose; being propelled forward in part by the action of the auricles, but chiefly by its own unopposed gravity.

Let us now inquire whether these aneurisms were antecedent to, coeval

with, consequent on, or subsequent to, the disease of the valves. The two first inquiries may be disposed of, by our confessing, that through want of a sufficient number of cases, and consequent experience, a perfect silence, must be maintained; but in objecting to trace their origin to, and refusing to acquiesce in any opinion which may attribute these aneurisms to a disorganization of the valves, it is but right to state that this opposition has been grounded on, and the inference drawn from, the extreme rarity of the coexistence of these affections, which, if viewed in the light of cause and effect, ought to retain a greater comparative frequency than has hitherto been recorded.

In searching after the predisposing and determining causes of this disease, pathological examination, and ocular demonstration, direct our attention to the existence of acute inflammation of the serous and muscular structures of this viscus, at some previous period; but, in recalling to mind the different divisions of inflammation; the various changes effected by each, in different parts of the animal economy; the hardening and softening; the thickening and thinning; the increase and decrease of volume, in organs whose structures accurately correspond, we must revert to the effects of that slow, insidious, subacute inflammatory process on other parts of this organ, to be enabled to offer a satisfactory explanation.

During the progress of this inflammatory action, we have had frequent opportunities of attesting that the muscular fibres become weakened and flabby; decreased in bulk and energy; yielding during this atrophying process to the force of the circulating current; and dilating the cavities, in thus yielding during each act of propelling the blood into the arterial system. From an excess of innervation; from a predominance of this enfeebled, thinned, attenuated condition of the fibres in two or more circumscribed spaces, and their consequent inability to offer further resistance to the circulating column of blood, they either give way, are absorbed, or form on various portions of the heart's surface those aneurismal sacs, into the interior of which, the blood entering as it passes through the cavity of the ven-

tricle, a gradual distension of its walls takes place, until in the course of time it may have attained a size equalling the normal dimensions of the heart.

That these pouches did not originate in rupture, nor ulceration of the fibres, consequent on the formation of an abscess in the substance of the ventricle, may be inferred from the uninterrupted continuity of the fibres after the removal of the lining membrane of the aneurism, the endocardium ventricle, and there being none of the distinguishing marks of a former abscess.

I can well conceive that if this aneurismal affection of the parietes of the ventricle had existed *per se*, and the auriculo-ventricular valves had remained sound, we should have had symptoms precisely the same, and physical signs differing but slightly from those described, as the attendant phenomena of permanent patency of the apertures, with morbid growths or excrescences on the surface of the valves*.

NOTES ON SOME POINTS OF INDIAN PRACTICE.

To the Editor of the Medical Gazette.

SIR,

THE following rough notes are intended to give some account of the impression which Indian disease has made on me within the first ten months of my residence in Lower Bengal. How far any opinions here expressed may have to be modified by enlarged experience, it is impossible to say; but it is right to premise, that my experience has been limited to what has successively come to my notice in our Queen's regiment, and in some hundreds of Company's Artillery (both European and native), the former stationed in Fort William, the latter at Dum Dum, within a short distance from Calcutta.

I believe it is an opinion commonly entertained at home, that the characters of Indian are extremely different from those of European disease,—that disease in all cases runs its course very rapidly in this country,

* We have taken the liberty of considerably shortening this paper: if the reader should perceive any want of perspicuity, he will, therefore, please to attribute it to us.—ED. GAZ.

and that remedies must always be applied with a very bold hand. This opinion requires to be greatly modified; and with the exception of those formidable diseases, cholera, dysentery, and hepatitis, no others appear to call for especially active treatment, as it is termed. Many practitioners, however, think very differently, and deplete and administer calomel in quantities, often followed by the most disastrous consequences; and there is certainly in this country a much greater risk of too much being done for a patient than too little.

After all there is a great deal of uniformity in Indian disease, as well as in our treatment of it. The last of these facts is, in a considerable degree, to be attributed to the writings of the late Mr. Twining, whose book is usually adopted by assistant surgeons, on their first arrival, as a hand-book. In this there is certainly one advantage, that by uniformity of treatment they are soon enabled to form a fair estimate of the value of the remedies which they employ. In a country so vast as India, it must be a matter of regret, that so little has been done to develop the resources of its materia medica, for we still depend wholly on England for our medical supplies; and the partial substitution of chirayta for gentian is almost a solitary instance of a native being introduced with success in the place of an European remedy*. In the following notes, which possess no novelty, and chiefly refer to a few points of practice among Europeans, it is not proposed to enter into any details regarding diseases whose history and pathology are well known, and have been carefully described.

Diseases of Europeans.

Fevers.—There appears to be nothing peculiar in the character of intermittents in this country; purgatives, occasional general and local depletion (the last of which should be employed with much care), quinine; and in obstinate cases, the liquor potass. arsenit. (the judicious use of which is scarcely ever followed by any disagreeable consequences), are our main remedies. Mac-

kintosh's plan of bleeding in the cold stage, a most dangerous one according to all analogy, has met with few advocates, and seems to be almost forgotten. Narcotine, lately introduced into practice by Dr. O'Shaughnessy, although it may possess some febrifuge virtues, is undoubtedly very far inferior to quinine. Agues, of course, bear in all countries a local character; and the Arracan fever has almost the inveteracy of that of Walcheren. I shall say little of *remittents*, as my experience of them has been very small; they are very insidious in their character, and demand constant observation on the part of the physician. Slight cases have recovered under the use of leeches, saline purgatives, and antimonial diaphoretics, followed sometimes by the moderate exhibition of quinine.

Continued fever is not of common occurrence, and petechiæ, or typhoid symptoms, are rarely seen. Patients, however, not unfrequently fall into a very low state, attended with cerebral symptoms, in which cases stimulants and local counter-irritants are indicated. It may be here not unworthy of remark, that in a few fatal fever cases, which were examined last summer, no abdominal lesions could be detected. Slight effusion into the ventricles, and slight congestion of the vessels of the brain and arachnoid were the only perceptible morbid changes.

There was a slight form of fever very prevalent in June of last year, which may be termed *catarrhal*. As many as eighteen men have been admitted into hospital in one day, all complaining of headache, and of pain in the loins, with their conjunctivæ injected, and general irritation of the mucous membranes. A few leeches, with saline purgatives, always effected a cure.

Some account of the *Chusan fever*, as I have heard it described by Mr. Wrightson, and other gentlemen who have had these cases under their care, will probably be interesting, both from the peculiarity of its features, and from the fearful ravages which it has made among some of our corps, especially the Cameronians. The disease usually commences with an attack of fever, which is followed by dysentery, sometimes of a pretty acute character. To this usually succeed a chronic diarrhœa, and a gene-

* Much is however expected, and with good reason, from Dr. O'Shaughnessy, who has been for some time engaged in preparing a work on this subject.

ral wasting away of the system, to which we may add occasional anasarca and ascites. The tongue assumes a morbidly red appearance, looking as if the epithelium had been removed; yet the appetite remains good. From this condition the patient rarely ever rallies, an increase of diarrhoea supervenes, and he is carried off in a few days. The post-mortem appearances are: the stomach and intestines pale and shrivelled, not ulcerated, (though some think they have detected slight abrasions near the pyloric orifice); the liver and spleen are generally enlarged, soft, and gorged; and, what appears to be most characteristic of this disease, the mesenteric glands are enlarged to the size of beans, and full of concretions: so that mesenteric obstruction, and consequent atrophy, would almost seem to be the cause of death.

The symptoms and appearances above detailed were common to the Europeans, and to the Bengal volunteers; and it is a melancholy fact, that scarcely a man of the invalids, who have returned to this country, is likely to recover. No very satisfactory reasons have hitherto been assigned for the origin of this disease, although the report of the superintending surgeon of the expedition may, when published, throw some light on the subject.

Of *cholera*, which has prevailed pretty extensively during the last month among Europeans and natives, it need only be remarked, that the usual treatment is, to exhibit calomel in ten grain and scruple doses, combined with one grain of opium, every hour or two hours, according to circumstances; to give stimulants internally; and externally to use frictions and sinapisms: and very unsatisfactory this treatment often is. One point regarding the prognosis, lately suggested by Dr. Mouat of her Majesty's service, may be worthy of attention. He states, that when pulsation of the heart cannot be detected with the stethoscope, the case is hopeless; that if it can, the patient may recover. The last epidemic of cholera in Bengal occurred some months ago, in the decayed city of Dacca, in which most of the inhabitants are in a state of abject poverty.

Syphilis is very prevalent both among

Europeans and natives; but it does not seem to present any peculiarity worthy of notice. Secondary symptoms are not very common, and perioritis occurs only in cases in which the use of calomel has been excessive. I have not yet met with any case of syphilitic iritis. The majority of cases get well readily, under the use of saline purgatives, and of low diet; and sarsaparilla combined with bichloride of mercury, or with iodide of potassium, are the most useful auxiliaries in secondary cases.

Delirium tremens is unfortunately a very common affection, usually occurring, as experience shows, among confirmed tipplers. Men who are in the constant habit of drinking, though not in sufficient quantities to produce intoxication, are much more liable to attacks of it than those who only indulge occasionally to excess. In these cases depletion is not often indicated. The moderate use of opiates, along with purgatives, appears to offer the most successful mode of treatment. Terebinthinate enemata, and opium and camphor in the solid form, generally produce the very best effects. Cannabim, the active principle of hemp, as lately introduced to notice by Dr. O'Shaughnessy, has been employed with some advantage in this disease; but its efficacy is understood to be much less than that of opium or morphia.

The form in which *rheumatism* usually occurs is frequently very obstinate; it is oftener chronic than acute; and I have not hitherto met with any case of rheumatic pericarditis. Diaphoretics, guaiacum, and small doses of iodide of potassium, are useful in its milder, and calomel and opium in its severer, forms. Where there is thickening about the joints, the local application of the tincture of iodine is sometimes very efficacious, as also occasionally in indolent buboes.

Dysentery is one of the most formidable diseases with which we have to contend, whether the violence of its attacks, or the frequency with which relapses occur, be considered. In its acuter forms I have not seen a great deal of it; whereas, in its milder ones, where the boundary between it and diarrhoea is indistinctly marked, cases of it present themselves daily. It cannot admit of a doubt that calomel and drastic purgatives have been most injudi-

ciously used in this disease, and that a return to a milder mode of treatment will be attended with the most beneficial results. Indeed, it has been stated, that the present distinguished Inspector General proposes to issue an order, forbidding the use of calomel among the Queen's troops. There is no difference of opinion as to the propriety of free depletion in the earlier stages of this disease, followed up by the use of mild purgatives, among which castor oil is quite invaluable. The combination of blue pill, ipecacuanha, gentian, and hyoscyamus, so commonly employed, is a most useful preparation; and opium is also a very important remedy, although the belief that it merely masks the disease is very prevalent. An opiate enema, or Dover's powder, may in most stages of the disease be most usefully administered.

On post-mortem examination the colon is always found to be the chief seat of disease. In one case it was enormously ulcerated and thickened near the caput cœcum; while in another it was studded with patches of deep, though comparatively superficial ulceration.

Hepatitis, in its different forms, is of course a very common disease, although acute abscess appears to be less common here than at Madras. Last year the number of cases of hepatic abscesses was very considerable, and their progress was as usual very insidious. I have seen a man admitted into hospital with an anxious countenance, and complaining of pain in his right side, who died suddenly two days after, in which case the whole of the right lobe was found converted into one huge abscess. In this man the abscess of his liver must have existed for many days before his admission, although it had caused him only a little uneasiness. The late universally respected Inspector-General* was ill for three months before his death. The chief symptoms were general languor and debility, along with irritability of stomach. He was attended by the most eminent physicians in Calcutta, who frequently examined the region of the liver, in which nothing could be detected, and uneasiness was never felt; yet he was suddenly seized with pain in his side, expectorated a large quantity of pus, and sunk; dying, no doubt, of hepatic abscess.

I remember another case, in which

the diagnosis was very difficult. A young man had a constant fixed pain in his right side, in the region of the liver, and in his shoulder, accompanied with a dry cough; in consequence of this his case was at first treated as an hepatic affection, and abscess of the liver was suspected. Ultimately the case became evidently one of phthisis; and on *sectio cadaveris* a large vomica was found near the bottom of the right lung, the apex being nearly healthy. The liver did, however, bear some traces of former inflammation.

Another case attracted a good deal of attention. The health of an hospital serjeant had long been much impaired, when he was attacked with a constant diarrhœa; his motions being of an excessively unhealthy character, and protrusion of the rectum followed. A palliative treatment was adopted, but the symptoms got worse: the abdomen excessively attenuated. On laying the hand on the abdomen, what appeared to be a pulsating tumor was felt, and he was generally believed to labour under aneurism of the aorta. He gradually sunk. The aorta was carefully examined after death, by Mr. Freeman, and found perfectly healthy; while the liver was studded with abscesses, and the small intestines partially ulcerated.

There appears to be but one opinion as to the treatment of acute hepatitis; and free depletion with the use of calomel is the almost invariable practice. When the case has once advanced to the formation of abscess, the treatment is very difficult; but I have not met with any case in which the abscess opened externally, or which had its contents evacuated by artificial means.

Affections of the lungs are not among the most common cases, as soldiers before coming to this country have usually passed the age when tubercles are most likely to develope themselves; yet phthisis occasionally appears, and runs its course as at home. Chronic bronchitis is by no means of unfrequent occurrence; and in a climate where the changes of temperature are so rapid, common catarrhs are frequent. I have not seen many affections of the heart, though inordinate action of it, and palpitation, sometimes attend convalescence from fever, and often prove rather intractable.

The diseases of children are, if

* Dr. Donald Macleod.

possible, of more importance here than at home; and it is quite extraordinary how rapidly a diarrhœa, or cephalic attack, during the progress of dentition, will carry off its victim. I have seen one well-marked case of cholera in a child of seven, who died a quarter of an hour after its admission into the hospital. A detachment which lately arrived from N.S. Wales lost several children of dysentery, and in every case lumbrici were present. Nothing peculiar appears to be indicated in the treatment of these affections. No medicine exceeds in value the Hydrarg. c. creta, combined with other remedies; yet it is a common saying, that children will slip through one's fingers in spite of the most judicious treatment.

Surgery.

There can, of course, be little that is peculiar to the surgical diseases of this country; and this will immediately strike any one who peruses Mr. Brett's recent work on the Surgical Diseases of India. The cases among Europeans are usually of the most trifling nature, and operations are very rare. Acute otitis and otorrhœa are at times very prevalent.

I remember one case in which a horse artillery man, who fell backwards from his horse, came into hospital with both humeri dislocated under the clavicle*. In another case, of a native who had dislocated both condyles of his lower jaw by yawning, every effort at reduction was unavailing; yet he possessed the powers of mastication and of speaking to a considerable degree, and, as the jaw had then only been one month dislocated, it would no doubt in time become serviceable enough. The difference between Europeans and natives, in the power of sustaining any great injury, is very marked. Thus I have, within the last few days, seen a native labouring under extravasation of urine of thirty-six hours standing, and who exhibits little constitutional disturbance; while the parts would have ere now been sloughing in a European, and the patient labouring under violent irritative fever.

From this feature in the constitutional power of the natives, operations may be undertaken on them which would be out of the question in the

case of Europeans; and there is a wide field for operative practice, on cases of stone, enlargement of the scrotum, and cataract. Some forms of ophthalmia in this country take on a very intractable character. In one case, occurring in a European, the inflammation was limited to the conjunctiva, and was of a remittent character. The eye would be nearly well in the morning, and before night highly injected. Every variety of alteratives, as well as local applications, was made use of without any permanent benefit. In another case, in a half caste, the sclerotica and cornea of both eyes were involved, and the inflammation was at times so violent as to threaten the destruction of the corneæ. Those more violent attacks were moderated by the use of calomel and opium; but it has been found quite impossible to get the eyes well, and they are rather worse now than when he came under treatment four months ago.

Diseases of Natives.

Some of the most common diseases of natives are ague, affections of the spleen, dysentery, rheumatism, lepra, and psoriasis; and among these disease of the spleen offers the chief peculiarities. It is always attended with general debility, and especially of the vascular system, from which some have been led to suggest an analogy between it and scurvy.

Enlargement of the spleen is usually treated with a combination of tonics and purgatives—of iron and gentian, with colocynth and scammony, &c. but with very small success. More confidence might perhaps be placed, with justice, in the exhibition of quinine and the mineral acids, or even the liquor arsenicalis. Some have proposed the use of iodine, but spleen patients are seldom in a state in which they could bear its exhibition. It is a common saying, that practice among natives is not satisfactory; and one reason, though it does not say much for our humanity, is, that we often do not take enough of interest in our patients: another difficulty always meets us, in the impossibility of regulating their diet. And here, it may not be out of place to mention one interesting fact, which shews how much that form of functional amaurosis, termed day-blindness, depends on the condition of

* He afterwards returned with one of them dislocated again. I easily reduced it with my hand.

the digestive organs. In the Burmah war some of the troops suffered much from this affection, until it was discovered that they had a deficient supply of the condiments usually employed by them in making their curries. Whenever this deficiency was supplied, they all recovered perfectly.

The subject of medical and of general statistics appears to be daily exciting more interest at home, and the reports lately compiled from the records in the Director General's office are replete with valuable information. Some cultivators of statistics, however, appear to indulge in many visionary ideas, and even talk of reducing the principle of the science (far better termed art) of medicine to the accuracy of mathematics. In all this they seem quite to forget that disease is a far more varied and complicated problem than any one in the whole range of the physical sciences, and that every thing connected with its causation is hid in far deeper mystery than the principles of meteorology, of which, after continued and accurate information (far more accurate, be it remembered, than can ever be applied to disease), we are only beginning to recognise a few outlines. It is nevertheless much to be regretted that the medical authorities in this country have paid so little attention to the statistics of disease. Such records as do exist among the company's troops are very imperfect, and relate chiefly to Europeans. Those among the Queen's troops are more perfect, but still leave much to be desired. One very considerable evil arises from the very defective nature of the classification of disease adopted in the returns among the company's troops; and it will scarcely be credited that in a new one, recently adopted by the Medical Board, cataract has gravely been set down among nervous diseases!

As some of the readers of the GAZETTE may take an interest in the state of our periodical literature, I subjoin a list of the scientific and medical journals at present published in Calcutta. The Asiatic Society continues to publish its journal, and the Medical and Physical Society publishes occasionally the papers or abstracts of the papers read before it. Mr. Corbyn's Scientific and Medical Journal both go on, and Mr. M'Clelland has lately started a journal of

Zoology and Natural History, which is likely to be supported with spirit.

I am, sir,

Your obedient servant,

J. MACPHERSON,
Assistant Surgeon to the Royal Horse
Artillery.

Dum Dum, March 8, 1841.

P.S. Farcy and glanders have been very prevalent among the troop horses here for some years, usually attacking the animals in the rainy season. Besides tubercular deposits in the lungs, abscess and softening of the ribs are often found after death. No case of glanders in the human subject has hitherto occurred here.

PECULIAR STATE OF THE BLOOD.

To the Editor of the Medical Gazette.

SIR,

I TAKE the liberty of sending you, for publication in your valuable journal, the following case, in which the blood was found deficient in, if not altogether void of, serum, and to all appearance too thick, in consequence, for free circulation through the capillary vessels; giving rise to a painful, harassing, and obstinate complaint, which corresponded with that in Bateman's work on Cutaneous Diseases, under the title of *Urticaria tuberosa*.

My object in giving it publicity through your pages, is with the view of drawing the attention of the profession to the state of the blood in similar cases; hoping that the inquiry may lead to the true pathology of the disease, and to a rational and successful method of treatment.

A German sailor was placed under my care in the summer of 1840, at Archangel, for the cure of the above-mentioned complaint. He stated that he had laboured under the disease six months; that he lay a considerable time in an hospital in Holland for relief, from which he was discharged as incurable; that, to avoid starvation, he went to sea, and on the passage out from Holland to Archangel, that his complaint grew worse than ever, depriving him of the use of his limbs, and compelling him to take to his bed.

His whole body was covered with painful and intolerably itchy swellings, of the size and shape of an almond, white at top, and surrounded at base by a rosé-coloured blush, which increased in intensity as night approached. His legs and arms were swelled, stiff, and painful; as also his eyelids, which he could hardly raise sufficiently to enable him to see. The conjunctivæ were injected with red blood, and manifestly affected in the same manner as the skin, being here and there, especially towards the inner angles, swelled. The eyes had a slimy glassy appearance, betokening some distress within the head. He complained of a dull pain and sense of weight in his head, with great intolerance of light. His pulse was small, weak, and of the natural speed; he had no appetite for food, but drank a good deal; his tongue was white, and his bowels costive; he discharged a considerable quantity of urine, which emitted almost immediately after being voided an ammoniacal odour; he had frequent nausea, but seldom or ever vomited. On examining the region of the liver, pressure gave him some pain; externally, however, there was no fulness observable. Towards evening the pain and itching increased, and continued to keep him in torments all night. As morning approached both remitted, leaving him weak and exhausted.

Considering the disease as proceeding from a disordered state of the stomach and biliary organs, I directed my attention towards the removal of the same, prescribing for this purpose the usual means; as calomel, saline effervescing draughts, taraxacum, &c.

After making use of these for six days, I had no reason to flatter myself with having made any impression on the disease. If the bowels were brought to a proper state, the pain in the region of the liver diminished, and the white tongue removed, the eruption was much worse, the pain, itching, and redness being much increased. Nor was the warm-bath, nor hydrocyanic acid, applied externally in the shape of lotion, of the slightest benefit to assuage his sufferings. The patient observed to me that he felt much worse immediately after purgation, and upon close observation; I found that he was correct. His headache and intolerance of light were also increased.

Under these circumstances I resolved, though the strength of the patient and the state of the pulse contraindicated such a step, to take some blood from his arm; and though I had made a free opening in the vein, which was large, it was with much difficulty I succeeded in getting ten ounces from him. Being desirous of seeing whether the blood would exhibit any signs of inflammatory action, I took every care of it, and watched its coagulation. To my no small surprise, I found, after it had coagulated and stood some time in that state, that there was no separation into serum and crassamentum, but that it formed one dense mass of crassamentum, of a highly red colour, strictly adhering to the sides of the plate, on the margins of which, where some blood had slowly trickled from the arm, were streaks or bands of fibrin, of considerable tenacity. Thinking that it might not have stood a sufficient time, I put the plate in a cool place till next day, when, however, there was not the slightest appearance of serum observable; nor was there any separation between the crassamentum and the sides of the vessel to lead me to think that evaporation had taken place. After the bleeding, the patient expressed himself a little relieved from the pain and weight in the head; and on the whole he passed a better night.

It occurred to my mind, that the peculiar state of the blood, which I described, from whatever source it might derive its origin, was the cause of the eruption, and of the train of harassing symptoms attending it (the blood being too thick for free circulation through the capillary vessels); and that the only chance of relieving the patient was to restore its watery constituents. For this purpose, and to reduce the quantity of fibrin, I know of no remedy equal to frequent bleeding from the system. I accordingly bled the patient again, to the extent of a pint; put him on a thin watery diet; prescribed hyd. c. magnes. twice a-day, and continued the use of saline draughts, with an excess of alkali, and of ext. taraxaci, with an infusion of calumba, in order to restore the digestive organs to a healthy action, from a derangement of which I suspected the peculiar state of the blood to originate. On examining the blood after

coagulation, it exhibited the same appearance precisely as on the former occasion, except that there were a few drops of limpid watery fluid here and there on the surface of the clot, as if they had oozed from it.

After this second bleeding, the patient experienced decided relief: his headache, and intolerance of light had left him; the redness of the conjunctivæ had disappeared; during the day the eruption was hardly observable, and gave no trouble; at night, however, it visited him regularly, with its usual train of symptoms, but in a much less violent degree.

In this state the patient left me, being obliged to proceed to sea with his vessel; and whether he continued to improve, or whether he relapsed into his former state, I do not know.

The questions which present themselves for solution, as connected with this case, are:—

1. Was the eruption, with its train of symptoms, owing, as I suspected, to the peculiar state of the blood described, and thus proving that the Boerhaavian doctrine of lentor or spissitude of the blood is not without some foundation?

2. What was the cause of this diseased state of the blood? Was it owing to a peculiar derangement of the digestive organs? Or was the deranged state observed in this case an effect, not a cause?

3. Might not the kidneys have been diseased in such a manner as to drain the blood of all its serous constituents? This question suggests itself from the extraordinary quantity of urine voided in this case by the patient. (I am sorry that I had forgotten, or neglected, to determine whether the urine was albuminous or not.)

In support of the opinion that the eruption was owing to want of sufficient fluidity of the blood, was the fact that purging, which is well known to diminish the watery constituents of the blood, always exasperated the disease.

I am, sir,

Your obedient servant,

JOHN MACKENZIE,
Surgeon.

9, Adam Street, Adelphi,
June 21, 1841.

MEDICAL GAZETTE.

Friday, June 25, 1841.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”

CICERO.

APOTHECARIES v. CHEMISTS.

THE opinions given by the four Judges of the Court of Queen's Bench, in making the rule absolute for a new trial in this case, were so unanimous that in the former trial the Jury had been misdirected by Mr. Baron Maule, and it is so evident that but for this misdirection the verdict would have been different, that it is no more than reasonable to anticipate that the result of the new trial will be the reverse of the last, and that the Society of Apothecaries will have their rights restored. But even if this be the case, the difficulties that will still be in the way of materially amending the evils of the present unsettled state of professional privileges will be very great.

A list has, we believe, been already made out of twelve hundred persons who, if the Apothecaries' Company can fully establish the rights which they claim in this action, will be liable to their prosecution, each with much probability of suffering conviction. But if the whole list could be prosecuted and punished it would be of little avail; for it does not include a tithe part of the open offenders against the Apothecaries' Act; nor is it possible to put an end to the practices by which it is more quietly but yet more extensively evaded. In fact, the evasion of the Act is universal among the retail chemists and druggists; or, if not universal, at least any one who chooses evades it as often as he pleases, and to any extent that he may think profitable; and the few exceptions are to be found among those whose business as *pure* chemists and druggists is so large as to yield them better returns than

they could obtain in practice as small and cheap apothecaries. With these exceptions all chemists are practitioners of medicine; all are virtually apothecaries; and the differences between them depend only on the degree of approximation which they make towards being physicians, and even operating surgeons; for of late they have become in no few instances *myotomists*, and curers of various distortions. So that if a jury were set to determine, *as a question of fact*, not (as they were in the late trial) what was the difference between a chemist and an apothecary in 1814, but what is now the difference between them in 1841, it would really be very difficult for them to decide. *In fact*, the two are but one class, whose members differ from each other not in kind, but only in degree: in the degree, we mean, in which each separates himself from the simple business of selling drugs and other related articles, to those who apply for them, and raises himself to the verbal recommendation of the drugs which he himself sells, or to the written prescription of drugs to be bought of himself or of another. In law, however, the difference is more easily defined. The chemist, as we shewed last week, if he will keep strictly within the law, has nothing to do with diseases, but to sell the drugs which others ask for at his counter.

But law not enforced, or law that cannot be put in action as often as it is infringed upon, is, to the majority even of honest men, no law at all, and if the public are to receive the protection which the Apothecaries' Act was designed, and, at the time of its enactment, was very well adapted to afford, they must look to some other power for it. The force of this is already deeply damaged by the number of attacks made upon it, and there is now much danger, if it be put in action too rapidly, or with too great energy, that it will go utterly to pieces.

But even if the Apothecaries' Act could be so enforced as to put an end to the contraband practice of all druggists, and to make the rights of an apothecary the exclusive privilege of the Licentiates of the worshipful Society, we are not confident that the result would be beneficial to the public. It is not more to the honour of the profession than it is to the expense of the public's purse, that apothecaries have of late years risen so much in scientific rank, and that now many occupy places among the highest in the public confidence. But the attainment of this higher position has not only required a much more prolonged and difficult course of education, but, after each possessor has attained it, has demanded, or at least expected, for its maintenance, a much higher rate of expense. In a word, to be an apothecary now-a-days costs much more than it did twenty-six years ago, and that portion of the public who could afford to employ and maintain the whole Society of Apothecaries then, cannot do so now, and therefore look to a new class to supply their necessities—*safely* if it be possible, but *cheaply* at all events.

Of course not even a chemist would pretend for a moment that any but *very* old women would apply for his services, except for their being cheaper than those of better qualified practitioners; the cost therefore is the only circumstance on which a competition can be entered into. But it may be a reasonable question, whether, in deference to the opinion which the public so plainly express by their acts in this matter, some thing should not speedily be done to avert the evil which they are bringing upon themselves. The public it is plain *will* employ chemists and druggists for the treatment of their maladies. Can they be prevented from doing so? No; the Apothecaries' Act has not force enough to prevent chemists

from practising. Can they be safely permitted to do so? No; chemists know no more of practice than they do of astrology, and are in no respect more fit to treat any disease than the patient himself is. Can, then, the licensed apothecaries, who *are* fit to treat diseases be placed in a position to compete on equal terms (knowledge excepted) with the chemists and druggists? No; they cannot descend from the position which they have attained at considerable expense and labour, to that in which any man may place himself at pleasure, and that too with a very small stock in trade. What, then, is to be done for the preservation of the public who will not, or rather cannot, guide themselves in such matters, and of whom the great mass often have their lives put in jeopardy by being entrusted, at the time when medicine might most avail, to those in whose hands remedies may be either inert or poisonous, but can never be certain, or even likely, to have a beneficial influence? We believe nothing will be of advantage to them but the securing that chemists should be made in a measure fit for the practice of the more simple parts of medicine.

The demand for this change is not made in the clamour of a few, nor is it even quietly expressed by many; yet it is expressed more plainly than words could do it by the practice and custom of a great majority of the public. If the people knew the utter absence of safety in employing druggists as medical practitioners, we have no doubt that they would petition in numbers for a measure like that to which we allude; but the truth is, that they are unable to judge of any merits comparatively, however widely different they may be; and many of them believe (their hopes all the while prompting and encouraging their thoughts) that for most sicknesses a druggist will do just as well as a licensed practitioner; or that,

at any rate, if he is not quite so safe an adviser, yet there is less difference in the degrees of safety than in the price. Nor can the public be instructed to believe better things than these: and it is vain to expect that they will do right till there is scarcely any wrong way in which they can go. Now we believe this state of things would be nearly attained if druggists were licensed to be practitioners in the lower departments of medicine, and were licensed upon exhibiting at an examination as much knowledge of practical physic as should afford a tolerable guarantee that they would avoid doing mischief, and could do some good.

In determining the standard of knowledge with which one should be admissible to the license, it ought especially to be borne in mind, that it need not be so high as to require for its attainment a long or expensive course of education; for if this were the case the progress of the apothecaries would be imitated by the chemists, and the latter would soon be involved in the same difficulties as the former have now to contend against. For as soon as the entrance to a profession or a trade is made more difficult and more expensive, men higher in rank, and of more ambition, straightway enter it; and these, as they must maintain themselves at least in the station in which they were born and educated, require a proportionably higher remuneration than their predecessors. This has been often abundantly proved in our profession, and if opportunity were again given to prove it in the case of the chemists licensed and made practitioners, we should immediately after the proof find that a class would spring up from among the grocers, or some other now unambitious tradesmen, who would take the position at present occupied by the druggists, and constitute what the public *will* have at all times, and under

all circumstances, whether compatible with their own safety or not—a class of cheap medical practitioners.

We repeat, then, let the chemists be licensed as soon as they prove themselves fit to practise, and let them be licensed under such circumstances that they may be *the* cheap practitioners, and may continue to be so as long as possible; for the admission of another class, even to a pretence of practice, would be intolerable. And in recommending this plan we must have it observed, that no injury would fall upon the general practitioner if it were adopted: the practice of a chemist would not be extended by a license; the only difference would be, that what he now does unlicensed and ignorantly, he would then do legally, and perhaps with knowledge and utility. If any think that such a plan would legalize infringement upon their privileges, we can only remind them, that they can never hope to prevent the infringements of their privileges, which are already carried on in every street of the kingdom; and that, in its relation to their profits, it can make no difference whether they are diminished by the law or against the law.

With respect to the extent to which a licensed chemist might be allowed to practise, we think that it should be limited to that class of patients who can present themselves at his counter. This would include all those who suffer from ephemeral disorders, and from them upwards to those who suffer from diseases similar to those which are commonly observed among the out-patients of hospitals, and of non-visiting dispensaries—a class amply sufficient to afford a handsome remuneration to those who are capable of taking charge of them. The restriction should be, that a chemist should not visit a patient at his own house: the care of such as cannot, or will not, leave their beds or their rooms, should be left to the higher classes of the profession.

With regard to the degree of knowledge which should be deemed necessary for obtaining a license to this amount of practice, we think it should include a general acquaintance with the chief parts of anatomy and physiology, a practical familiarity with the appearances and the uses of the *materia medica*, and a practical knowledge of those parts of medicine and surgery in the treatment of which the licentiate is to be engaged. Such a knowledge as this might be attained by two year's dispensing of medicine, either during an apprenticeship to a practising druggist, or after the completion of an apprenticeship to one not practising; and by attendance for a session on lectures on anatomy, medicine, and surgery, and the *materia medica*, and by dissections during the same period.

We can easily imagine that one objection to this plan will be, a "little knowledge is a dangerous thing;" and we shall therefore at once reply to it, that a little knowledge is not dangerous except when its possessor regards it as great knowledge, or when it is employed about great things. Both of these conditions might easily be provided for in the future management of the practice of a chemist's business; or even if they could not, yet still when men do already pretend to great knowledge, and occupy themselves in important offices, it would be better that they should have a little knowledge than none at all.

COMPOUND NATURE OF SOME OF THE SUPPOSED ELEMENTARY SUBSTANCES.

CONVERSION OF CARBON INTO SILICON.

THE Edinburgh savans have, during the last few weeks, been labouring under considerable excitement in consequence of the alleged discoveries of a young physician of that city; which discoveries, if true, will create a complete revolution in chemistry, as well as in all the allied sciences. It was reported that a Dr. Brown had positively succeeded in converting carbon into silicon, and that Professor Christison had

repeated his experiments, and found them to be correct!! Furthermore it was stated that Dr. Brown was occupied with the transmutation of other supposed elementary substances, and that he had succeeded in converting iron into rhodium; and we understand that specimens of the metals have been publicly exhibited, though the process for effecting the transmutation has not yet been published. Rumour also reports the conversion of oxygen into sulphur, magnesium into calcium, and the decomposition of arsenicum*.

For several years past chemists have been acquainted with the fact that two or even three substances, possessing very dissimilar physical and chemical properties, may be composed of the same elements, and in the same relative quantity. Thus cyanogen gas is a compound of—

2 atoms Carbon	=	12
1 atom Nitrogen	=	14
<hr/>		
1 atom Cyanogen	=	26

And paracyanogen, a brown solid, is composed (according to Professor Johnstone) of—

8 atoms Carbon	=	48
4 atoms Nitrogen	=	56
<hr/>		
1 atom Paracyanogen	=	104

Now 12 : 14 :: 48 : 56. So that the relative proportions of carbon and nitrogen in these two bodies is the same. Such substances are called *isomeric* (from *isos*, *equal*, and *μερος*, *part*).

On the 15th February, 1840, a paper was read to the Royal Society of Edinburgh—

On the Preparation of Paracyanogen; and on the isomerism of Cyanogen and Paracyanogen. By SAMUEL M. BROWN, M.D.†

In this memoir, the author, after making

* By the way, Dr. Lambe (*Medical and Experimental Inquiry into the Origin, Symptoms, and Cure of Constitutional Diseases*, London, 1805), long since asserted that arsenious acid was generated by the putrefaction of organic matter; and in 1828 he announced (*An Investigation of the Properties of Thames Water*, p. 14,) that arsenicum (or a substance simulating it) had been procured from Thames water. The recent assertion of Orfila, that arsenicum exists in the human bones, (a statement the accuracy of which Dr. O. Rees has lately denied), and the alleged discovery of Dr. Brown, gives additional interest to the statement of Dr. Lambe.

† Edinburgh Monthly Journal of Medical Science, March 1841.

some critical observations on the history of the discovery of the remarkable body, paracyanogen, proved by a selection of experiments, that, once formed, it can never be resolved into cyanogen again, and made it evident that the cyanogen obtained from it by Professor Johnstone (Phil. Trans. 1838), had only been retained by the absorptive power of paracyanogen. The main object of the paper, however, was to make out, by a series of experiments, that *the bicyanuret of mercury, decomposed under high pressures, is resolved into mercury and paracyanogen*; and in the course of the experimental details, Dr. Brown described a “differential barometer,” which he used to estimate the degree of pressure (1·78) under which cyanogen is separated from mercury as paracyanogen.

Perhaps the most interesting part of this novel contribution to the science of chemistry, was that which contained the author’s view of the constitution of paracyanogen, and of isomeric groups of bodies in general. He represents paracyanogen as a cyanide of cyanogen, *Cy* and *Cy*, a compound of two “equal and similar atoms.” Our space does not admit of a statement of his argument; suffice it, that once established, it deeply affects the doctrines of affinity and constitution which have hitherto been laid down, and affords a practical foundation for an hypothesis of the composition of the elements, susceptible either of establishment or refutation by experiment.

Professor Forbes having stated the mathematical difficulty which stands in the way of the conception of the chemical union of two “equal and similar atoms,” Dr. Brown explained that the perception of that very difficulty on his part, had been the mental initiative of his experiments.

We understand that this memoir is to be followed by another, on the transmutation of carbon into silicon, or, at all events, of some one element into some other.

The following is a notice of Dr. Brown’s paper, read at the Royal Society of Edinburgh, on the 3d of May, 1841, on the conversion of carbon into silicon, given in Sir W. Jardine’s *Annals and Magazine of Natural History* for June.

Experimental Researches on the Production of Silicon from Paracyanogen. By J. BROWN, M.D.

The author had intimated in a former paper, that he had been led to infer from experiment, that two familiar substances, long and universally regarded as distinct elements, are only modifications of one and the same material form; and having extended

his enquiries, he now ventures to maintain that carbon and silicon are isomeric bodies. The method in which he establishes this proposition is very simple, and consists in the description of a number of processes by which carbon may be transferred into silicon; and crucial experiments, intend to prove that there is no intelligible source of fallacy in the processes which are given. Accordingly, the present communication is of a freely practical character. It is composed of five sections: the first treats of the production of silicon from free paracyanogen; the second of the formation of amorphous, mixed siliciurets of copper, iron, and platinum, by the reaction of paracyanogen on these metals; the third, of the quantity of nitrogen separated from paracyanogen when it is changed into nitrogen and silicon; the fourth describes processes for the preparation of definite and crystalline siliciurets of iron from the paracyanide of iron, and from the ferrocyanide of potassium; and the fifth gives easy formulæ for the extraction of silicic acid from the ferrocyanide of potassium by the action of carbonate of potassa.

Our reason for noticing this investigation in a periodical devoted to the objects of natural history, is this: if Dr. Brown's observations be corroborated by those who repeat his singular experiments, there will be opened up a new sphere of geological inquiry of the highest order. With the aid of a transelemental chemistry (for we understand Dr. Brown has transformed several other elementary forms besides carbon), we may approach the subject of the molecular genesis of the earth; and the geological relations of carbon and silicon are certainly sufficiently striking to warrant the entertainment of this hope. As it is, there are several points in natural history which seem to be illustrated by the particular case of transformation now in hand. As one instance, we could specify the siliceous character of many organic remains, found in circumstances in which the source of silicon is perplexing and unintelligible.

In the discussion which followed, Professor Traill remarked, that though he had not had an opportunity of repeating Dr. Brown's experiments, yet, from his acquaintance with that gentleman, he had a strong conviction of their value; and this notwithstanding the very startling principles and extraordinary conclusions to which they necessarily led. He had no hesitation in saying, that since the early days of Davy, when that great chemist brought the metalloids to light, no investigation had been made approximating in importance to the present, whose publication would do honour to the Society, and whose interest, as it regarded the subjects of Botany, Palæontology,

and Geology, in its widest range, was altogether unbounded.

Professor Christison begged to meet a statement which he had understood had gone abroad, that he had given a guarantee to the accuracy of these investigations. This was by no means the case. The fact was, that, now for some time otherwise employed, he was not capable of testing these admirable experiments: no one, in fact, could do so but a first-rate analytic chemist, perfectly master of the most recent manipulations of the laboratory; and he would warn every one against coming to a decision regarding these conclusions, well styled *startling*, either for or against such preliminary investigation. At the same time, it was true that he had been familiar with the details of the inquiry; he had searched, along with the author, but in vain, for grounds of fallacy, and he formed the very highest estimate of their value and importance.

As some of our chemical readers may, perhaps, be desirous of submitting Dr. Brown's statements to the test of experiments, we subjoin the following directions for the preparation of silicic acid from ferrocyanide of potassium*.

"Mix equal weights of anhydrous ferrocyanide of potassium and carbonate of potassa, and keep the mixture five hours (for 2000 grains of the ferrocyanide) at a good white heat, in a well-closed crucible of hammered iron. The product yields to water a mixed solution of cyanide of potassium, undecomposed carbonate of potassa, and silicate of potassa. The silicic acid may be separated by adding an excess of hydrochloric acid, ignition, and elutriation."

It would be premature at present to criticise Dr. Brown's paper, inasmuch as its accuracy must be established or disproved by experiment, and not by mere reasoning. We regret much that in the reports of it hitherto published no mention is made of the quantity of silicic acid obtained from a given weight of ingredients used; because if the quantity be relatively small, the probability is that the conclusions of the author are erroneous; for the obvious sources of small quantities of silicic acid in the above experiments would be the glass vessel (presumed to be) used in some of the processes, the iron crucible, the carbonate of potassa of commerce, and the ferrocyanide of potassium†.

* Edinburgh Monthly Journal of Medical Science, for June, 1841.

† It is by no means improbable that this salt, as found in commerce, may contain traces of silicic acid, derived from the potashes or iron pot used in its preparation.

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns, June 8, 1841.)

	PRICE.						DUTY and 5 per cent.	DUTY PAID.	
	£	s.	d.	£	s.	d.		In 1841, to last week.	Same time in 1840.
Aloes, Barbadoes, D.P. c	15	0	0	to 30	0	0	{ B P. lb 0 2 } F. lb 0 8 }	64,619	41,649
Hepatic (dry) BD. c	5	0	0	10	0	0			
Cape, BD. c	2	10	0	3	5	0			
Anise, Oil of, German, D.P. lb							F. lb 1 4		521
E. I. lb	0	5	0	0	5	6	E. I. 1 4	447	382
Asafoetida, B.D. c	1	10	0	3	10	0	c 6 0		43
Balsam, Canada, D.P. lb	0	1	0	0	1	3	lb 0 1	989	2,887
Copaiba, BD. lb	0	1	6				c 4 0	227	358
Peru, BD. lb	0	4	6				lb 1 0	412	154
Benzoin (best) BD. c	25	0	0	50	0	0	c 4 0	53	21
Camphor, unrefined, BD. c	11	0	0	12	0	0	c 1 0	362	185
Cantharides, D.P. lb	6	3	3				lb 1 0	6,810	8,249
Caraway, Oil of, D.P. lb	0	8	6	0	8	9	lb 4 0	629	951
Cascarilla or Eleutheria Bark, D.P. c.	3	10	0				lb 0 1	20,017	1,915
Cassia, Oil of, BD. lb	0	10	6				lb 1 4	1,129	1,492
Castor Oil, East India, BD. lb	0	0	2½	0	0	6	c 1 3	{ 3,774	3,351
West I. (bottle) D.P. 1½ lb									
Castoreum, American lb	0	17	0	0	18	0	{ lb 0 6	235	298
D.P. Hudson's Bay lb	0	18	0	1	0	0			
Catechu, BD. Pale c	0	18	6				{ c 1 0	19,527	29,707
Dark c	1	2	0						
Cinchona Bark, Pale (Crown) lb	0	2	0	0	3	6	{ lb 0 1	42,675	22,786
BD. Red lb	0	2	0	0	4	0			
Yellow lb	0	2	4	0	3	0	{ lb 0 2	5,282	2,644
Colocynth, Turkey lb	0	1	6	0	2	9			
D.P. Mogadore lb	0	1	0				lb 0 2	6,677	6,690
Calumba Root, BD. c	0	12	0	1	15	0	lb 0 6	17,278	19,920
Cubebs, BD. c	3	15	0				c 4 0	30	26
Gamboge, BD. c	10	0	0	19	0	0	c 4 0	273	181
Gentian, D.P. c	1	10	0				c 6 0	494	1
Guaiacum, D.P. lb	0	0	6	0	1	0	{ c 6 0	4,132	3,925
Gum Arabic, Turkey, fine, D.P. c	12	0	0	13	0	0			
Do. seconds, D.P. c	7	0	0	7	10	0	{ c 6 0	3,772	2,643
Barbary, brown, BD. c	2	6	0						
Do. white, D.P. c	5	10	0				c 6 0	7,669	8,260
E. I. fine yellow, BD. c	2	5	0	2	14	0	c 6 0	26	104
Do. dark brown, B.D. c	1	15	0	2	5	0	lb 0 1	1,995	1,925
— Senegal garblings, D.P. c	3	5	0				lb 1 0	4,500	3,032
— Tragacanth, D.P. c	8	0	0	12	0	0	lb 0 6	20,372	22,354
Iceland Moss (Lichen), D.P. lb	0	0	2½	0	0	3	{ lb 0 3	6,393	7,522
Ipecacuanha Root, B.D. lb	0	1	6						
Jalap, BD. lb	0	2	0				oz 6 0	501	876
Manna, flaky, BD. lb	0	2	3	0	2	6	{ c 6 0	63	92
Sicilian, BD. lb									
Musk, China, BD. oz	1	0	0	3	10	0	lb 2 6	554	272
Myrrh, East India, BD. c	5	0	0	14	0	0	lb 1 0	11,694	19,220
Turkey, BD. c	2	0	0	11	10	0	lb 4 0	1,136	2,936
Nux Vomica, BD. lb	0	8	0	0	9	0	lb 0 1	139,116	154,750
Opium, Turkey, BD. lb	0	10	6				lb 1 0	12,195	4,740
Peppermint, Oil of, F. BD. lb	0	12	0	0	12	6	{ F. lb 1 0	8,426	11,799
Quicksilver, BD. lb	0	3	11						
Rhubarb, East India, BD. lb	0	5	0	0	7	0	{ lb 1 0	560	1,565
Dutch, trimmed, D.P. lb	0	8	0	0	9	0			
Russian, BD. lb	0	7	6	0	8	6	lb 0 6	62,066	61,169
Saffron, French, BD. lb							{ lb 2 6	3,189	6,328
Spanish lb	1	17	6	2	0	0			
Sarsaparilla, Honduras, BD. lb	0	1	0	0	1	9	{ E. I. lb 0 6	45,551	45,837
Lisbon, BD. lb	0	2	0						
Scammony, Smyrna, D.P. lb							{ Other sorts 0 6	35,921	35,572
Aleppo lb	0	18	0	1	0	0			
Senna, East India, BD. lb	0	0	4	0	0	5			
Alexandria, D.P. lb	0	1	6	0	1	8			
Smyrna, D.P. lb	0	1	0	0	1	3			
Tripoli, D.P. lb	0	1	0	0	1	3			

††† B D. In Bond. — c. Cwt. — B. P. British Possessions. — F. Foreign. — D. P. Duty paid.

IRITIS OCCURRING SOON AFTER
THE EXHIBITION OF A MER-
CURIAL COURSE;

CURED BY A SUBSEQUENT COURSE.

JOHN PATRICK, a seaman, born in England, aged 23 years, admitted into New York Hospital, November 17th, 1838, with phymosis and chancres within the prepuce. The prepuce was split open, blue pills were given internally, and black wash applied to the sores. The mouth became sore, the chancres healed, and the mercurial remedies were discontinued.

December 19th.—The patient was attacked with iritis. Ordered Calomel gr. ij. and Opium gr. $\frac{1}{4}$, every three hours. Emp. Epispast. nuchæ. On the 21st, the gums became sore, and the iritis began rapidly to subside.

25th.—Profuse salivation; excessive soreness and swelling of the mouth; the symptoms of iritis have nearly disappeared. The mouth was directed to be washed with a weak solution of the acetate of lead, and pills of acetate of lead and opium were given internally. In a few days the mouth became well, and there were no traces remaining of iritis.—*New York Journal of Medicine and Surgery.*

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Friday, June 11.

Joshua Barlow.—John Bennet.—Robert Smith Davison.—Henry James Shirley.—Matthewson Corry.—Samuel Adamson Homan.—John Franks Chittenden.—Charles Futford.—Charles Day.—Michael Daniell.—John William Moore Miller.

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

June.	THERMOMETER.		BAROMETER.	
Wednesday 16	from 36 to 68		30.12 to 30.13	
Thursday . 17	43	67	30.05	29.95
Friday . . . 18	41	77	29.81	29.61
Saturday . 19	52	68	29.62	29.65
Sunday . . 20	46	63	29.74	29.65
Monday . . 21	58	67	29.75	29.96
Tuesday . 22	49	67	30.05	30.02

Prevailing wind S.W.

On the 16th clear; the 17th, morning cloudy, otherwise clear; the 18th clear, except the evening; the 19th, morning cloudy, with rain, otherwise clear; the 20th, morning clear, afternoon cloudy, sunshine at times, evening overcast, with rain; the 21st generally clear, frequent showers during the day; the 22d clear.

Thunder Storm.—A storm of thunder and vivid lightning, accompanied with remarkable heavy rain, and hail of an unusual size, from about 7 till 20 minutes past, on the evening of Friday last, the 18th instant.

Rain fallen, 755 of an inch, of which 415 fell during the thunder storm of the 18th.

CHARLES HENRY ADAMS.

TESTIMONIAL TO DR. LONSDALE,
DEMONSTRATOR OF ANATOMY, QUEEN'S
COLLEGE, EDINBURGH.

ON Friday, the 18th ult., a meeting of the students of Queen's College was held in the Class-room, Argyle Square, when Mr. J. M. Adams, surgeon, presented, in name of the Anatomical Students, an elegant and valuable microscope, by Chevalier, of Paris, to Dr. H. Lonsdale.

The gift was accompanied by an address, signed by nearly 100 gentlemen who had attended the Doctor's prelections during the Winter Session, testifying to his zeal and abilities as a teacher, and gratefully acknowledging the uniform kindness and attention which they had experienced from him.

A TABLE OF MORTALITY FOR THE
METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 12th June, 1841.

Small Pox	24
Measles	18
Scarlatina	4
Hooping Cough	28
Croup	15
Thrush	8
Diarrhœa	7
Dysentery	1
Cholera	0
Influenza	2
Typhus	29
Erysipelas	5
Syphilis	1
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	121
Diseases of the Lungs, and other Organs of Respiration	283
Diseases of the Heart and Blood-vessels	13
Diseases of the Stomach, Liver, and other Organs of Digestion	69
Diseases of the Kidneys, &c.....	4
Childbed	4
Ovarian Dropsy	0
Diseases of Uterus, &c.	4
Rheumatism	3
Diseases of Joints, &c.	1
Ulcer	0
Fistula	0
Diseases of Skin, &c	2
Diseases of Uncertain Seat	90
Old Age or Natural Decay	47
Deaths by Violence, Privation, or Intemperance	39
Causes not specified	4
Deaths from all Causes	826

NOTICES.

"FIAT JUSTITIA" cannot be inserted except with the real signature of the writer.

We shall be happy to have Dr. P. B.'s paper. We should have answered him privately, but that no address was given.

We cannot insert the letter of "A Provincial Physician" unless he favours us with his name.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, JULY 2, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XL.
Concluded.

INTERMITTENT FEVER—*continued.*

Exciting cause. Malaria; known only by its effects; places which it chiefly infests; conditions of its production; its effects upon the human body; influence of soil upon its evolvment.

THE exciting cause of intermittent and remittent fevers—the primary exciting cause I mean, that without which ague would never occur at all—deserves a somewhat particular consideration. I need scarcely say that it consists in certain invisible effluvia or emanations from the surface of the earth, which were formerly called marsh miasmata, but to which it has, of late years, become fashionable to apply the foreign term *malaria*. In some respects the latter designation is the more convenient of the two.

The malaria is a specific poison, producing specific effects upon the human body. In its medical sense, it is not simply bad air, or impure air, although the word is loosely employed by many to express any mixed kind of contamination of the atmosphere. Thus we hear of the malaria of London: but ague, even when it occurs in London, is very seldom, indeed, now-a-days, of London growth. The impure air incident to large and populous cities is prejudicial enough to health, as I formerly took occasion to shew you: but it does not *generate* fever: neither continued fever, nor intermittent.

The emanations which cause ague have
709.—XXVIII.

been called marsh miasmata, because they are notoriously common in marshy places. But they are not peculiar to marshy places. For this reason, and for brevity's sake, I prefer using the single word *malaria*. In this country, thank God, we witness its milder evils only, and those not very often: but it is the bane and scourge of large portions of the world. Whether you practise here or abroad, it is very fit that you should know the qualities, habitats, and habits, of this wide-spread poison. The mildest form of fever to which it gives birth is the intermittent fever, or ague: but in climates and places where it exists in greater abundance and intensity, the fever becomes remittent, or even assumes the continued form. This has led to strange errors, and proved a fertile source of difference and controversy amongst medical men; not a few of whom confound the severe continued fevers which spring from the malaria, and which are never contagious; with the severe continued fevers usually called typhous, which are unquestionably communicable from person to person.

The effluvia which thus form the sole exciting cause of intermittent and remittent fevers proceed from the surface of the earth, and are, probably, gaseous, or aëriform: at any rate they are involved in the atmosphere. But they are imperceptible by any of our senses. Of their physical or chemical qualities we really know nothing. We are made aware of their existence only by their noxious effects; and the inference that they exist was not made till within the last century and a half. Time out of mind, indeed, it had been matter of common observation that the inhabitants of wet and marshy situations were especially subject to these definite and unequivocal forms of disease. But the Italian physician, Lancisi, was the first, so far as I know, to put forth distinct ideas concerning malaria, in his book, published about 1695, *De noxiis paludum effluviis*.

This is the great original work upon the subject.

To the production of this deleterious agent, a certain degree of temperature seems necessary. It does not appear to exist within the arctic circle: nor does it manifest itself during the colder seasons of more temperate climates. It is very seldom traceable beyond the 56th degree of north latitude; and it is supposed to require for its development a continuous temperature higher than 60° of Fahrenheit's thermometer. The nearer we approach the equator, the more abundant, virulent, and pernicious does the poison become, wherever it is evolved at all. In this climate it gives rise to intermittents, and principally to tertians. As we go south, in Spain, and along the shores of the Mediterranean, the remittent becomes the predominant form; and (what is very instructive) remittents there contracted often improve into intermittents upon the removal of the patient to a colder climate. Under the tropical heats, in the West Indies for example, the fevers very frequently assume the continued form.

And another condition of the development of the poison soon becomes apparent. It requires a certain degree of moisture. Of all these regions, malaria, showing itself always by its effects alone, infests certain parts only; which parts are, most generally, remarkable for their humid and swampy character. Thus, in this island, intermittents are produced chiefly, I may say almost exclusively, along the eastern coast; in parts of Kent, Essex, Cambridgeshire, Norfolk, Lincolnshire, and the East Riding of Yorkshire: and in each of these counties there are marshes, or fens, or low grounds and lands that are occasionally overflowed. Many of these spots have, within the last fifty years, been drained, and brought under cultivation; and agues are consequently much more rare in England than they formerly were. In Sydenham's time they were very frequent, and very fatal indeed, in this metropolis. James the first, and Oliver Cromwell, both died of ague contracted in London. At present (as I said before) we seldom meet with them. Except in the year 1827, I have never, since I have been in practice, known ague to be at all common here. This comparative freedom from malaria is mainly owing, no doubt, to the improved character of the draining and sewerage.

Agues, or aguish fevers, are endemic along every part of the low and level coast of Holland. In Italy, the Pontine marshes, near Rome, have possessed for ages an infamous celebrity of the same kind. The whole of the district called the Maremma, which stretches for about thirty leagues along the shores of the Mediterranean, and which is in some places ten or twelve leagues broad,

is rendered dangerous, and almost uninhabitable, by the vast quantity of malaria annually evolved from its soil. In America large districts are, for the same reason, prolific of disease: and the late Bishop Heber, in his *Narrative of a Journey through the Upper Provinces of India*, gives the following striking picture of the influence of the malaria in that part of the world. It seems to be alike pestiferous to man and beast.

"I asked Mr. Boulderson if it were true that the monkeys forsook these woods during the unwholesome months. He answered that not the monkeys only, but every thing which has the breath of life, instinctively deserts them from the beginning of April to October. The tigers go up to the hills; the antelopes and wild hogs make incursions into the cultivated plain; and those persons, such as dâk-bearers, or military officers, who are obliged to traverse the forest in the intervening months, agree that not so much as a bird can be heard or seen in the frightful solitude. Yet, during the time of the heaviest rains, while the water falls in torrents, and the cloudy sky tends to prevent evaporation from the ground, the forest may be passed with tolerable safety. *It is in the extreme heat, and immediately after the rains have ceased*, in May, the latter end of August, and the early part of September, *that it is most deadly*. In October the animals return. By the latter end of that month the woodcutters, and the cow-men, again venture, though cautiously. From the middle of November to March troops pass and repass, and with common precaution no risk is usually apprehended."

Persons who live in England might perhaps be disposed to think lightly of the malaria, had not such fearful evidence of its appalling power been brought home to the experience of our countrymen, in the early part of the present century, by the result of the unfortunate expedition to Walcheren. Sir Gilbert Blane has given an account of the ravages it there committed among our troops. You may see his paper, to which I shall presently again refer, in the third volume of the *Medico-Chirurgical Transactions*.

Not only a certain degree of heat, and a certain quantity of moisture, but the presence of all the four elements of the ancients, would appear to be requisite for the production of this poison. Air of course there must be; and earth also is essential. If heat and moisture were alone adequate, we should find the fever prevailing among sailors when out at sea: but it is not so, whatever may be the temperature under which they cruise. It is when they approach the coast, or land upon it, that they are attacked. The water of marshes has been examined under the microscope, and analyzed again and again, with a view to the discovery of the nature of this

pestilential agent; but in vain. A more likely way to detect the noxious material would seem to be by examining the *air* of malarious districts: and this has been done carefully and repeatedly by expert chemists; and with the same want of success. The poisonous principle eludes the test of the most delicate chemical agents.

Where there is much heat, and much moisture, there we usually find also much and rank vegetation, and much vegetable dissolution and decay. The belief was as natural, therefore, as it has been general, that the putrefaction of vegetable matters was somehow or other requisite to the formation of the poison that exists so commonly in swampy situations. This belief has descended, almost unquestioned, from the time of Lancisi; and it obtains almost universal acceptance, I fancy, among physicians of the present day. Yet very strong facts have been adduced to show that the decomposition of vegetable substances is only an accidental, though a frequent, *accompaniment* of the miasm; and not by any means an essential condition of its evolution.

In the first place, the decomposition of vegetable matter goes on abundantly without the production of malaria. The rotting cabbage-leaves of Covent Garden, and those which taint the air of the streets from the neglected dust-holes of London, during the hot weather of summer, give rise to no ague. The same may be said of the putrefying and offensive sea-weed, which is deposited in large quantities upon some very healthy parts of our sea coast. But the converse facts are the most remarkable and conclusive. I have stated that *marshes* are not necessary to produce malaria: but Dr. William Ferguson—a physician who has had, and who has well used, very sufficient opportunities of investigating the question—shews that *vegetation* is not necessary: that the peculiar poison may abound where there is no decaying vegetable matter, and no vegetable matter to decay. As the prevailing belief is, in my opinion, an erroneous one, and as it is really of great importance that correct views of this subject should be taken, and disseminated by medical men, I will mention a few of the most striking of the facts detailed by Dr. Ferguson. They are contained in a very interesting paper “*On the Nature and History of the Marsh Poison*,” published in the *Edinburgh Philosophical Transactions*.

In August 1794, after a very hot and dry summer, our army in Holland encamped at Rosendaal and Oosterhout. The soil, in both places, was a level plain of sand, with perfectly dry surface, where no vegetation existed, or *could* exist, but stunted heath plants. It was universally percolated to within a few inches of the surface, with water which, so far from being putrid, was

perfectly potable. Here fevers of the intermittent and remittent type appeared among the troops in great abundance. It is interesting to observe that the soil in Walcheren is precisely similar. Sir Gilbert Blane describes it as consisting “of a fine white sand, known in the eastern counties of England by the name of silt, and about a third part of clay.” It was after a hot and dry summer also that the British army suffered in that island from the endemic fever, to a degree which Dr. Ferguson speaks of as “being almost unprecedented in the annals of warfare.”

In the year 1809, several regiments of our army in Spain took up an encampment in a hilly ravine which had lately been a water-course. Pools of water still remained here and there among the rocks, so pure that the soldiers were anxious to bivouack near them for the sake of using the water. Several of the men were seized with violent remitting fever before they could move from the bivouack the next morning. “Till then (says Dr. Ferguson) it had always been believed amongst us that vegetable putrefaction (the humid decay of vegetables) was essential to the production of pestiferous miasmata; but in the instance of the half-dried ravine before us, from the stony bed of which (as soil never could lie for the torrents) the very existence even of vegetation was impossible, it proved as pestiferous as the bed of a fen.”

After the battle of Talavera, the army retreated along the course of the Guadiana river, into the plains of Estremadura. The country was so arid and dry for want of rain, that the Guadiana itself, and all the smaller streams, had in fact *ceased to be streams*, and were no more than lines of detached pools in the courses that had formerly been rivers. The troops there “suffered from remittent fevers of such destructive malignity, that the enemy, and all Europe, believed that the British host was extirpated.”

Civdad Rodrigo is situated on a rocky bank of the river Agueda, a remarkably clear stream: but the approach to it on the side of Portugal is through a bare open hollow country, that has been likened to the dried-up bed of an extensive lake; and upon more than one occasion, when this low land, after having been flooded in the rainy season, had become as dry as a brick-ground, with the vegetation utterly lured up, there arose fevers to our troops, which, for malignity of type, could only be matched by those before mentioned on the Guadiana.

Many more facts to the same purpose are related in Dr. Ferguson’s paper, which is in every way well worth your perusal. He tells us “that in the most unhealthy parts of Spain, we may in vain, towards the close of the summer, look for lakes, marshes,

ditches, pools, or even vegetation. Spain, generally speaking, is then, though as prolific of endemic fever as Walcheren, beyond all doubt one of the driest countries of Europe; and it is not till it has again been made one of the wettest, by the periodical rains, with its vegetation and aquatic weeds restored, that it can be called healthy, or even habitable with any degree of safety."

Our time will not allow of my extracting any farther evidence on this point: one circumstance of contrast, however, I am unwilling to omit.

The river Tagus is, at Lisbon, about two miles broad; and it separates a healthy from a very unhealthy region. On the one side is a bare hilly country; the foundation of the soil, and of the beds of the streams, being rock, with free open water-courses among the hills. This is the healthy side. But the Alentejo land, on the other side, though as dry superficially, being perfectly flat and sandy, is most pestiferous. Moreover, in and near Lisbon there are numerous gardens, where they keep water, during the three months' absolute drought of the summer season, in stone reservoirs. These reservoirs, containing water in the most concentrated state of foulness and putridity, are placed close to the houses and sleeping rooms: the inhabitants literally live and breathe in their atmosphere. "Yet no one ever heard or dreamt of fever being generated amongst them from such a source; though the most ignorant native is well aware that were he only to cross the river, and sleep on the sandy shores of the Alentejo, where a *particle* of water at that season had not been seen for *months*, and where water, being absorbed into the sand as soon as it fell, was *never* known to be *putrid*, he would run the greatest risk of being seized with remittent fever."

Now these facts, and facts like these, seem to prove that the malaria, and the product of vegetable decomposition, are two distinct things. They are often in company with each other, but they have no necessary connexion. Whoever, in a malarious country, waits for the evidence of putrefaction, will wait, says Dr. Ferguson, too long. For producing malaria it appears to be requisite that there should be a surface capable of absorbing moisture, and that this surface should be flooded and soaked with water, and then dried: and the higher the temperature, and the quicker the drying process, the more plentiful and the more virulent (more virulent probably because more plentiful) is the poison that is evolved.

The putrefaction of *animal* matter is sometimes spoken of as an element in the formation of the malarious poison. But the evidence I have just set before you refutes his supposition as completely as it excludes

the alleged necessity of vegetable decay. I hope to prove to you, in a future part of the course, that neither animal nor vegetable decomposition is sufficient to generate fever of any kind.

Dr. Ferguson's facts are generally in accordance with the observations which others have made upon the same subject: and his views will be found to account for some phænomena which the ordinary theory of vegetable putrefaction did not cleverly explain.

There is good reason for believing that in all cases the poisonous emanations proceed from parts of the surface that have been flooded and then dried, rather than from parts that are still wet, or putrid. And this elucidates a circumstance very often noticed, viz. that neighbouring places—especially high and low lands lying near each other—change their character in respect to salubrity upon the occurrence of rains. The low grounds, which had previously been very dangerous, become healthy when they are flooded over; and the higher lands, which are made wet, and which rapidly dry again, produce the malaria abundantly. For the same reason, the edges or borders of swamps, which of course expand or contract according to the wetness or dryness of the season, are more unsafe than their centres. The drying and half-dried margins of the purest streams may be prolific of the evil, when, from the want of confining banks, those margins have been flooded by the rising of the waters.

There is no observation more general than that, in malarious places, agues and remittent fevers abound more in hot and dry years than in those which are cold and moist. And this influence of temperature it is which mainly determines the differences observable in regard to these fevers at *different elevations*, and in *different seasons* of the year. In the higher grounds of the West Indies *agues* occur, as in this country: as you descend, and the mean atmospheric temperature increases, *remittents* are met with: and in the lowest and hottest parts the fever becomes *continued*. The following instructive facts are stated by Dr. Ferguson. In 1816, the British garrison of English harbour, in Antigua, was disposed in three separate barracks, on fortified hills surrounding the dock-yard. One of the barracks was on an eminence named Monk's Hill, six hundred feet above the level of the marshes. The other two were situate on an eminence called the ridge, one at the height of five hundred, and the other at the height of three hundred feet. So pestiferous were the marshes among which the dock-yard was placed, that it often happened to a well-seasoned soldier, coming down from Monk's Hill, and mounting the night-guard in perfect health, to be seized

with furious delirium while standing sentry, and to expire within less than thirty hours after being carried up to his barracks, with a yellow skin, and having had black vomiting. Those in the barracks on Monk's hill, *who did not come down*, the superior officers, the women, children, and drummers, had no fever of any kind. Seventeen artillery men, in the barrack at the height of three hundred feet, did not come down to the night guards. (We shall see hereafter that malarious places are always most dangerous at *night*.) Every one of these men was attacked with remittent fever, of which one of them died. At the barrack on the top of the ridge, at the height of five hundred feet, there scarcely occurred any fever worthy of notice. Thus, *in the same place*, the malaria, in the level plain, caused continued fever, resembling, and I believe identical with, yellow fever: at the elevation of three hundred feet it gave rise to remittent fever: and at the height of five hundred or six hundred feet its influence was scarcely felt at all. In the neighbourhood of the Pontine marshes you see the villages perched curiously on the intervening hills; the Italians having been taught by experience that these elevated spots afford comparative security against the effects of the miasmata.

Wherever the malaria prevails, it produces its peculiar consequences chiefly in certain seasons: and it is in the autumn especially that agues and aguish fevers occur; that is to say, after the heats of summer: and the hotter and drier the preceding summer, the more frequent and fatal are the autumnal fevers. The Pontine marshes lie to the southward of Rome; and Horace, you know, says or sings,

*Frustra per autumnos nocentem
Corporibus metuemus austrum.*

The effects of these morbid effluvia upon the human body vary much under different circumstances. Where they are most concentrated and deadly, their operation may be almost immediate. Witness their speedy influence upon the soldiers who descended at night from Monk's Hill. So also sailors, who have gone on shore for a single night only, have been attacked by the fever before they could return to the ship. On the other hand, when the emanations are less copious, or less intense, there is sometimes a long and uncertain period of incubation. The disease remains latent, or the poison lies dormant, for a considerable space of time. Many of the soldiers who were exposed to the malaria at Walcheren did not experience its bad effects until after they had returned, and even resided several months in England. In the same way, labourers, especially the itinerant Irish, will go down in the autumn for harvest work into Lincolnshire, and

bring back the seeds of the disorder within them, and yet may not be attacked with ague for weeks or months; upon the occurrence of an east wind, perhaps, or unusual exposure to cold and wet. We trace in all this some analogy with the animal contagions; but the period of incubation is more irregular and accidental, and it is probable that in many instances the ague would not happen at all, but for the concurrent operation of some other malign influence.

Another fact worthy of notice in respect to the agency of the malaria upon the human frame, is that it affects strangers much more readily and decidedly than the natives of the place. In other words, habit mitigates the injurious effects of the poison. Persons become *seasoned* to it. At Walcheren, though almost every adult among the lower classes had laboured, in the course of his life, under the endemic intermittent, yet they were infinitely less subject to it than strangers: and they will not believe that their beloved birth-place is unhealthy. Sir Gilbert Blane says that persons of education, and even medical men, denied indignantly that their country was less healthy than any other; and attributed the sickness that raged among our troops to some trivial circumstance of diet or habits, and not to any insalubrity of the air. This is a curious moral feature; but a very general one. In the pestilential plains of Estremadura the superstitious natives, unable or unwilling to account for disease of a type so uncommon among the soldiers, from any unwholesomeness of the air, declared that they had all been poisoned by eating mushrooms.

It was found also, at Walcheren, that the strangers who survived the first attacks became thereafter much less liable to the endemic fevers. The French general, Monnet, who had held the command at Flushing for seven years, had acquired a knowledge of this fact, and endeavoured to turn it to practical account. He recommended that troops should not be frequently changed; for when it was the custom to send battalions from Bergen op Zoom every fourth night, in succession, to work on the lines of Flushing, these men never failed, on their return, to be taken ill in great numbers. General Monnet therefore advised, however displeasing it might be to the officers, that a stationary garrison should be retained at Walcheren, in order that the men might be habituated or seasoned to the air, (*acclimatés*) and he adduced the instance of a French regiment which suffered in the second year of its being stationed there only one half the sickness and mortality which it suffered during the first year; and hardly suffered at all in the third year.

But although the natives and residents in malarious places are not so liable as new-

comers to the violent and distinct forms of fever, they are chronically affected by the insalubrity of the atmosphere. They are spoken of by travellers as being puny, yellow, and sickly; feeble in body and spiritless in mind; as having yellow faces, swelled bellies, and wasted limbs; as being subject to dropsies and fluxes; phlegmatic, melancholy, and shortlived.

One remarkable exception is mentioned by Dr. Ferguson. From some peculiarity of idiosyncrasy (which he conjectures may be somehow connected with the texture of his skin) the negro appears to be proof against endemic fevers. "To him marsh miasmata are in fact no poison; and hence his incalculable value as a soldier, for field service, in the West Indies. The warm, moist, low, and leeward situations where these pernicious exhalations are generated and concentrated, prove to *him* congenial. He delights in them, for there he enjoys life and health, as much as his feelings are abhorrent to the currents of wind that sweep the mountain tops, where alone the whites find security against endemic fevers."

No very certain or extensive observations have yet been made in respect to the *kind of soil* from which the miasmata are most apt to be extricated. That which is loose, penetrable, porous, and sandy, appears highly favourable to their formation. So are soils which, containing much clay, are very retentive of moisture. One curious fact, however, bearing upon this question, seems to have been made out; viz. that what is termed peat-bog, or peat-moss, is not productive of malaria. Many parts of Scotland and of Ireland, that are occupied by large tracts of marsh in which the peat-moss abounds, are completely free from these fevers. Dr. Bisset affirms that the exhalations from black peat-moss do not occasion intermittents, "at least in high moors under a clear sharp air." Now in the climate of Virginia, this counteracting influence of a sharp air can scarcely be looked for: yet it is a remarkable fact, that though the provinces of North America, especially North and South Carolina and Virginia, are full of ague, that disease is never seen among the inhabitants near the country of the *Dismal Swamp*, a moist tract of 150,000 acres on the frontiers of Virginia and North Carolina. Weld, the traveller, informs us, that this immense tract is covered with trees, and abounds with water, which appears the moment the shallowest trench is dug. This water is brown, like brandy, but quite clear, and not unpalatable. The colour is ascribed by the inhabitants to the roots of juniper; and the water is said to be diuretic. (*Craigie*).

CASES OF HEPATIC ABSCESS, IN WHICH EXPLORATION AND PUNCTURE WERE ADOPTED;

With Clinical Remarks by DR. MURRAY,
Inspector of Hospitals, Calcutta.

1st.—Case of abscess of the liver, punctured.
By Dr. Mortimer, Surgeon of the General Hospital.

THE subject of the case was a seaman, aged 39, who had been about two years in India. He was admitted with the usual symptoms of acute hepatitis of some days' standing, which had, by account, succeeded an attack of bowel complaint.

The more urgent symptoms, after a time, appeared to yield in a great measure to the usual treatment; but the mercury used, although it affected his mouth, did not produce ptyalism, nor did he convalesce as he would have done had the disease been overcome.

The constitutional symptoms generally considered as indicative of abscess, although observable, were rather obscurely marked; and notwithstanding a good deal of general fulness, extending from one hypochondrium to the other, there was no particular spot in which local examination afforded unequivocal evidence of the existence of matter.

By the new exploring needle, however, having been introduced at two inches in a right line from the junction of the cartilages of the 7th and 8th ribs, a few drops of purulent matter were discharged through the tube of the instrument at its handle. The external opening was then enlarged, and a common trocar introduced, through the canula of which about eight ounces of very thick healthy pus were evacuated.

The patient expressed himself considerably relieved by the operation, but this was the only benefit derived from it; and the case terminated fatally two days afterwards.

Post-mortem appearances.—On opening the abdomen, the liver was found stretching entirely across, and occupying both hypochondria: the stomach being altogether concealed and pushed backwards by the left lobe. There was no adhesion of the liver anteriorly to the peritoneal lining of the abdomen, excepting for about a quarter of an inch around the puncture, where there appeared a recent deposit of lymph.

It was found that the purulent matter discharged through the opening made before death had been contained in an abscess situated nearly in the middle of the right lobe. The size of the cavity appeared to have contracted considerably, and was separated by a septum of parenchymatous substance of about half an inch in thickness, from another and much larger abscess situated superiorly, and occupying the greater part of the upper

and convex portion of the right lobe, which had pushed the diaphragm considerably upwards. There was another circumscribed abscess, larger than either of the other two, situated below and partly behind that first noticed, occupying the whole of the inferior part of the lobe. The contents of the last appeared to be retained within their cavity at the lower boundary merely by the peritoneal covering of the viscus, somewhat thickened, and adhering strongly to the contiguous viscera and upper part of the right kidney, which were all so united together that they could not be separated. There was also an abscess in the middle of the left lobe of about the same size as the one first mentioned.

All the abscesses contained thick healthy-looking purulent matter.

2d.—Three cases of hepatic abscess, in which the exploratory needle was used. By Dr. Mouat, 13th Light Dragoons.

Sergeant Damon Derrick, 4th (King's own) regiment; in India two years. Was attacked with acute hepatitis in the early part of last August, (1839), for which he was bled three times, had leeches to his side twice, three blisters, calomel, purgatives, &c. but with little relief. The pain continued, though in a mitigated degree, and the side swelled, accompanied with colliquative stools and sweats, and every sign of hepatic abscess, when it was determined to puncture the liver.

On the 11th of August the exploratory needle was introduced posteriorly at the angle, and between the 7th and 8th ribs, to the depth of two inches and a half, but without detecting pus. However, we found no inconvenience ensue from the puncture, and the second day after the operation the abscess apparently burst into the intestines, and he gradually recovered from that period. He never experienced any bad symptom from the introduction of the exploratory needle; he thinks, on the contrary, that it rather did him good.

Other two men of the same regiment, private Jessy Austen and William Leg, were operated on about the same time in a similar manner, and experienced no bad consequences from the introduction of the needle. Leg was twice punctured: the first operation failed to reach the cyst; but the second puncture did, when a trocar was afterwards introduced, and a large abscess evacuated. He was better for some days, but then became worse, with an increase of hectic symptoms, and he died. On dissection it was found he had other abscesses:—the first puncture had healed, and the second, leading to the cyst of the abscess, had united it to the pleura. So that, as far as the operation was concerned, it was safe, and unattended with a single untoward consequence.

The same also may be said of Austen.

His liver was punctured in two places without finding matter, yet he died with abscess in both lobes; when it was found that the punctures from the exploratory needle had healed, without producing any bad effect.

I was consulted in all these three cases, and I am fully satisfied as to the safety of puncturing the substance of the liver by the exploratory needle, which, I believe, I was the first to use and bring into notice at Bangalore*.

3d.—Summary of the case of private Andrew Killagher, His Majesty's 39th Regiment. By Assist.-Surgeon M'Grigor.

This man, æt. 33, sixteen years in the service, of turbulent character and irregular habits, was admitted into hospital at Bellary, in the beginning of July 1839, with a *relapse* of acute hepatitis, and put under active antiphlogistic treatment, without its affecting the arrest of the disease. At the end of a fortnight he still complained of pain and weight in the epigastrium and right side; and, along with this, he had severe constitutional fever, which had reduced him so much that he lay supine, and seemed scarcely able to speak. Pulse 104; bowels irregular.

On the 22d of July I called Dr. Davidson, the superintending surgeon, into consultation, when it was agreed to puncture the liver, to which the patient willingly consented, from the prospect of its affording him relief.

I entered the trocar near the margin of the ribs, and close also to the ensiform cartilage; and as I continued to feel resistance, I pushed it in to its whole length; but on withdrawing the stilette blood only came out.

Much to our satisfaction, however, the patient expressed himself greatly relieved by the operation, and he continued from that time to improve.

I left the canula in the wound till the following night, with a view to prevent the escape of blood into the cavity of the peritoneum, and excite adhesion between this membrane and the liver; when, there being much pain around the puncture, and a feeling of stiffness of the abdomen, it was removed.

The wounded vessels in the liver had ceased to bleed; and there was no further hæmorrhage. The abdomen was fomented, a dose of morphia given, and the patient afterwards passed a very fine night.

Next morning we found him in raptures at the relief he had obtained. He had no pain nor uneasiness; but he had got a slight diarrhoea. Pulse 90.

* Though the operation of exploring deep seated abscesses had been previously practised in England, to Dr. Mouat is due the credit of introducing and recommending the practice in this presidency, and we believe in India.

By the 28th his improvement was so great that all were astonished. He said himself he was sure he would have died but for the operation; and certainly at the time I introduced the trocar I thought him in a very bad way.

Common simple treatment, chiefly alterative, aperient and dietetic, only was employed afterwards, and he gradually improved, but did not get quite well. His liver remained enlarged and heavy, his feet became slightly œdematous, and he was transferred to the Dépôt, at Poonamallee, as an invalid, from whence he was sent to England for change of climate.

4th.—*Case of Hepatic Abscess cured by early puncture.*—By Dr. Everard, Asst. Surg. H. M. 54th Regiment.

Private James M'Eldoon, H. M. 54th Regt. æt. 38; twelve years in India; generally healthy, but lately of intemperate habits, was admitted into hospital 10th November, 1839, complaining of severe pain in the right hypochondrium, increased by inspiration or pressure, which he says came on three days ago, and has gradually increased since. Has severe headache; pulse full; skin hot; bowels open; urine high coloured.

Ft. V. S. ad effectum; R. Calomel gr. v.; Pulv. Jalap ʒiss.; spoon diet.

[Various remedies, chiefly mercury, leeches, and purging, employed.]

20th.—Swelling more prominent, with feeling of fluctuation. There is evidently no adhesion between the liver and abdominal parietes. He is emaciated, and has sense of weight and oppression in the epigastrium.

Contin. Cataplasms.

22d.—The Deputy Inspector (Dr. Murray) visited the hospital this morning, and after examination of this man, thrust a trocar into his liver through the epigastrium, without waiting to make any preparatory operation to induce adhesion between the parts. About a pint and a half of thin brown matter came out on withdrawing the stilette, and the canula was left in the wound. The man looks pale, and has become weak. Pulse soft; skin cool; bowels freely open.

The canula was tied in by a bandage round the body, a bit of lint put into the orifice, and a large warm poultice over all. To have some light fish for dinner, and half a pint of beer; with tea and toast.

Vespere.—Has got great relief from the operation. Matter oozes out from the abscess by the canula.

R. Acet. Morphiae, gr. ss. h.s.s.

23d.—Had a good night—doing well.

Subsequent History.—For a few days after the operation, about two ounces of unhealthy greenish bilious matter came away,

after which the discharge became gradually more healthy (occasionally bilious) and less; but did not entirely cease till the 19th February following. As the liver continued enlarged and hard after the evacuation of the abscess, hydriodate of potash was given with advantage.

At present (10th March) his appetite and general health are good; he sleeps well, and he has recovered his strength. He has no uneasiness in the side, though the liver is attached to the abdominal parietes, where it is punctured, and feels somewhat enlarged. There was never any effusion of matter into the cavity of the peritonæum, as adhesion soon took place after the puncture. The operation is considered to have saved this patient's life.

5th.—*Abstract of a case of Hepatic Abscess, explored and punctured.*—By Dr. Maclean.

Private John Gorman, H. M. 55th Regiment, aged 33, was admitted into the General Hospital under Dr. Mortimer on the 8th of February, 1840, with fever, cough, difficulty of breathing, mucous expectoration, and daily rigors at noon. He had been ill for four days previously to admission, and ascribed the cause of his illness to cold caught while on guard at night.

He did not complain particularly of uneasiness in the hepatic region or epigastrium till the 21st of February, when a general fulness in the latter was observable. On the 22d he felt very weak. On the 25th the fulness of the epigastrium and oppression of breathing were increased, and his countenance became anxious; pulse 84. On the 27th it is reported that he had again chills for several nights, followed by free perspiration; pulse 100. On the 29th he was transferred to the 54th Hospital under Dr. Everard. On the 2d of March his liver was explored and punctured by Dr. Murray, who made the following memorandum at the time on the case.

Being informed by Dr. Everard that he had got a patient in a dangerous state transferred to his hospital, suffering from pectoral symptoms, with obscure manifestations of suppuration in the liver, I went to consult with him on the case.

I found the liver extending nearly three inches into the epigastrium towards the umbilicus, tender to the touch, but not so much as to preclude examination. The right rectus muscle was more tense than the left, or rather it became so on attempting to examine the tumor, as if to screen it from pressure, which Mr. Twining gives as a characteristic symptom of central abscess of the liver. The patient had many shivering fits about three weeks ago, and now has profuse cold perspirations at night (*hectic fever*),

with a considerable puriform deposit in the urine; but there is no fluctuation perceptible in the enlarged viscus. Decubitus on the back or left side is extremely oppressive. He has a frequent tickling cough, and great difficulty of breathing, with sense of weight in the hepatic region; his legs and body are œdematous; he has no appetite; great thirst; pulse 120 and intermittent; prostration of strength great.

Finding him suffering so much, and evidently in a very dangerous way, and considering the general as well as local symptoms decidedly indicative of existing suppuration, I pushed a trocar into the liver where it protruded into the epigastrium; but only a little blood flowed on withdrawing the stilette. Not satisfied with this exploration, I pushed the new exploratory instrument into the liver behind the middle of the side, between the eighth and ninth ribs, when, to our satisfaction, pus flowed; not, however, through the tube of the instrument, but by the side of it, apparently from my having gone beyond the abscess.

I then withdrew the explorer, and introduced a large sized flat trocar, by which eight or nine ounces of thick curdy pus were evacuated. When the evacuation was nearly completed, a gurgling of air took place through the canula, apparently from the action of the diaphragm, and a cork was then fitted to the canula, (which was retained *in situ*) with directions to take it out at mid-day and in the evening, to allow accumulated matter to escape. A bit of sticking-plaster was applied over the orifice of the first puncture in the epigastrium.

After this, the patient's breathing, and his decubitus on the back, were somewhat relieved; and he said he felt altogether better. He complained of thirst, for which he was ordered small quantities of lemonade, and the following mixture:—

℞ Liq. Ammon. Acet. ℥v.; Spt. Æther Nit. ℥ij.; Syrup Simpl. ℥ij.; Aquæ ℥xxx. (M.—℥ij. 3tiâ q. q. horâ.)

Although his bowels were free, he was ordered a purging enema in the evening; and a grain of Acet. Morph. at bed-time. A large poultice to be applied over the hepatic region and side. Spoon diet; and congee.

3d March, *Mane*.—Says he did not sleep, and that he has not slept for many nights, but that he feels better this morning. The canula was withdrawn last night, and a tent of lint introduced instead. A little thick matter is discharged at each dressing. The urine is now clear. Pulse 100; skin warm and moist; bowels open; thirst less.

Contin. mistura. To have an egg and one pint of beer, with spoon diet.

Vespere.—Slept a good deal during the

day; breathing easier; has no uneasiness in the site of the puncture in epigastrium; pulse 100; skin less clammy.

5th.—Did not rest so well. No discharge from the side this morning. The urine again deposits a thick yellow sediment; great depression and anxiety; pulse 120, and rather full; skin clammy. Dr. Murray made another exploratory puncture into the liver on the right side of the epigastrium, without finding pus; but a quantity of serous fluid was evacuated from the cavity of the abdomen, on withdrawing the canula out of the liver.

The side to be well fomented, and afterwards poulticed.

℞ Enema; R. Tinct. Opii; Tinct. Hyosciami, aa. M. L.; Aq. Puræ j. h. s. s.

He gradually sank, and died on the 13th.

Dissection, seven hours after death.—Previously to opening the body, the exploratory instrument was introduced into the liver, near the end of the eleventh (floating) rib, when thin yellow pus issued freely through its canal, shewing it had entered an abscess.

œdematous swelling of the hands, feet, and ancles, with emaciation of the arms, legs, and thighs, abdominal enlargement, distension of the right hypochondrium, bulging of the ribs of the right side, and an ulcerative sloughy state of the wound, constituted the external appearances.

On dividing and turning back the abdominal parietes, it was found that firm adhesion, of recent formation, had taken place between it and the liver, where the two punctures were made in the epigastric region; and attentive examination could not detect any mark of inflammation in the substance of the gland around the points punctured: the cicatrization of it was perfect. The right lobe of the liver extended (was apparently pushed down) to within an inch of the crest of the ileum and umbilicus; and the left lobe nearly reached to the spleen.

It was found that the exploratory instrument which before dissection was pushed into the liver, had entered a large distinct abscess situated in the right side of the concave surface of the gland, which had very narrowly escaped being penetrated in the exploration made on the 5th instant. Its area was considerably larger than a man's fist, and it contained upwards of a pint and a half of thick yellow greenish pus.

Immediately above this abscess was the empty contracted sac of the one opened and evacuated on the 2d of March: the inner surface of it had a dark gangrenous appearance, which extended throughout the course of the wound.

At the centre of the upper convex part of

the liver was a third distinct abscess, *the largest of all*, containing nearly three pints of matter, which seemed not only the chief cause of the projection of the organ beyond the ribs, by its pushing it downwards, but also of the projection of the diaphragm into the right cavity of the chest: it pushed the diaphragm as high up as the fourth rib. The upper part of the walls of this abscess adhered extensively to the diaphragm; as did those of the lowest abscess to the cellular substance and other parts above the right kidney.

There was no adhesion between the liver and the colon or stomach.

The anterior part of the right lobe (where the two punctures in the epigastrium were made) and the left lobe were somewhat enlarged, but their substance did not appear otherwise unhealthy.

The gall bladder was contracted, and of a pale colour.

The right cavity of the thorax was full of darkish serum,—it contained at least five pints; and the lung of that side was collapsed, compressed into a surprisingly small size, non-crepitant, and perfectly unserviceable.

The left lung, heart, and large blood-vessels, presented no change from health.

From the state of the liver and contents of the left cavity of the thorax, there was no doubt as to the cause of death. The event was probably neither accelerated nor retarded by the punctures made in the former: it was judged that the patient was past recovery by any human means at the time they were made.

Clinical Remarks by Dr. Murray.

We have derived much information of an interesting and instructive kind from this unfortunate but important case [of Gorman].

Three days subsequently to making the punctures on the 2d instant, the purulent discharge ceased, when great increase of dyspnoea, anxiety, and restlessness, supervened; and the liver was felt projecting still farther beyond the ribs all across the side.

The hectic fever, weight in the hepatic region, and sense of suffocation on *decubitus* on the back and left side, continued; the latter symptom was much more prominent than I ever witnessed in any case of simple hepatic disease; and was owing, as appeared on dissection, to the hydrothorax. The case was altogether distressing and alarming, and the prognosis most unfavourable.

In the hope of being able to reach another abscess, or to re-open the one I had before penetrated in a more depending position, and thereby afford relief to the patient, after making a preparatory puncture with a lancet through the integuments into the enlarged liver, about an inch and a half more to the

right, and lower than the first puncture (*i. e.* a little below the cartilaginous junction of the ninth to the eighth rib); I inserted a trocar in the direction of the centre of the diaphragm, but without coming to any matter.

The operation did not appear to give much pain, (*the preparatory puncture with the lancet rendering the entrance of the trocar easy*), and it was not followed by any bad effects; but we did not deem it advisable to institute any further exploration at this time.

Very little blood came from the liver, but on withdrawing the canula, a quantity of serous (*ascitic*) fluid escaped through it from the cavity of the peritoneum, indicating that no adhesion then existed between the liver and abdominal parietes at that part.

The details of the case, and post-mortem examination, show, first, that as no bad consequences resulted from making the punctures, danger need not be apprehended from pushing a trocar into the parenchymatous substance of an enlarged liver; secondly, that the punctures were the means of causing adhesion between the liver and abdominal parietes; thirdly, that the man's life, if it was not eased and prolonged, at any rate was not shortened by the operations; and, *lastly*, that even had I succeeded on the 2d instant in reaching all the abscesses to evacuate them, so great was the *quantum* of disease in the liver and right lung, that there would have been no chance of the patient's recovery.

It has been customary for medical officers to mark with a sort of *exultance* the unexpected discovery of an abscess in the liver after death, as if thereby showing that death must have been the inevitable result of any treatment in such cases; but, hereafter, I shall rather be inclined, *in general*, to consider such a discovery as a reflection upon their discrimination and practice.

There is at present too great reluctance on the part of most practitioners to explore enlarged livers, even when there are strong characteristic symptoms of existing abscess, from apprehension of danger in the operation. A *detering* story is told here of a patient once dying from hæmorrhage in consequence of a trocar having been pushed into his liver; but I can call to mind seventeen cases within the last few years, wherein I performed this operation without any bad consequences; by which six of the patients were recovered, and are alive at this day I believe.

I consider, that, *with a good anatomical and pathological knowledge of the region in our mind's eye*, to enable us to avoid the large hepatic vessels, the gall-bladder,* the

* A tumid gall-bladder projecting at the epi-

colon, and the stomach; there is abundance of evidence to authorize us, nay that it is our bounden duty, to explore the liver, without hesitation or delay, in most cases where pathognomonic symptoms of abscess in it exist, and the disease is interfering seriously and prejudiciously with the functions of the organ, and with the general health of the patient.

By early accurate diagnosis, and active constitutional and local (*preventive*) treatment, a favourable termination may very often be happily brought about in hepatic diseases without the necessity of operative procedure; but when abscess has once formed, we know how little advantage is to be expected from persisting in the use of mercury or any other medicine: therefore, let the question be fairly put—Does not the trocar, with a well regulated diet, hold out a better prospect of success?

The case under consideration was at first under the care of my friend Dr. Mortimer, who I must mention was about to explore the liver when the man was transferred to the 54th hospital, where the Doctor continued to take a warm interest in him to the last, and kindly conducted the *Autopsia*.

After having seen this dissection, I would hereafter explore to a greater extent any analogous case; and I am, moreover, of opinion, that all our punctures should be made from the abdominal cavity—entering the trocar or explorer under the edge of the cartilages of the seventh, eighth, or ninth ribs, as circumstances may indicate. We may often indeed get nearer to the abscess through one of the intercostal spaces; and I think primary exploration may sometimes be advantageously made in this situation by a very minute flat canular instrument; but, from not having seen any patient recover where the matter was evacuated in this direction (through the diaphragm); from finding that the action of the fibres of the diaphragm impedes the free discharge of the matter, somewhat like a valve; from observing that air sometimes enters the wound when made here; and from considering that the opening is not so dependent through the walls of the thorax as when made through the abdominal parietes; I beg to recommend the latter mode in all cases; and I must also say that I would prefer a long flat trocar to any other instrument, as the stilette can be withdrawn occasionally during the operation to ascertain if any abscess has been penetrated; and the canula can be left in *situ* afterwards, if thought desirable.

gastrium, may be distinguished from an abscess of the liver, by a peculiar induration continuously surrounding the latter which is not present in the former, it being a soft, elastic circumscribed tumor, the result of over distension from fluid in a natural cavity.

When the abscess is central, or situated in the upper (convex) part of the liver, it will require much confidence and boldness in the practitioner to operate effectually; but what arduous operation does not? The example of perseverance set us by Surgeon Wilkins, of the 41st regiment, (mentioned at page 480, Vol. I. of this journal,) where he introduced the long trocar for puncturing the bladder in order to reach the abscess, by which means he cured his patient, should not be lost sight of.

It seems to me only necessary to draw the attention of medical officers to the general fatality of hepatic abscess, and to excite reflection thereon, to carry conviction to their minds of the advantage of the practice I have been endeavouring to establish. I am by no means anxious, however, that my views should be hastily adopted, as nothing is more injurious than the reception of any proposition without scrupulous examination:—if it be false, this perpetuates error; if true, it abridges its utility by leaving the principle insufficiently investigated and explained, and thus abridging its application*.

CONTRIBUTIONS
TO THE
CHEMICAL PATHOLOGY OF SOME
FORMS OF MORBID DIGESTION.

BY GOLDING BIRD,

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(For the Medical Gazette.)

NOTWITHSTANDING the labours of the physician and chemist since the first application of chemistry to physiology and pathology, we begin even now only to experience a faint glimmering of that light which we may hope to see diffused over those important sciences, when our knowledge of the composition of the secretions becomes, at least, as perfect as that of the structure of the secreting organs themselves. Since the publication of the laborious joint work of Prof. Tiedemann and Gmelin, but few additions have been made to our knowledge of the chemistry of the secretions subservient to healthy digestion; and still fewer contributions have been made towards elucidating the nature of those which are exclusively the product of morbid action. In this subject lies concealed a vast field of inquiry hitherto almost unentered—a

* From the Madras Quarterly Medical Journal, Vol. ii. : 1840.

circumstance readily explained by the difficulties surrounding it. In now offering my own very crude and imperfect observations to notice, I do not presume to even hope that I shall succeed in removing any of these difficulties: all that I dare aspire to is, that the results of my investigations may serve as incentives to others, and that in a few years we may at least know as much concerning the composition of the various morbid secretions of the digestive organs, as we now do of those of the kidney.

I had intended to have deferred publishing these investigations until the whole chain of inquiry had become complete; but as I cannot avoid regarding this as hopeless, at least in my hands, and a large amount of crude matter having accumulated, I have thought it better to make public, through the pages of the *MEDICAL GAZETTE*, the results I have arrived at, leaving to others the task of supplying their deficiencies, and correcting any errors into which I may have fallen.

The present short series of papers is devoted exclusively to an investigation into the nature and composition of the different fluids discharged by the mouth from the stomach, in various states of depraved digestion. Wherever I have been able, I have examined the nature of the healthy secretions for myself; and where this could not be effected, I have had recourse to those authorities which I regarded as most trustworthy. In closing these prefatory remarks, I may be permitted to observe, that I do not propose in these papers to limit myself to mere abstract chemical observations, but, whenever it is practicable, shall endeavour to connect the characters of the peculiar secretion submitted to examination with the history of the case affording it. I am anxious, also, to impress on my professional brethren, that the only part of these communications I offer with confidence is that which depends upon experiment and observation: any hypothesis or theory I may hazard, or conclusion I may venture to draw, is offered with the utmost diffidence, and with the strongest desire of seeing them replaced by better.

The fluids discharged from the mouth under the influence of any exciting cause, may necessarily vary considerably in their source, and, con-

sequently, in their physical and chemical characters. Independently of any peculiar products of disease, we may have saliva, mucus of the mouth, fauces, œsophagus, stomach, or bronchi, gastric and pancreatic juice, and bile, either nearly separate, or mixed in various proportions; and as, with the exception of bile, all these secretions are colourless, and closely resemble each other in their general appearance, the greatest possible difficulty is experienced in ascertaining their real origin. This difficulty is enhanced by the too frequent absence of any differential physical or chemical character, whose existence would enable us to recognise the proper nature of the particular secretion. It is true that the free acids in the gastric juice, and the presence of sulphocyanogen in saliva, afford tolerably ready means of distinguishing these two secretions from many others; but we shall have occasion to see that even these, in several instances, completely fail. In the present imperfect state of our knowledge, all abstract analysis of animal fluids are comparatively useless, from their necessarily imperfect character; hence I have preferred submitting the different vomited fluids to the action of a series of reagents, and comparing the results after the manner so successfully employed by the learned professors of Heidelberg, in their researches on healthy digestion. This mode, although excessively tedious, appears to hold out the only probable chance of proceeding successfully in investigations of this kind. It has also the advantage of not requiring any great tact of chemical manipulation; and hence enables every practitioner who will take the trouble, to offer his own mite of information.

The first secretion which it is necessary to examine in its healthy and diseased condition is saliva. This fluid, in its healthy state, has been made the subject of numerous and careful investigations; and our knowledge of its constituents may be regarded as more perfect than that of any other secretion, the bile and urine excepted; whilst, in its morbid condition, it has been scarcely at all examined.

Abstract and differential properties of healthy saliva.

Saliva, obtained by ejecting it from the mouth, is always mingled with a

considerable proportion of mucus secreted by the numerous little glands studding the lining membrane of the mouth and fauces: hence all the chemical characters given of the fluid thus procured must be regarded rather as applying to this mixture than to the mere secretion of the salivary glands. The fortunate occurrence of a case in which a fistulous opening, through which the secretion of the parotid gland escaped externally, has, in the hands of Dr. Mitscherlich, supplied us with every information we can desire on this subject*. This curious case illustrates many very important points connected with the secretion of saliva, under the various circumstances connected with mastication, speaking, &c. and deserves more notice than it appears to have met with in this country.

A Jew, from Salkoski, ætatis 40, came under the care of Dr. C. G. Mitscherlich, in Berlin, in 1831; he was suffering from severe pain, referred to the third molar tooth of the left side; this was removed, and the whole surrounding parts were found swollen and tender, the tumefaction extending to the zygoma. After the application of a poultice, an abscess matured and burst. Eventually a fistulous opening upon the cheek, about half a line wide, was left, whilst the saliva was prevented escaping into the mouth in consequence of the previous closure of the duct of Steno, apparently by the antecedent inflammation; hence the secretion escaped through the fistulous opening as soon as formed. For the purpose of collecting the saliva for examination, a small bag of varnished silk was carefully adjusted to the side of the cheek over the opening into the duct. When the bag was thus attached, the following interesting facts were observed:—

1. When little or no motion of the lower jaw occurred, and the act of mastication was unperformed, no evident secretion of saliva was observed; thus, on a fast-day, during which the man took no food from 10 A.M. to 7 P.M., and spoke but little, scarcely a trace of moisture was found in the bag attached during those nine hours to the fistulous opening.

2. The secretion of the saliva was excited and kept up by every thing which rendered a movement of the lower jaw necessary, as speaking, mastication, coughing, &c.; the quantity secreted during the ingestion of food increasing in the ratio of the stimulating or pungent properties of the latter, or the difficulty it opposed to the performance of mastication; and during sleep little or no saliva was collected in the bag.

3. The amount of saliva secreted by one parotid gland differs with the character of the food, and the rapidity of the movements of the lower jaw required to effect its mastication. On one day, 28·86 grains of saliva were collected from 8½ A.M. to noon; during this time he spoke but little, read in the Talmud with a low voice, and breakfasted; during the afternoon, 29·45 grains were collected in four hours. On an average 124 grains were secreted in the course of twenty-four hours. From repeated experiments, the smallest quantity secreted during the three daily meals amounted to 713 grains, and the largest to 1154·7 grains. With the ordinary food of the hospital in which this patient was treated, the quantity secreted in twenty-four hours varied from 917·5 to 1472·5 grains.

4. To determine the quantity of saliva secreted in a given time, Dr. Mitscherlich desired the patient to spit into a vessel all the saliva secreted in the mouth during fifteen minutes, its generation being excited by moving the jaw. In this space of time 14·26 grains of saliva were collected in the bag attached to the fistulous opening, and 97·1 grains were spat from the mouth; hence the quantity secreted in fifteen minutes by the right parotid, submaxillary, and sublingual glands, was rather more than six times the quantity secreted by the left parotid. This estimation must be regarded as little else than a mere approximation to the truth, as the fluid spat from the mouth contained necessarily a considerable quantity of mucus.

It is tolerably certain that the total quantity of saliva secreted during a meal, especially during the ingestion of hard and stimulating food, has been much underrated; for during a not very copious meal, an ounce was collected from the left parotid; hence not less than seven ounces must have

* Ueber den Speichel des Menschen, von C. G. Mitscherlich. Rust, Magazin für die Gesamte Heilkunde, 1832, p. 491.

been secreted by the whole salivary apparatus. The quantity of saliva appeared to be by no means proportionate to the bulk of food taken at one meal; indeed, rather the reverse. During a dinner, consisting of spiced soup and boiled beef, with an abundance of garlic and horse-radish, 502 grains of saliva oozed from the parotid: when the meal was changed for one chiefly composed of beef, potatoes, and bread, 462 grains were collected; whilst but 250 grains were obtained when the man dined on rice-milk, broth, boiled beef, and bread. The times occupied by these meals were respectively twenty-eight, twenty-three, and twenty minutes.

Specific gravity.—The density of saliva has been very differently stated by chemists. Gmelin has given 1·0043 as the average specific gravity of saliva spat from the mouth, and, consequently, mixed with mucus, which would much diminish its real density, as I have repeatedly found the latter secretion as low as 1·0027. Dr. Mitscherlich found the saliva collected from the parotid of his patient varied in density from 1·0061 to 1·0088: during a meal it was generally found to be 1·0074. My own observations have been alone made from saliva collected from the mouth, and freed from insoluble mucus by filtration; and here I have found the specific gravity to vary considerably, as shewn in the following table:—

Saliva from a healthy individual	1·0091
Saliva excited by masticating caoutchouc	1·0155
Saliva from mercurial ptyalism	1·0043
Saliva in another case	1·0062
Saliva from ptyalism excited by creosote	1·0084
Saliva from ptyalism excited by chlorine	1·0062
Saliva from a case of hysteric ptyalism	1·0080

The mean density, as determined from these observations, is 1·0081, considerably above the average of Gmelin, a circumstance probably to be partly accounted for by the process used by that philosopher to determine the specific gravity; for which purpose he employed a glass globule, and compared the weights lost by it when weighed in distilled water and in the saliva under examination. The density of the secretion, of course, increases in proportion to the amount of solid matter in solution. Thus, in Dr.

Mitscherlich's patient, it was found that when the saliva was of specific gr. 1·0072, it contained 1·468 { per cent. of solids.

1·0079,	“	1·551	“
1·0083,	“	1·632	“

These results may be regarded as extremely accurate, the evaporations having been effected at the temperature of the atmosphere under an air-pump vacuum.

Acid and alkaline state of saliva.

Almost every writer has given a different account of the behaviour of saliva towards test papers. The observations of Dr. Mitscherlich have now satisfactorily explained the cause of this disagreement among different observers. He found that the secretion from the parotid was always faintly acid when mastication was not going on; but the ingestion of food, whether fluid or solid, invariably caused it to become alkaline. Often the very first morsel of food was sufficient to produce this effect in a very decided manner. I have repeatedly tested the truth of Mitscherlich's statements with oral saliva, and am perfectly convinced of their accuracy. The reason why the secretion found in the mouth during fasting is so nearly neutral as scarcely to affect the most delicate test-paper, arises from it containing a considerable quantity of mucus, which I have elsewhere shewn is generally alkaline*; and hence the slightly acid reaction proper to the very scanty secretion of the salivary glands during fasting is neutralized by the alkaline condition of the mucus. One hundred grains of saliva from the parotid of Mitscherlich's patient required, for complete saturation, 0·223 of sulphuric acid, or just the quantity necessary for the neutralization of 0·174 of soda. In another experiment, 0·196 grains of acid was sufficient for the saturation of 100 grains of saliva, equivalent to the neutralization of 0·153 of free soda.

From a very elaborate and careful examination, 100 grains of parotid saliva, of sp. gr. 1·0075, were found to

* In diseases in which the secretion of the mouth and fauces becomes excessive, as in scarlatina and cynanche tonsillaris, its alkaline properties become exceedingly obvious. In the former disease especially, I have repeatedly had my attention drawn to this by the patient's friends, who had noticed the infusion of roses, used as a gargle, changed to a green colour on being spat from the mouth.

contain 0.494 grains of saline matter, consisting of a proportion of potass and soda too considerable to be neutralized by the acids present: and supposing these existed in combination with potass as the stronger base, .094 grains potass, and 0.024 soda, would be left unsaturated. At least a portion of these bases existed in combination with an organic acid, as the lactic, and mucus, or more probably albumen; for on saturating fresh saliva with an acid, a deposit of matter resembling coagulated albumen, but regarded by Mitscherlich as mucus, always ensued.

Dr. C. H. Schultz, of Berlin, in a very ingenious monograph on digestion* penned whilst convalescing from the horrors of severe dyspepsia, has laid considerable stress on the alkaline characters of saliva. This physician obtained the secretion by holding a lump of sugar in his mouth†; and hence, from the facts observed by Mitscherlich, its alkalinity must have been expected. Each drachm of the secretion thus obtained required a drop of wine vinegar for its complete saturation; but, in the course of a few hours, recovered its alkalinity. Schultz has stated the free alkali to be ammonia; and that the vapour evolved from heated saliva not only turns red litmus paper to blue, but evolves copious fumes, when a rod, dipped in hydrochloric acid, is held near the vessel. This statement is opposed to the very accurate observations of Mitscherlich, and is quite unsupported by my own experiments; for out of seven different specimens of saliva obtained from separate cases, one only afforded any evidence of the presence of ammonia; and this was a case of profuse mercurial ptyalism. The evidence of the evolution of ammonia, based on the production of fumes on approximating a tube dipped in hydrochloric acid to a previously heated fluid, is frequently fallacious; for vapours which may be mistaken for those afforded by traces of hydrochlorate of ammonia are produced whenever a rod dipped in strong hydrochloric acid is held over a tube containing almost any fluid heated below ebullition; hence, although it is by no means improbable that Schultz's own saliva contained free ammonia,

yet the evidence on which it rests is not indisputable; and, even if it were, would, I suspect, as shewn by my own experiments, constitute the exception rather than the rule. It is true that the circumstance of saliva, when slightly acid, or after being neutralized by the addition of an acid, recovering its alkalinity in the course of a few hours, would, on a *primâ facie* view, appear to countenance Schultz's opinion of the volatile nature of the alkali exciting the secretion. We shall, however, have occasion to learn that this fact admits of a totally different explanation.

Chemical composition of saliva.

All the secretions of the body may be regarded as composed of two classes of proximate elements; one consisting of those common to all or most other secretions, and the other including those peculiar to the particular fluid: to the former belong water, certain saline combinations, as chloride of sodium, lactate of soda, with salts of potass, lime, magnesia, and ammonia, with not unfrequently silicic acid and iron; whilst to the latter are referred the characteristic ingredients which serve to distinguish *par excellence* one secretion chemically from another: these are either peculiar saline crystallizable compounds, generally belonging to the cyanogen series, as sulphocyanogen, allantoin, urea, uric acid, and perhaps murexid (purpurate of ammonia of Dr. Prout), and all capable of being formed artificially, and readily convertible into each other; or consist of certain other generally highly azotised ingredients, either characterized by intensity of colour and peculiarity of taste, as in the bile, or marked by the absence of colour, blandness of taste, and by their each and all giving abundant evidence of being chiefly composed of albumen in an uncertain state of combination, as in all the mucous class of animal secretions. The chemical constitution of saliva, as in the case of other mucous secretions, has been, I conceive, much obscured by admitting among its ingredients certain ill-defined and worse-understood proximate principles, often holding their component elements in a state of such loose combination, that a second evaporation of their solutions, the addition of alcohol, or a few days' exposure, is sufficient to effect such

* De alimentorum concoctione, experimenta nova, Berolini, 1834, 4to.

† Op. citat. p. 56.

‡ Guy's Hospital Reports, vol. 3, p. 42.

changes as to give rise to the formation of new substances ; at least if difference of solubility, or in behaviour towards some comparatively unimportant reagent, be regarded as sufficient for dignifying them as new and distinct, which has been too frequently the case in researches connected with physiological chemistry.

1. If fresh saliva be filtered through paper, numerous flocculi, regarded as insoluble mucus, are separated ; these consist of a substance much resembling, in its chemical characters, insoluble albumen, mixed with particles of epithelium, readily distinguishable under the microscope.

2. When freshly filtered saliva is mixed with a minute quantity of any dilute acid, a deposit falls, in weight seldom equalling one per cent. of the whole : this deposit appears to consist of coagulated albumen in a slightly modified state, and is generally referred to that convenient genus of unintelligible matters denominated mucoid, and, like the *extractive*, which in all analyses of vegetable products made until within the last ten years formed so important an ingredient, is now rapidly vanishing from the results of modern analyses.

3. When fresh saliva is exposed to the air, it gradually becomes turbid, and a white deposit, in no way distinguishable from coagulated albumen, occurs. This change is attributed by Mitscherlich to the absorption of carbonic acid from the atmosphere, although he does not cite any experiment in support of this view. It is, however, certain that the presence of carbonic acid is by no means either essential or necessary for the production of this effect ; for I find that freshly filtered saliva becomes as opaque by an exposure to an atmosphere of hydrogen gas, as it does when left in the open air. The deposit which falls when saliva is kept under a receiver of hydrogen is quite insoluble in nitric acid, and closely resembles that produced by the addition of an acid to the recent secretion.

4. When saliva is carefully evaporated to dryness, it loses its volatile ingredients, consisting of water, and rarely of some ammoniacal salt ; the residual mass is certainly generally alkaline, although Schultz has stated the contrary to be the case—a circumstance showing how little dependence can be

placed upon the results of abstract analyses of animal fluids.

The extract of saliva, by digestion in alcohol, is separated into two portions ; the one being soluble, and the other insoluble in that menstruum. The portion dissolved by the alcohol consists of organic ingredients common to most animal fluids, and composed chiefly of traces of fatty matter, with lactate of soda (the mixture constituting what was formerly recognised as mucos-extractive matter), and some other saline ingredients, among which the presence of sulphocyanogen, combined with one of the alkaline metals, is the most remarkable circumstance, as it may be regarded as furnishing the most satisfactory evidence we possess of a fluid containing saliva.

That portion of the extract of saliva which refuses to yield to the solvent action of alcohol, is by the action of water separated into a soluble and insoluble portion ; the first being regarded as the peculiar matter of saliva, the *ptyalin* of Berzelius ; the latter as salivary mucus, (*Speichelschleim* of Mitscherlich). Of these, the so-called salivary mucus closely resembles the matter precipitated from fresh saliva by exposure to the air, or the addition of an acid, and may be regarded as but slightly modified coagulated albumen, combined with a small quantity of saline matter. The other ingredient, *ptyalin*, affords an excellent illustration of the indefinite nature of most of the proximate ingredients of mucous secretions ; for not only do Berzelius, Leopold Gmelin, Mitscherlich, and Schultz, all of whom have most carefully examined its properties, and whose accuracy of observation is beyond all question, differ materially in their description of more than one of its chemical characters, but the three last named philosophers have correctly stated that each time a solution of ptyalin is evaporated, and the residue digested in water, a considerable quantity of insoluble matter separates, formed in fact by the process of desiccation, for which, as Gmelin has shewn, the heat of a water-bath is sufficient. This observation, which is due to our talented countryman, Dr. Bostock, is very important, for it shews how little positive difference really exists between the principle dignified with the name of ptyalin, and the albuminous elements of other mucous secretions. This view

receives still further confirmation from the results of the decomposing influence of the voltaic current on ptyalin, which I observed in the course of my researches on the action of such currents on albuminous fluids, elsewhere recorded*. Mr. Brande long ago shewed that when saliva was submitted to an electric current—a substance in all respects analogous, if not identical, with albumen, separated at one of the electrodes, although the presence of this principle was not previously demonstrable. To ascertain which ingredient of saliva yielded this substance, I prepared some ptyalin according to the process described by Berzelius†, dissolved it in water, and exposed it to the action of the couronne des tasses of 36 pairs, used in the experiments related in my papers on albumen. Coagulation ensued at both electrodes, most copiously at the negative, where an odour of chlorine was evolved; and by no character whatever could the coagulated mass thus separated from ptyalin be distinguished from albumen; with which, indeed, I cannot but believe it to be identical. I have, in the papers alluded to, stated my conviction that the evolution of chlorine, by the decomposition of the chlorides present in the fluid exposed to the electric current, is the active agent in causing the separation of what I have regarded as albumen; and, in accordance with this, I have invariably found that a current of gaseous chlorine was sufficient to at once destroy the peculiar state of combination in which albumen exists in many mucous secretions, and develop what I conceive to be their true nature. Hence, if a few bubbles of chlorine gas be passed through saliva, or a solution of ptyalin, a deposit, often very copious, of a substance which I cannot distinguish from albumen, rapidly occurs.

5. When the extract of saliva is incinerated, its fixed salts are left, mixed with the carbonates of potass and soda, the bases of which existed in the secretion, combined with organic acids, chiefly the sulphocyanic and lactic, and albumen. The salts consist of chloride of potassium and phosphate of lime, mixed with a minute portion of silica.

The whole quantity rarely equals half a grain from a hundred of saliva.

Saliva may thus be regarded as composed of the following ingredients :—

Those common to other animal fluids—

Insoluble albumen combined with soda.

Lactates of potass and soda.

Chloride of potassium, and perhaps sodium.

Phosphate of lime.

Silica.

Those proper to saliva, and distinguishing it from analogous secretions.

Sulphocyanogen.

A peculiar albuminous principle, (ptyalin) convertible into a matter identical with the so-called insoluble mucus, by repeatedly evaporating its aqueous solution, and evolving albumen by the action of an electric current, or of chlorine gas.

It may now be asked how is saliva to be distinguished from other fluids discharged from the mouth, as in pyrosis, gastrorrhœa, &c.? It is evident, *in limine*, that the peculiar matter, or ptyalin, is utterly useless for this purpose, as not only do our best chemists positively differ in their accounts of its properties, but the mere act of evaporation is sufficient to mask all its peculiarities, and convert it into a matter perfectly undistinguishable from that present abundantly in all mucous fluids. In some cases, it is true, the specific gravity of the fluid, as I shall endeavour to shew hereafter, may afford some little assistance, but notwithstanding this, we are compelled to fall back upon the chemical characters of sulphocyanogen as alone characteristic of the secretion in which it exists: yet even here we must proceed with caution, for the presence of this element has been doubted, and even denied, by some very careful observers. As, however, I shall have occasion to base some arguments on the origin of the fluid of pyrosis, upon the assumption that the ingredient under consideration really exists in saliva, I trust I shall be excused for examining a little at length this essential but disputed point.

[To be continued.]

* London and Edinburgh Philosophical Magazine, vol. x. p. 87.

† *Traité de Chimie*, Paris, 1833, vol. vii. p. 155.

ON THE
DIVISION OF MEDICAL LABOUR.

BY ROBERT HULL, M.D.

[For the London Medical Gazette.]

THE principle of honour, in our profession, is tested in the maintenance of our division of labour; of *fair* competition.

Physician, surgeon, apothecary, are the ancient, the true, the English arrangement.

Theoretical amalgamations may be very plausible; and foreign examples may be different; but I say, in defiance of the utilitarian cosmopolite, that the medical habitudes of this country are best, are alone, adapted to England.

The stir about medical reform is not made by true Englishmen; but by lovers of America; by imitators of France.

Let any body trace its history. The upholders in Parliament have, all of them, been radical reformers; in the profession itself, reforming radicals.

Before these philanthropists undertook the patronage of the suffering faculty, the faculty were not conscious of suffering.

The pure physician obtained his moderate living; content with less income than the apothecary, possessed of more rank.

The pure surgeon envied not the titular distinction of the physician; coveted not the retail fortune of the apothecary; but, conscious of the heroism of his department, preferred being a plain chirurgien, to the possession of an equipage, with gallipots; or of a handle to his name.

The agitation is raised by the general practitioners.

The clamour for reform is raised by a few only, but a noisy few.

They consider it a hardship that they should be placed, professionally or socially, in a position below the pure physician or the pure surgeon.

The clamour for reform is a demand for the abrogation of divided labour.

It was first raised by superficial persons; men of a *new* light; despisers of the institutions of past days. Self-complacent philosophers, whose abuse of national customs in our profession was founded on ignorance of their practical benefit; or crafty world-

lings, who coveted popularity; and a fortune easily made by notoriety, good or bad.

But they were backed by some, who ought to have known better. A practitioner, of just celebrity, thus delivered himself in favour of promiscuous practice:—

“In our view *surgery* includes all injuries; nearly all external diseases, and most of those internal ones, which produce changes recognisable externally; and all which require operations, external applications, or any kind of medical treatment. The rest of medicine belongs to the *physician*.”

Rest, indeed! Crumbs thrown in contemptuous kindness to the animals beneath the chirurgic table!

But, what wretched tuition this from a metropolitan cathedra! To a herd of boys, requiring restraint and humiliation—not to think more highly of themselves than they ought!

“Gentlemen! you are all admirable Crichtons! Never mind that foolish aphorism—‘Ο Βίος βραχύς and ἡ τέχνη μακρῇ. Art is not too long for any of you, whencesoever you come, whithersoever you go. Carry into the provinces a contempt of peculiar studies and divided practice. Cry down the pure physicians, the pure chirurgiens in your neighbourhood. Medicine is one and indivisible. You, who have pestled in the mortar, and walked the hospitals, hasten back to the country to carry on the healing business: you are as competent as the best.”

Pretty counsel this for Mr. Dix of Modbury, and Mr. Hosegood of Piddletrenthide!

To me it seems that the wise paternal master should have said—“Mr. Hosegood and Mr. Dix, remember your objects! You are going to remote districts, where you *must* practise generally; where, unhappily, the division of labour cannot be brought to bear; where the pure surgeon, the pure physician, cannot be supported. Prepare, then, for the useful! Aim not at microscopic anatomy, for you are to be rustic physicians. Devote not undue time to chemistry; for you are also to be rural surgeons. Make the best of your various studies; but, when you do arrive at your country arenas, sacrifice not the lives of your patients, lest, by summoning Mr. Barnes from Exeter, or Dr. Symes from Bridport,

you should forego a character for universal skill !”

Such admonitions would be kind to the students; useful to the public. The young gentlemen, in after life, when fully bearded, would remember them with gratitude; for *then* they would find them alone applicable to human capacities; and the public would be treated by practical, humble-minded persons, instead of slight, superficial prigs.

Yet, one in-authority has ventured to deny “the inherent, essential differences between surgery and physic, which require that they should be exercised by different persons, and taught by distinct teachers.” Has cavilled at “a College of Physicians, and a College of Surgeons.” Has pronounced the distinction artificial, that “it originated in a period of barbarism; has been upheld by a blind deference to authority; and, although the same causes, together with the interests of *certain* established bodies, may protract its existence, it has received a fatal blow.”

Ἐυρήσει τὰ σάθρα τοῦ δεῖνα αὐτὸς ὁ πόλεμος.

We shall see whether, in this instance, the medical Demosthenes be a true prophet!

The Royal College of Physicians has recently declared that they will *not* sanction the abolition of our specific division of labour, wisely doubting the existence of many admirable Crichtons.

The College provides for the average capacities of medical men; and the justification of this learned body may be found in the answers to three questions. First, is the division of labour, as a general principle, desirable, necessary? Second, is the division applicable to medicine? Third, is the particular division, which obtains in England, the most adapted to this country?

Now, the division, as a general principle, is admitted universally. Then, is it applicable to our profession? Why not? It is for those, who grant the general principle, to shew cause why they deny its application to the healing art. On them the burthen.

But there is no reason for dialectic fencing. The disinterested person, who is philosophically humble, and will but think, must concede the necessity for division in our extensive, almost im-

mense; our multifarious, almost infinitely variegated art.

Anatomy, botany, chemistry, the materia medica, natural philosophy, physiology, zoology, and numerous sciences beside, are all subsidiary, some essential to medicine.

Any one of these might occupy the longest life: nor then be exhausted. The student, therefore, hopeless of a *mastery* over all or either, must apportion his attention to few: at all events, not to very many. It is the dream of a visionary, if he calculates upon a profound acquaintance with that mass of sciences.

But he may obtain a basis of general acquirement, more or less special, whereon to erect a practical superstructure.

This brings us to the professional divisions, which have long obtained in this (once) soberized country.

Interwoven with the very structure of English society, the medical practice hath been tripartite; physic, surgery, pharmacy: *or* surgery united with pharmacy—the calling of the general practitioner.

An arrangement founded on the public demand and wants; compatible with true science; conducive to fair competition.

This adapted, this scientific, this fair, this ancient division of labour, the medical reformer seeks to destroy.

One faculty is his aim. Each practitioner is to form a compact cyclopædia of therapeutic lore. Locality alone is to effect a difference betwixt the *savant* earning a bare subsistence among the Cornish mines, and the physician to her Majesty—whose sacred person may heaven preserve from the levelers of the *one* faculty.

The necessity for the distinct physician is proved by the distinctions among physicians. Even the tripartite analysis is not sufficient!

We constantly behold pure practitioners, men of great capacity, rich medical acquisition, indefatigable application, pursuing mainly *peculiar* investigations. Look at the medical chemistry of Dr. Paris. Would the world have rejoiced in *such* useful, masterly productions as his, if their author had studied with equal order the surgery of the eye, of the bladder, of the bowels?

The most splendid discoveries in our

art have been the result of almost exclusive labour. Divided work is the best security for attempted perfection.

The levelling amalgamator may vociferate in lectures, or advertise in books, that the same men can be equally skilled in physic and chirurgery; but the signal will not be respected, if it meaneth "come to me." The public, like Nelson, "have a right to be blind at times:" and *malgré* the universalist, the panaceist, they will consult Dr. Abercrombie for their brains, Dr. Hope about their hearts, and Dr. Johnson about their bowels.

The predilection for peculiar medical studies is entirely individual peculiarity. It cannot be inculcated. It may or may not be developed.

It is sufficient for practical use, that the medical disorders generally be the department of a class of practitioners. If, of this class, any man choose to lay himself out for stricter studies and practice, so much the better. Such a physician is neither blamed, nor satirized, nor envied by his medical brothers. Nay, he is respected; he is heard with deference; he is welcomed to consultation. It is the inerudite superficial who snarls at these divisional philosophers. By the usage, in this civilized nation, of centuries, the title of Doctor hath been given to the physician, with a full understanding by the well-bred classes of society that the physician is not to be the surgeon.

On this perfect understanding young men are educated to the healing profession. The physician is supposed to possess an advantage in his title: to forego his chance of making money by the *taking* operations of the hand, and by the lucrative extensive retail of the apothecary, the nimble shilling; whether this practitioner keep a shop, or simply, as it is proudly called, "a surgery."

The pure physician, then, claims the respect of the other practisers, on whose department he is too honourable and just to obtrude. If a physician encroach on the domain of the chirurgeon, he should be cut by his brethren, as one who has not stood by his order. Yet, I fancy, there is not one but he has made a promise to maintain the dignity of that order when he received his degree or his license.

But if a physician add the vendition of drugs, if he invade the department

of the apothecary, he should be rumped, by common consent. No term is dearer to Englishmen than the word *fair*.

It is not fair for the medicus to secure chirurgic patients by the attraction of his prefix.

It is not fair for him to take such low fees, that patients are decoyed away from the general practitioner.

Poaching on the manor of another is well understood, and resented by sportsmen. Professional poaching is more disgraceful.

The lawyers permit no poaching upon their divisions. We hear not of "one faculty" in the law. Every man is contented in that station of law in which it hath pleased his friends to place him, and works away cheerily *per varios casus; per tot discrimina*.

The solicitor repines not that high office and rank should be exclusively open to a certain class of his legal brethren. Grumbles not that Lord Cottenham has obtained the peerage; that Sir William Follett will secure it. Deprecates not the great name of a Sugden; covets not, as undeserved, the Devonian acres of a Preston.

The public is ever practically overlooking the division of labour, when it employs the irregular medical.

On the profession devolves the duty, at all personal sacrifices, to refuse the recognition of such a transgressor.

The public, ignorant of our proprieties, is always ready to employ a medical Jack-of-all-trades.

The profession, if there be truth in my argumentation, if there be the greatest good accruing from divided work, even for the public themselves—the profession should set an example of strict adherence to the proper. Let the laity employ whomsoever they please. Only let them not plead, in excusation, the indifference of the healing corps itself.

If we refuse to sanction, by fraternity, the grosser proceedings of the outrageous empiric, we are bound to repudiate less deviations from the rules; all infractions of what, by common consent, is deemed the best practical regula for the healing art.

For the world really is no judge of what is correct in therapeutic affairs: they can form no notion, correct and logical, of erratic practisers, save what they learn from the professional body.

For what is their own unassisted intelligence about professional matters? Witness the empirics of all ages, from Asclepiades to Morison!

“Asclepiades, orandi magister, nec satis in arte ea quæstuosus, ut ad alia, quam forum, sagacis ingenii, huc se repente convertit; atque, ut necesse erat homini, qui nec id egisset, nec remedia nosset, oculis usuque percipienda, torrenti ac meditata oratione blandiens omnia abdicavit; * * * faventibus cunctis, universum prope humanum genus circumegit in se, non alio modo, quam si cœlo emissus advenisset.”

This is the story of all places and all times. The people have been proved by all history to be no judges of medical merit. The worldly success, the popularity of any pretender, is no criterion of science.

No era has been without its charlatan in every great town and country; and the varieties of charlatanism are infinite. It may be practised from the ignorant, but artful plebeian, up to the academic irregular. All lawlessness, all puff, all intrigue for patients, all hugging, are charlatannerie. How easy the process for an unprincipled man! How strong the temptation! How dangerous for society, when we consider the multiplicity of craftsmen ever on the look out for an opening to lucre by shorter and less honest methods!

The public are very much to be palliated when they foster the irregular practitioner. For the medical art is the only art whose professors can elude the test of successful deeds.

If the medical performer fail in his essays, he has only to turn round on his objector and say, “The death of the patient disgraces not me. I never pretended to obliterate from the calendar the inevitable hour—*πᾶσιν ἡμῖν καθανεῖν ὀφείλεται*.” But if he succeed; that is, if the patient survive, in spite of the medicinal treatment, through a ferrugineous frame, or the special providence of heaven, he raises the cry of victory, and demands from all that are interested in the case the laurel wreath for a saved citizen.

It is the profession, and the initiated only, who can decide when failure is not disgraceful, nor victory the result of merit.

In other professions, where common

untechnical sense is brought to bear, the meed in general is given to the deserving. The just triumphs of Wellington, peaceful civilians can appreciate and hail. But it requires the scientific warrior to pronounce that even failure may be glorious.

“The most splendid affair,” said Napoleon, “that has occurred in my time, was a defeat—the attack upon Bergen-op-zoom by Graham.”

But whence, it may be inquired, whence the fame and the wealth of the medical ignoramus or irregular? “The voice of the people is that of Heaven.”

First, from the actual cures which every ignoramus does effect, however few they be. Second, from the numerous cures, which, in spite of attempted murder, the *vis medicatrix naturæ* can and will perform: which cures, direct and natural, are all paraded as done by the doctor—all puffed—all magnified—all multiplied by rumour; while the failures, the deaths, the slaughter, are all systematically hushed up, or wilfully overlooked.

The public are utterly incompetent to pronounce on medical merit.

The great fortunes made by Solomon, by St. John Long, and all that genus, are a damning proof.

The medical profession must be true to itself. It must establish its own laws: it must maintain them rigidly: it must make no appeal for the decision of the people, on any litigated or doubtful professional question.

Once having made up our minds that the division of labour is essential to the public weal, let us adhere most rigidly to this division: let us scout, *coute qui coute* to ourselves individually every violator of this distinction; whether he be a wilful and selfish sinner, or an honest person whose intellect does not appreciate the utility of departments.

But will this protect the public, will it save them from themselves?

Ὅλοιο σὺ καὶ χῶστις ἄκοντας φίλους
Πρόθυμός ἐστι μὴ καλῶς ευεργετεῖν.

If not, let them take the consequences! If not, let the irregular go on to prosper, to be enriched, to be insolent! It is the duty of the medical profession practically to protest against irregularities! How? Let each and every honest individual refuse to meet

the professional culprit; whether requested to consult with him by the humble shopkeeper of Gutter Lane, or commanded by majesty itself.

If not, if the public perceive that, in spite of any temporary alienation or protest, the amalgamating irregular be countenanced, be met by the members of our profession; they have a right to discredit themselves the division of labour. Can that be important with which the faculty itself dispenses? Why expect of the people to walk in a course which the medical body quits?

It used to be the fashion for a gentleman to "drop his degree," if he practised surgery; and our only method to put a stop to an innovation on this fair, English custom, is to decline any meeting—professional, I mean—with these mongrel practisers. Let us endeavour to disprove the charge against these times, that "every thing is low."

TETANUS CURED BY TOBACCO.

To the Editor of the Medical Gazette.

SIR,

SINCE you did me the honour to publish a successful case of idiopathic tetanus, in the autumn of 1839, I think I may reasonably indulge the hope that you will insert, in your valuable periodical, the accompanying case, where a similar treatment has been adopted, and with the same result.—I remain, sir,

Your obedient servant,
HENRY BULLOCK.

Uxbridge, June 24, 1841.

George Clarke, aged 33, a healthy labourer, of tolerably regular and temperate habits, was admitted as a patient of the Uxbridge Dispensary, labouring under symptoms of tetanus, on Thursday the 13th of May. Upon entering his apartment I found him lying in a state of opisthotonos, with his countenance peculiarly characteristic of the disease, the paroxysms occurring about once in three minutes, when the pain was most agonizing, whilst during the interval even the muscular rigidity was intense, the perspirations profuse, the function of deglutition was performed with the utmost difficulty, and never without producing a spasm.

The history of the case, as obtained

from the patient and his wife, is, that six weeks ago he received a blow on the left side, between the lower rib and the crista of the ilium, and that he has had pain in that situation ever since, though not sufficient to incapacitate him for his ordinary occupation of a labourer until Tuesday (two days before I saw him), when he complained of stiff neck and sore throat, with an almost entire inability to swallow, which symptoms have increased up to the present time. His pulse is 80, and regular. Firm pressure over the seat of the accident creates no further pain than when applied to any other portion of the abdominal muscles, which always has the effect of exciting a paroxysm; nor is there any perceptible fulness there. The trismus admits of the separation of the jaws to an extent of about the third of an inch. His respiration is hurried, and he describes a constant and severe pain in the site of the diaphragm, extending from the ensiform cartilage to the spine. I now prescribed—

R Hyd. Chlor., Ext. Coloc. Co. aa. gr. v.;
Ol. Croton. ℥j. ft. pil. ij. st. sumend.

R Tabaci Foliorum, ʒss.; Aquæ ferventis, ʒix. Macera per quartam horæ partem, et cola ft. enema statim injiciend.

Evening.—The remedies have had the desired effect of removing from his bowels large, black, and scybalous evacuations. There had been no collapse peculiar to tobacco; and the man's sufferings appeared rather enhanced than alleviated since my visit in the morning.

Enema repetatur sextâ quâque horâ.

R Quinæ Disulph., Ferri Sulph., Zinci, Sulph. aa. gr. ij.; Acid. Sulph. Dil. ℥v. ex Mist. Camph. 3tiâ quâque horâ. Brandy in arrow-root, and strong beef-tea.

I will not render the perusal of the case wearisome by a narration of the daily reports and slighter signs of amendment and alterations, but will merely state the general outline. The above plan having been rigidly adhered to, for three days, the patient had very materially improved, the paroxysms occurring about every twenty minutes, when they were shorter in duration and less in severity. There is great permanent rigidity of the whole muscular system. The trismus has so much diminished as that the first joint

of the index finger can be now easily admitted into the mouth; there is yet extensive opisthotonos; his countenance has very much improved, his perspirations lessened, his breathing is not so much oppressed; and he is now able to sleep for half an hour at a time: his pulse remains the same except at the period of a paroxysm, when, of course, it becomes disturbed; however, this shortly subsides. The tobacco produced the usual symptoms of excessive prostration every time it was administered, which generally continued for two hours, with the exception of the first three, when there was no sensible effect. This agent was now omitted, and he was to continue the tonic with half a pint of brandy per diem, and

Hydr. Chlor., Extr. Coloc. Co. aa. gr. v.
ft. pil. ij. alt. nocte.

5th day.—Since the last report, that of the third day, all the symptoms have returned, and in a more aggravated form than previously, the jaws being perfectly clenched, and his pulse quick and feeble. Notwithstanding this depressed state of the circulation, the enemata were again ordered, and to be injected every eight hours; and after the two first he began to improve; if however, from any accident, a longer interval were allowed to elapse, the spasm and profuse perspiration immediately increased. This treatment, omitting the pills, was diligently persevered in for four days, without intermission, and with the best effect; then the tobacco was discontinued gradually.

12th day.—He is now very much better, indeed there is scarcely any spasm, and he is able to open his mouth without restraint. He was desired to continue his medicine three times a day, with calomel and colocynth every alternate day. Port wine, a pint daily, ale and mutton chops.

21st day.—The man is quite well, and able to walk out, merely suffering from debility. Mr. James, who witnessed the case throughout, also observed the very remarkable relapse on the omission of the tobacco, and the immediate amendment on its renewal.

I have termed this case one of idiopathic tetanus, merely because it is not one of visible traumatic. From the fact of there having been pain in the situa-

tion of the blow ever since its infliction, I am disposed to think some injury was sustained, whether by nervous filament, muscular fibre, or fascia, is uncertain; but probably there was partial rupture of one or more tissues, which I apprehend in a peculiar state of constitution would be inadequate to excite that violent form of reflected constitutional irritation which we designate tetanus. But the treatment is what I am anxious should attract particular attention. I am aware I have no new remedy to suggest; indeed, I should search the Pharmacopœia in vain, were I to attempt to find a remedy which had not been employed in the treatment of tetanus. Tobacco, then, I consider our sheet anchor in arresting this formidable disease, but it must be employed with a decision and boldness commensurate only with the fatality of the malady, and the extreme urgency of the case. The disease being so rapidly destructive, our time and field of action are very limited; hence I have been induced to use this desperate remedy with a fearless perseverance, as possessing the peculiar property of most powerfully affecting the *entire* system, desiring at the same time to fortify the nervous system by tonics and stimulants, also to introduce as much support as possible by means of the strongest animal broths and jellies; for the effect of the remedy, I conceive, as well as that of the disease, upon the vital powers, would tend to render the support imperative; nor is there any thing incompatible in this mode of treatment.

It is worthy of remark that the patient has never experienced any of the original pain in the side since the appearance of the tetanic symptoms.

MEDICAL GAZETTE.

Friday, July 2, 1841.

“Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri; potestas modo veniendi in
publicum sit, dicendi periculum non recuso.”

CICERO.

THE NEW PARKS.

IN the brief and disturbed session of Parliament which has just been ab-

ruptly brought to a close, no measure was passed that is likely to prove more beneficial than that for the formation of a Park in the eastern district of London; for it is sincerely and confidently to be hoped that the benefit may not terminate in this single improvement, but may be only the first-fruits of the more strict attention which late years have found legislators ready to pay to those things, which, though they may have little or no immediate influence on the existing conflict of parties and of general political principles, are yet of the highest importance to the social condition and health of the lower orders, and so to the well-being of the whole commonwealth.

With the rapid increase of population, which, in a progressively increasing ratio, has taken place during the last few years in our metropolis—an increase depending not only on the national tendency to multiplication, but still more on the growing disposition to centralization of government, and the consequent anxiety of numbers to resort to head-quarters—with this increase of population, the multiplication and spreading of houses, great as it has been, has not kept pace. In nearly every part of London, streets are to be found, not only close and ill-ventilated, but overflowing with poor inhabitants, who, if they are not by necessity compelled to remain all day within doors, are in many instances so far removed from any free and open space, that they learn almost to forget the luxury and benefits of exercise in a clear and wholesome atmosphere.

It has often been boasted, that *considering its size*, London is better ventilated and drained than any city in the world; and in its ordinary meaning the boast is a very true one. For considering that the difficulties of cleansing the earth and air of cities increase in a direct proportion to their size, it is

indeed marvellous, and worthy of boast, that they should have been in any fair measure overcome in London. But it should not be forgotten that just in proportion to the size is the increase of the *necessities* for purification; for it is only in huge cities, like our own, that a great majority of the inhabitants are compelled to be all day exposed to the noxious influences of accumulated impurities. In smaller places, the evils of the homes, even of the poorest, may commonly be in a few minutes escaped from; and without trouble a poor man may obtain at least as much of the advantages of open air and exercise as in London are obtained by the inhabitants of the more central squares. But the mechanic of the metropolis can do nothing of this kind: he himself, when his day's work is over, is too tired to walk three or four miles to the nearest open public ground; and for his children, the only place in which they can be allowed to stroll is the nearest alley that is not so great a thoroughfare as to be dangerous,

In the drainage of London there is comparatively little room for improvement; at least the general plan and management of it is excellent; its defects are only local, and depending chiefly on the negligence of the inhabitants of certain districts. But for a supply of fresh air, what we require is, that in every district the inhabitants should be within half a mile of some large open place, into which the children of the lower classes might be permitted to resort with safety. And this can only be attained by increasing the number either of Squares and Places, or of Parks on the plan of Victoria Park, and increasing them in even a greater ratio than the population is augmented.

The advantages of the plan are two-fold. It affords opportunity for healthy exercise, and it purifies in some mea-

sure the air of the surrounding district. It is very well for those who do not know by their own habits what hard work either of body or mind is, to find their exercise in sauntering through busy streets, and to think it strange that others should want any thing more than these affords, for amusement and recreation ; but streets are not the place for a hard-worked man to take his leisure in. The benefits of exercise are not the result of mere muscular exertion, even in a healthy atmosphere ; this is indeed of great importance, but it needs besides the absence of the excitement of business, and the removal of that which tends to keep a man's mind still bent on the objects of his daily and necessary pursuits. All the advantages of bodily exertion may be found in an active and laborious employment ; but those of exercise, properly so called, are to be had in all their fulness only in leisure from business, and in situations that have some tendency to withdraw the mind as well as the body from its wonted occupation.

But the advantages of parks and open spaces are by no means confined to their affording facilities for exercise and recreation ; they have a more constant and wide-spreading influence upon the purification of the whole slowly-changing volumes of air. There are hundreds of places in this metropolis into which *wind* never finds admission, in which the air is never perceptibly moved, and in which the only change it undergoes is by the gradual mixing of its impure gases with those of adjacent spaces, and of the upper strata of the atmosphere. Nay, even among the wider streets, there are many through which a free current is very rarely blown ; and Londoners, or at least the majority of them, shrink before a breeze that one from the pro-

vinces or the coast would hardly notice. It is probable, therefore, that by far the greater proportion of the change of the air which is effected in the metropolis is the result of the mixture of the gases composing it. But how slow a process this is one may easily judge by the perceptible (we had almost said the palpable) difference between the air of an alley and that of the wide street or other space into which it opens. A part of the gases generated on the ground and near it, are, indeed, by the greater heat there, soon carried to some distance above it ; but that which is thus disposed of is but a small part of what is rendered impure ; nor are the heavy and most noxious of the gases thus most rapidly carried off. Indeed, it is probable that by this process the change of air is, as far as it concerns the health of those who breathe it, and who constantly render it more and more impure, almost unimportant. The only effectual natural process for its purification, in the absence of the wind, is the gradual mixture of the gases, in accordance with Dalton's law that the interspaces of one are as vacua to the particles of another.

No law can be truer, nor can any have more striking examples, than are constantly presented of the operation of this ; but unhappily in practice its influence is very slowly exercised. The heavy gases generated in combustion, respiration, and putrefaction, will remain for hours near their sources ; and, in some of the close streets and alleys of the metropolis, are very rarely carried off by mixture, in any perceptible quantity, during the whole day. It is only in the night, when combustion, in some measure, ceases, and the whole surface of the earth is cooled that they are gradually removed, and the whole atmosphere of the city is brought more nearly to an equa-

lity. Nothing, indeed, can be more striking than the difference even in the *sensible* qualities of the air of London in the early morning, and in the evening; in the former it has a coolness and a refreshing clearness, even in the smaller streets, which those who know it only in the heat of later hours can scarcely imagine.

There can be no doubt that in the favoured situations much of the necessary change of the air depends on the proximity of squares, and other open spaces, into which currents have free access from all sides and from above, and which, therefore, serve as a kind of reservoirs of pure air for the adjacent districts, as well as for spaces into which the foul atmospheres of the neighbouring streets, carried by mixture or other forces, are speedily diluted, and at length completely removed. The free communication between these and the wide expanse of the whole atmosphere renders them the only source through which fresh air can be supplied to the dwelling-places of the poor; and in a city built as London alone is, with the houses of the most opulent and luxurious often surrounded on every side by those of the poorest, it is probable that but for these open spaces pestilence would almost constantly rage in every part.

In those quarters of the town in which squares are tolerably numerous, or in which the close-set houses of the poor are intersected by the wide streets inhabited by the rich, there is probably already provided the best means of rapid ventilation of which they are capable—at least in the present condition of things, where building land is more valuable than gold mines. But in other quarters, not now containing the residences of the rich, and therefore (with an utter disregard of health in the anxiety after wealth) close packed with

manufactories, warehouses, and stores, the interspaces between which are filled by the houses of the poor; in these, since a number of smaller open spaces cannot be afforded, some very extensive one, as near as it can possibly be made, is absolutely necessary. Parks, or other similar places, therefore, should be established in every district not already possessing squares or wide streets; nor should any part be closely covered with buildings for an extent of two square miles, without some such provision for the purity of its air.

We should not have thought of calling attention to this subject, when the necessity of having open spaces near large towns is generally admitted and acted on, were it not that the principle on which they are called for and planned, seems to us not the most correct. They are usually arranged with a view only to afford the population room for recreation in the open air; and for this purpose they are, as we have already said, very highly beneficial, and very necessary. But this is by no means the only end to be attained; we want, besides, that in every district they should serve as reservoirs of pure air; and, for this purpose, one park, at a great distance, however large, is far from being so efficient as several smaller spaces more closely set. This, however, seems to us to have been too often overlooked. The tenor of petitions has commonly been to request the establishment of a huge park in the environs of some district, or to lay open to the public a greater extent than hitherto of the parks already existing; as of the Regent's Park, for example. It would have been much better to have petitioned that smaller spaces should have been opened *within* the district; and as for the making more of our present large parks

public, on this we really believe that much more than necessary stress was laid. For all the purposes of exercise they were already wide enough; and for those of ventilation, they effected as much when closed to the public as when open. Not, indeed, that we desire to see any part of the parks closed; we allude to the earnestness with which this part of the question was treated, merely for the purpose of illustrating our belief that the greatest benefit of open spaces is commonly overlooked by those who are most anxious to establish them. They should be open spaces, not *near* great cities, but *in* them.

In proof of the same error, we may observe, that while much is said of the necessity of establishing large parks all round London, very little is said against the scheme of building over the largest open space in its very centre. A plan, it is well known, is in agitation, and, unless some urgent representation of its evils be made, will certainly be adopted, of covering nearly the whole of Lincoln's Inn Fields with new Courts of Law. The sole advantage anticipated from the plan is that solicitors will be able more conveniently to attend to the progress of their cases; but surely this is quite an insufficient reason for the probable infliction of unhealthiness on a large surrounding district, already almost as densely inhabited as any other in the metropolis. We shall probably recur to this subject, that we may afford the profession in London an opportunity of manifesting their disinterested anxiety for the health and comfort of their fellow townsmen.

MEDICAL ATTENDANCE ON THE POOR.

WE were glad to hear it stated by Mr. Guthrie, in the course of the lectures lately delivered by him at Lincoln's Inn Fields, that in conse-

quence of the solicitations and representations of the President and Council of the College of Surgeons, the Poor Law Commissioners had agreed to propose a clause to Lord John Russell, to be introduced into the amended Poor Law Bill, preventing the employment in the charge of the poor of any medical person who was not duly qualified as a physician, or surgeon, or surgeon and apothecary, after March 1842, except under very particular circumstances—a statement of which must be annually laid before Parliament. If this be faithfully carried into effect, we believe that about seven hundred gentlemen now employed by the different Union officers will be displaced at the next appointment. Mr. Guthrie also stated, that he had reason to believe that a more just scale of remuneration, particularly for cases of operation, compound fractures, and protracted labours, would be adopted. He added, in conclusion, that the President and Council would not fail to attend to these points, when this Bill should be again considered in Parliament.

COLLEGE OF PHYSICIANS.

NEW FELLOWS.

THE following gentlemen were elected Fellows of the College of Physicians at the usual annual meeting last week, the new measures of reform not having yet come into operation:—

Dr. Blair.	Dr. Macarthur.
Dr. Yelloly.	Dr. Le Fevre.
Dr. Morrison.	Dr. R. Lee.
Dr. Cooper.	Dr. M. Hall.
Dr. Goldie.	Dr. A. T. Thomson.
Dr. C. Forbes.	

In the preceding list are some names which, we imagine, will rather astonish the parties themselves, and which certainly shew the philosophic indifference with which the Fellows regard the opinions expressed out of doors regarding their Collegiate proceedings.

We must add, that we miss some names which we should have been well pleased to see on the list, and the appearance of which would, in our humble judgment, have done credit to the College.

ROYAL MEDICAL & CHIRURGICAL SOCIETY.

Tuesday, June 22, 1841.

THE PRESIDENT IN THE CHAIR.

On Congenital Tumors of the Pelvis. By EDWARD STANLEY, F.R.S., Surgeon to St. Bartholomew's Hospital.

THE object of the paper is to point out various forms of congenital tumor of the pelvis, the discrimination of which becomes important with reference to the question of their removal by operation.

Four cases are related which, it is believed, embrace the chief varieties in the character of these anormal productions, and which are arranged under the following heads:—

First, the cases wherein the tumor is composed wholly of morbid structures, such as solid fibrinous tumors and membranous cysts.

Secondly, the cases wherein the tumor is composed of morbid structures in conjunction with isolated portions of perfectly formed animal organs, having no other relation to the living being with which they are connected than as they are dependent on it for the means of nutrition and growth. These cases, it is added, must be considered to belong to the class of parasitic monsters.

Thirdly, the cases wherein the tumor, being of the nature of spina bifida, consists of a membranous cyst communicating with the theca vertebralis.

Fourthly, the cases wherein the tumor consists wholly, or in part, of membranous cysts communicating with the spinal canal, but exteriorly to the theca, between this and the surrounding bony walls of the canal.

In all the cases here described; and in others to which reference is made, the tumor was attached to the external and posterior part of the walls of the pelvis, and consequently its situation was such as to allow of its removal by operation, provided there was no objection either from the depth of its attachments or from the continuity of any part of it with the membranes of the spinal marrow, or other of the internal organs of the body of the child to which it was united.

In one of the cases here related, the congenital tumor of large size was removed with complete success by the late Mr. Thomas Blizard. The tumor, which is preserved in the museum of the Royal College of Surgeons, consists of distinct solid fibrous substances, and of an isolated portion of intestine, three inches and a half in length, closed at each end, and having at one end a narrower process continued from it of the exact form of an appendix vermiformis. In the progress of the removal of the tumor this intestine was opened, and there flowed

from its interior a fluid closely resembling meconium in its colour and consistence. The author of the paper states that an analogous fact of the production of a fluid, exactly like meconium in its appearance, without the existence of a liver or other distinct hepatic apparatus, occurred to his observation in the dissection of an acephalous lamb, in which, with other malformation, the liver was wanting, and the intestines were filled by a thick fluid, dark coloured, but which, when diluted, presented the yellow colour of bile, but it had not a bitter flavour.

Mr. Macilwain asked, if, in any of the cases related, it had been ascertained whether the condition of the internal viscera was sound.

Mr. Stanley said, that he could speak with certainty only of the first, which he had himself examined. In that, all the viscera were perfectly healthy; and so, it might be fairly presumed, they were also in the subject of the second case, who had lived for thirteen years after the operation.

Dr. Hodgkin said, that he perfectly agreed with the author, in regarding some of these cases as examples of parasitic monstrosity, and he thought that, in this view, a further division might have been made, similar to that which, he believed, could be established among other examples of double monstrosity. Some of these exhibited portions of one foetus included within another, leaving no doubt that here there had been originally two germs, one of which had not been so far developed as the other. But there were others where the foetus being simple in one part of the body, seemed from that part to branch out into a double body; and these were particularly remarkable for that which might be regarded as the secondary part being attached to the other by that part of itself which corresponded to that to which it was attached. And these cases again were distinguished from another class, in which the doubleness was almost universal, as in the Siamese Twins*.

Mr. Stanley inquired whether any members were acquainted with cases illustrative of the fact which he had noticed, of meconium being found in portions of intestine, having no connection with an hepatic apparatus.

Dr. Weatherhead believed, that the liver was not essential to the excretion of bile, and entered at some length into the pathology of jaundice, to prove the probability that, in that disease, the bile was not first secreted, and then again taken into the blood, but that it was not secreted by the liver at

* We believe that this embodies the substances of the opinions which Dr. Hodgkin entertains, but, as he confessed himself, the subject was peculiarly one in which it was difficult to get a clear explanation by words only.

all, but being retained in the blood, was separated from it at the kidneys, skin, &c.

Dr. Robert Lee asked, if, in any of the cases related, it were known what was the arrangement of the umbilical vessels.

Mr. Stanley answered, that, in the two children, the portions of intestine containing meconium were entirely detached from the umbilical vessels of the perfect foetus, and received their blood only from the same vessels as the rest of the tumor. In the case of the acephalous lamb, the arrangement of the vessels was peculiar; the umbilical vein passed at once into the inferior cava, and then gave off a branch which was comparable with the vena portæ; but there was no communication at all between the large vascular trunks, and the arterial and venous systems communicated only through the medium of the capillaries.

A question was then raised, whether the fluid, not having been subjected to strict chemical examination, and not having the bitter taste, though it had the colour of that substance, could be regarded as certainly meconium, and this was left undecided.

Case of fatal Peritonitis caused by effusion of the Bile into the peritoneal cavity through an ulcerated opening in the gall-bladder. By W. BELL, M.D. (Presented by SIR JAMES CLARK, Bart. M.D.)

No discussion followed the reading of this paper. After which the following was read:—

Pathological and Surgical Observations on the Diseases of the Ear. By JOSEPH TOYNBEE, Esq. (Presented by DR. RICHARD BRIGHT.)

The present paper is the first of a series which the author hopes to lay before the Society on the same subject, and contains the details of forty-one dissections of the internal ear in patients who have died in hospitals and infirmaries of various diseases, and of whose faculty of hearing (as to the greater number, at least,) the author was uninformed. The following is a concise view of the state of the cavity of the tympanum in these cases:—

- | | |
|--|----|
| 1. In a healthy state | 10 |
| 2. With simple thickening of the investing membrane | 6 |
| 3. With bands of adhesion passing from various parts of the cavity of the tympanum, most frequently connecting the straps to its circumference | 4 |
| 4. With slight thickening of the investing membrane, accompanied by the existence of adhesive bands | 13 |
| 5. With considerable thickening of the investing membrane and bands of adhesion | 5 |

- | | |
|---|---|
| 6. With suppuration of the cavity of the tympanum | 1 |
| 7. With ankylosis of the base of the stapes to the circumference of the fenestra ovalis | 2 |

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“The large proportions of specimens which are undoubtedly in a diseased state,” says the author, “is very surprising; but it may be less so when I state that many persons whom I have examined, and who have considered that they hear perfectly well, cannot distinguish the ticking of my watch at the distance of two feet and a half, and, in some cases, at four or five inches only, though the same watch can be heard by a healthy ear seven or eight feet from the head. I am, therefore, disposed to believe that the function of the ear is impaired much more frequently than is generally supposed.” The author concludes his paper with an invitation to members of the profession to inspect the preparations described in his paper.

Mr. Cæsar Hawkins said, that a remark had been made by Dr. Wollaston, confirmatory of, or at least related to, the statement of persons being unaware of the extent, or even of the existence of their own deafness; namely, that in striking the several notes of the gamut, before a considerable number of persons, there would be found, as each of the higher notes was sounded, a number to whom they were inaudible.

Dr. Mayo then proposed, that an extraordinary meeting should be held for the reading of the papers which still remained, and a very warm discussion ensued relative to the admission of irrelevant matter in the ordinary debates of the Society. It was ultimately agreed that a meeting should be held on Tuesday, July 6th.

MEDICAL REFORM.

To the Editor of the Medical Gazette.

SIR,

ALTHOUGH so much has been said on medical reform, we confess we have not felt quite satisfied with what has been hitherto urged on that momentous subject: we therefore trust that you will indulge us with a corner in your journal, if you think the opinions entertained worthy of consideration. Our statements will be very concise, perhaps to a fault; but so much has already been published, by *extreme*, and *more moderate* parties, that we are persuaded the medical public, *at least*, will be best satisfied with as short a discussion as possible on a subject so desirable to be accomplished and settled. A very clear exposition was published in that respectable journal, (The Provincial Medical

and Surgical) extracted from the Morning Chronicle of the 6th and 12th of March, where a uniformity of examination is advocated for all practitioners. The talented and zealous author maintains that such a measure would not be a levelling one; we, with deference, think otherwise, and are persuaded that more jealousies would subsist were all to be of one grade, though practising under a different name, than under the present system.

The measures we would venture to recommend to the legislature, and for the corporations to adopt, would be to have a uniformity of *examination* and *qualification* for each grade in the profession in the United Kingdom; afterwards, that every individual should be permitted to practise in his chosen locality. That all physicians of the first grade should of necessity in future take their Bachelor's degree in arts at the universities in England, Ireland, or Scotland, and be obliged to attend the proper medical curriculum before presenting themselves for examination. The surgeons, and surgeon-apothecaries, should likewise attend all that is pointed out by their several corporations, when uniformity in examinations in the United Kingdom is become the law. In expressing ourselves thus, of the latter, it will be understood that we have not that horror which some have expressed of the Apothecaries' Company, (though we have no objection to its being honoured with a different appellation at a future time), but strenuously insist that it has done much in raising the dignity of its members, and of benefitting the public to an extent never before known. By the examination of their candidates, they have sent forth men more generally qualified amongst the community than ever subsisted at any former period. In fact, it is these men who now feel their dignity lowered by the name of *apothecary*, and advocate equalization; and why, we would say, should they not be *curtailed* of this *apothecosis*? Let them be denominated *doctors* of a second grade, (as is open on the Continent), provided they give up pharmacy. The public would thus be less drugged, and the doctor should be remunerated by a specified moderate fee for his attendance. Let the denomination of apothecary adhere to the pharmacien and tradesman only, whose line might be pointed out by legislation. All qualified medical men should be registered with their different corporate bodies; and, if belonging to more than one, it should be so designated. With these few observations thrown together for your approbation, we beg to subscribe ourselves

Yours most faithfully

FILII ÆSCULAPII.

THE FLEXIBLE STETHOSCOPE.

To the Editor of the Medical Gazette.

SIR,

THE "Reply" by Dr. Golding Bird inserted in your GAZETTE of the 18th ult. does not answer the "charge of plagiarism by implication" preferred against him, as he says, in a "note" appended to my paper on "a New Plessor and Plessimeter" published in your GAZETTE of the 11th ult.

To relieve Dr. Golding Bird from this charge, it is necessary that he answer the following questions in an explicit and straight-forward manner:—

1. At what time, and from what source, did Dr. Golding Bird obtain the information of the flexible stethoscope which led him to adopt its use?

2. Had he used the flexible stethoscope in question, before his brother became clinical assistant at the Westminster Hospital?

3. Did he not know, or hear from his brother, that Dr. Burne was in the habit of using the flexible stethoscope at the Westminster Hospital?

The tone and character of the "Reply" need no comment from, sir,

Your obedient servant,

JOHN BURNE.

24, Lower Brook Street,
June 29, 1841.

REPORT ON DR. LOUVRIER'S METHOD OF TREATING ANGULAR ANCHYLOSIS OF THE KNEE.

BY M. A. BERARD.

(Read to the Academy of Medicine.)

[THIS method probably had its origin from the fact of a man who had suffered long from ankylosis of the knee being cured by the sudden wrenching of the parts in an accidental fall. We flatter ourselves that English caution would not venture to adopt any treatment like that of which the report speaks; but as these are not the most prudent days of surgery, we shall give a brief abstract of the paper, as a warning to those who might be inclined to entertain the idea of imitating M. Louvrier's proceedings.]

M. Louvrier commences by padding the limb that is to be straightened, especially about the knee and ankles, with cotton and pieces of old linen, held on by a roller applied from the foot to the middle of the thigh; he then places strong boiled-leather splints round the thigh and leg, and these, being moulded so as exactly to adapt themselves to the limb, he fixes firmly with straps. The foot is placed in a cotton sock,

then in a sandal laid over the ankle, with a very strong sole, and carrying on its posterior third a piece of steel with a mortice. The anchylosed limb being placed on the apparatus, counter-extension is made by a belt attached to the upper part of the thigh, and a strong strap fixed on the lower part of this serves to keep it firmly down upon the seat in which the patient sits. The thigh and leg are thus placed in a kind of leather gutter, the edges of which are laced over the front of the limb, and two long and very strong metallic splints are then fixed to the sides of the limb, each being composed of two branches with a hinge uniting them opposite the knee-joint.

Extension is made by means of strong cord made of catgut rolled round a winch, which is fixed at the end of the board in which the patient is set. This cord acts by pulling on the foot; but, at the same time, and by means of a very ingenious piece of mechanism, it exerts a constant and forcible pressure on the anterior part of the knee during the whole of the operation. Thus the straightening of the limb is effected by a double pressure; on the one hand, by the traction exercised on the two extremities of the angle; and on the other, by the pressure exerted upon its apex.

When all the parts of the apparatus are properly put on, M. L. works the winch by means of a crank, which he continues to turn till the limb is completely straightened. The whole extension is commonly made in the short space of from 25 to 30 seconds; and when the muscles were much retracted before the operation, the leg is often kept thus extended for half a minute before letting it go again.

This extraordinary method was adopted after much hesitation, and after repeated trials on the dead body, and of authentic reports of its having been successfully employed on some patients in the provinces, on a young man, who insisted on its being tried, in the Hospital Necker. No evil followed it; and then another patient came, who was also not injured by it. A third, fourth, and fifth, were successfully operated on, and then the plan became quite fashionable; "almost every week the apparatus *fonctionnait*," at some hospital or in private practice, till at last reverses came. Two patients died of the consequences of the operation, and it shortly fell into as great disrepute as just before it had been in favour. The first death was consequent on the integuments of the hand being torn across, exposing the popliteal nerve, and accompanied by rupture of the crucial ligaments and other deep parts in the neighbourhood of the joint. Intense inflammation succeeded, the tibia and fibula were drawn back upon the femur, and the patient died in three weeks after the injury. In a second case,

the popliteal artery was torn across, and the patient lost the foot and the lower part of the leg by the consequent gangrene. In the third, the patient died six weeks after the operation from the sloughing and other injuries (including a comminuted fracture of the femur) produced by the excessive pressure exercised on the front of the joint. In another case, in which the patient died of an accidental illness some months after the operation, the internal condyle of the femur had been fractured and become adherent to the surface of the tibia.

M. Berard justly remarks that we cannot but feel astonished that out of 21 patients, on whom M. Louvrier operated in Paris, only 3 should have suffered severe and irreparable injury, and that it can only be explained by the fact that the parts about joints may be extremely damaged, without leading to any important results, if the skin around them has not been wounded. But indeed the histories of the most favourable cases are not such as to offer any temptation to the performance of the operation, even if it could be done safely. The pain produced by it is agonizing, and sometimes lasts for several hours afterwards; and in none of the patients whom M. Berard saw were the voluntary movements of the knee-joint, or the complete straightness of the limb, restored. Although the condition of all the patients was rather improved, yet they could walk only with great difficulty, and not at all without the aid of some apparatus, or of a stick or crutch.—*Gazette Médicale, Mai 22, 1841.*

TREATMENT OF INVERSION OF THE NAIL.

BY DR. PAYAN,

Surgeon to the Hospital of Aix.

DR. PAYAN was led to investigate the subject of inversion of the nail, in consequence of having witnessed, in many cases, the inefficiency of the two modes of practice now generally followed, viz. those of Dupuytren and M. Lisfranc. The operation of M. Dupuytren is speedy and easy of execution, but is too frequently followed by a relapse. The same may be said of Lisfranc's operation, but upon the whole it is more successful than the first mentioned. Neither of these operations, however, strike at the root of the disease: the matrix of the nail is left untouched, and consequently, when it is reproduced, it frequently re-assumes its abnormal direction. To effect a radical cure, then, it is necessary to destroy the matrix of that portion of the nail which has become inverted.

M. Payan was led to employ escharotics with this view, from having observed that in

some cases in which the potassa fusa was applied to the soft parts covering the nail, according to the plan of Levrat Perraton, a radical cure was effected in a different manner from that which they had in view, by the caustic, from its deliquescence, spreading and destroying the matrix.

The following is the mode in which M. Payan first applied the escharotic:—The toe and its nail were covered with adhesive plaster, except a small part, including the base of the inverted portion of the nail and its matrix, which were left uncovered; this was done with the view of preventing the action of the caustic from extending to the surrounding parts. A small portion of potassa fusa about the size of a pea was then applied to the part left uncovered, and retained there for eight or ten hours. By this means an eschar was formed, involving the matrix of the inverted portion of the nail, the root of which, on the separation of the eschar, was seen black and detached. The cure, he says, may now be hastened by the removal of the inverted portion, and there is a certainty that it cannot be reproduced, as the matrix is destroyed. The cure is completed, he says, in from twenty-five days to a month and a half.

In his later cases M. Payan has used the Vienna powder, which consists of equal parts of potassa c. calce and quick lime, instead of the potassa fusa. This he makes into a paste with a few drops of alcohol, and applies in the same manner as the potassa. This paste possesses several advantages over the potassa:—its action is more easily limited,—it is more speedy in its operation, twenty or twenty-five minutes being sufficient,—the pain, he says, is less, and the eschar separates more speedily.—*Revue Médicale, July, 1840, and Edin. Mon. Journ.*

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Friday, June 18.

John James Cathcart.—Edward Hoskins.—George Nathaniel Grane.—Orlando George Bell.—Henry Young.—Edwin James Isbell.—Hugh Crawford Walshe.—John Barton Harrison.—William Smith.—James Williams.—John Thomas.—John Fenton.—James Crocome.

Friday, June 25.

J. T. Brumwell.—T. Bodkin.—J. W. C. Pennell.—P. Lavery.—A. Fuller.—J. Johnston.—W. B. Sellers.—G. F. Harpur.

DR. KNOX, OF EDINBURGH.

WE are requested by Dr. Knox to state, that there is no truth whatever in the report of his intending to leave this country. He has not, and never had, any intention of removing from Edinburgh; and the statements

on the subject which found their way into a London weekly journal must have been founded on erroneous information.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 19th June, 1841.

Small Pox	12
Measles	12
Scarlatina	16
Hooping Cough	28
Croup	9
Thrush	5
Diarrhoea	6
Dysentery	1
Cholera	0
Influenza.....	2
Typhus	19
Erysipelas	1
Syphilis	1
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	139
Diseases of the Lungs, and other Organs of Respiration.....	252
Diseases of the Heart and Blood-vessels	21
Diseases of the Stomach, Liver, and other Organs of Digestion	60
Diseases of the Kidneys, &c.....	2
Childbed	3
Ovarian Dropsy	0
Diseases of Uterus, &c.....	4
Rheumatism	2
Diseases of Joints, &c.....	3
Ulcer	0
Fistula	0
Diseases of Skin, &c	1
Diseases of Uncertain Seat	89
Old Age or Natural Decay.....	39
Deaths by Violence, Privation, or Intemperance	21
Causes not specified	4

Deaths from all Causes 750

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

June.	THERMOMETER.		BAROMETER.	
	from			
Wednesday 23	48 to 69		29.92 to 29.80	
Thursday . 24	43 65		29.72	29.64
Friday . . 25	53 64		29.51	29.49
Saturday . 26	54 65		29.62	29.69
Sunday . . 27	53 67		29.86	30.08
Monday . . 28	51 59		29.97	29.78
Tuesday . 29	49 63 -		29.71	29.78

Winds, South by West, and West.

On the 23d, generally clear; rain in the afternoon. The 24th, generally clear; a very heavy shower of rain between 11 and 12, A.M. The 25th, morning overcast, with heavy rain; otherwise clear. The 26th, morning cloudy, wind boisterous; afternoon showery, sunshine at times; evening clear. The 27th, evening clear, otherwise cloudy; raining frequently during the morning. The 28th, overcast, raining heavily and frequently during the day. The 29th, clear, except a heavy clap of thunder and rain from the East and S.E. from about 6 till after 7, P.M. Rain fallen, 1 inch, and 7-10ths of an inch.

NOTICE.

Dr. Borrett's paper is in hand.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, JULY 9, 1841.

LECTURES

ON THE

PRINCIPLES AND PRACTICE OF PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XLI.

Ague, continued. Speculations respecting its periodicity. Habits and properties of the malaria: most noxious at night; lies near the ground; is carried along by winds; cannot pass across water; attaches itself to trees; is diminished by the increase of cultivation and of population. Ultimate effects of the poison on the body. Ague formerly thought salutary. Prognosis. Propriety of stopping the disease.

You will remember the progress we made, at our last meeting, in the subject of intermittent fever. I described the ordinary phenomena of a paroxysm of ague; and afterwards mentioned certain unusual symptoms with which it is sometimes complicated. The three principal types of ague were also delineated; the quotidian, the tertian, and the quartan: as well as their respective characters, and intervals, and varieties, and changes of type. I spoke too of the predisposing causes of intermittent fever, which may all be briefly included under the head of circumstances that tend to debilitate the body: the strongest predisposing cause of all being a former attack of the disease. And I began to consider the great exciting cause of agues and aguish fevers; the malaria. I first directed your attention to the circumstances under which the malaria appears to be evolved. Since the time of Lancisi it had been very generally supposed that the humid putrefaction of vegetable

substances was necessary to the production of this peculiar and wide spread poison; and that heat accelerated the putrefactive process. That was Dr. Bancroft's opinion. That also is (I believe) the opinion held, and stated in lectures, by many pathologists at the present time. I shewed you, from facts which rest upon Dr. Ferguson's authority, that this opinion is founded in mistake: that the products of vegetable decay and decomposition may and do often coexist with malaria, but are distinct and separable from it, and by no means essential to its formation. There is reason to believe that the flooding of a porous earthy surface with water, and a subsequent drying of that surface under a certain degree of heat, constitute the sole or main conditions of the generation of the poison. We found that the effects of the malaria are modified by the temperature of the place: that in low and hot situations it may give rise to an affection not distinguishable in its symptoms from yellow fever; and that in proportion as the locality is higher and cooler, the fever tends to assume first the remittent, and then the intermittent type: that the period of incubation—the period which intervenes between exposure to the malaria and the invasion of the fever—is extremely variable in duration: that the poisonous effluvia affect strangers more certainly and more severely than natives of the place: that persons may become in some sort seasoned to the malarious districts: but that, with the exception of the negroes in the West Indies, the inhabitants of places much infested with the peculiar miasmata, are feeble, and sickly, and short-lived.

Periodicity of the fit.—There was one point which I briefly adverted to, and dismissed perhaps too unceremoniously: I mean the very curious fact of the *regular periodic recurrence* of the paroxysms of intermittent fever. I ought, I think, to have informed you of the views which pathologists have entertained respecting the explanation

of that singular circumstance; although it must be confessed that the solution of the phænomenon is still to be sought for. A great number of persons have tried their hands, however, upon this question. Many of the earlier attempts at explanation are either quite hypothetical, or totally insufficient and illogical. Willis ascribed the intermission to a periodic development of *fermentable matter* in the *blood*. But if any such development took place (of which we have no evidence) we should be no nearer the mark: the question would still recur, why the development of this matter should happen *periodically*: and the same remarks apply to various other so-called explanations brought forward by different writers of considerable reputation. Reil referred the intermittence of fevers to some *general law of the universe*; by which he meant, I believe, some vague generalization of such facts as the alternation of light and darkness, the periodic recurrence of the seasons, the ebbing and flowing of the tides, the succession of appetite and satiety, of the states of sleeping and waking, and so on: but this evidently is no explanation at all. M. Bailly offers a very singular conjecture upon the subject: he attributes the periodic phænomena to the modification necessarily induced in the human system, and particularly in the function of circulation, by the alternate change of position from the upright to the recumbent, and from the recumbent to the upright, every twenty-four hours; and he adduces in corroboration of this notion the alleged fact that animals, which undergo no such oscillation of posture, are not subject to intermittent fevers: but this is said not to be a fact. Rodet and Charpentier affirm that horses are liable to such complaints. Dr. Macculloch refers to the case of a dog which laboured under a regular tertian ague for some years; the cold paroxysms taking place always at three o'clock in the afternoon. Even if this were not so, M. Bailly's theory fails to account for the occurrence of *continued* fevers. If his views were correct, then we might avoid having ague by refraining from these changes of position from the vertical to the horizontal during sleep, and back again upon awaking. Recently M. Roche has put forth the opinion that the attacks of ague are periodic, because the *causes* of them are periodic. And if this could be made out, the conjecture would carry with it some shew of reason. He observes that the spring and the autumn are the seasons in which intermittent fevers chiefly break out, especially the autumn: and that during those periods there is a very sensible difference in the temperature and humidity of the atmosphere by day and by night, and even within the space of three or four hours; and that a consequent alterna-

tion of action and reaction is thus produced in the human body, and soon becomes an established habit. Throughout a part of the twenty-four hours, the operation of the miasmata is slight, or not manifest at all; while during another part of that period it is in full energy, and at about the same time daily. The emanations (which he conceives to proceed from putrefying vegetable matter) are most abundantly disengaged during the hottest part of the day; these watery effluvia are dissolved by the warm air to a certain amount; but after sunset, they are again deposited, and deposited the more copiously in proportion to the coldness of the atmosphere at that time: and coming in contact with the surface of the body, with the mucous membrane of the air passages, and perhaps also with that of the digestive organs, and being absorbed by those surfaces, it occasions the phænomena which constitute an ague fit. The influence of the miasmata being intermittent, we need not wonder, he says, that their effects should be intermittent too: and then he goes on to ascribe the repetition of the paroxysms, after the cause has ceased to be applied, to that tendency observable in the animal system to reproduce certain actions, simply because they have been produced before; in one word, to the effect of *habit*. At length the habit wears out, which accounts for the spontaneous recovery of those who are removed from the malarious district.

It seems to be a very serious objection to M. Roche's theory, that the disease does not shew itself, sometimes, for weeks or months after the patient has been exposed to the miasmata. His theory fails altogether also to account for the different *types* of intermittent fever. The differences of type are indeed *opposed* to the theory.

After all it is most probable that Cullen had discovered a *part* though not the whole of the truth respecting the periodicity of intermittent fevers, when he ascribed it to some law of the animal economy whereby it is subjected, in many respects, to a diurnal revolution. "Whether this depends," he says, "upon the original conformation of the body, or upon certain powers constantly applied to it, and inducing a *habit*, I cannot positively determine; but the returns of sleep and watching, of appetites and excretions, and the changes which regularly occur in the state of the pulse, shew sufficiently that in the human body a diurnal revolution takes place." But he also is much perplexed with the differences of type; and all that he can say on that point amounts to this—that as the three principal types observe, severally, the same time of day for their accession, and as quartans and tertians are apt to become quotidians, these to pass into the state of remittents, and these last to

become continued ; and that as even in the continued form daily exacerbations and remissions are generally to be observed—all this marks the power of diurnal revolution.

A most interesting experiment, as it appears to me, performed by M. Brachet upon himself, shews in a strong light the influence of acquired habit in continuing certain unnatural states of the system when once they have been originated : the experiment connects itself also with the peculiar phenomena of intermittent fever. Towards the end of the month of October, in the year 1822, M. Brachet took a cold bath, at midnight, for seven nights in succession, in the river Saone. On the first occasion he remained a quarter of an hour in the river ; on the second half an hour ; till at length he was able to stay in the water a full hour at a time. After each bath he betook himself to a warm bed, and in a short time became affected with considerable heat, followed by copious perspiration, in the midst of which he fell asleep. At the end of the seven days M. Brachet ceased to repeat this experiment : but what was his surprise at finding on the following nights, between twelve and one o'clock, that all the phenomena of a true ague fit appeared in due order and succession ! As, however, this artificial paroxysm was not very severe, and as he felt quite well during the day, M. Brachet determined not to interfere with it ; but to observe the result. It recurred six times with great regularity. On the seventh night after he had omitted the baths, he was summoned, towards midnight, to a woman in labour : the ride to her house heated him, and on his arrival he kept up the heat by placing himself before a large fire, and from that time the febrile phenomena ceased to recur.

The facts and theories which I have thus brought roughly together, in respect to the periodicity of agues, are not without interest, but they shew that we have yet much to learn on this subject. Granting that habit may have its share in continuing the regular recurrences, we want some explanation of the return of the second and third fit, after certain determinate intervals, to give a *beginning* to the habit. In respect to the quotidian, Dr. Cullen's *diurnal revolution* might come to the rescue ; but this principle evidently will not apply to the tertian type. I know of no two-day, or bidual habit. And the objection holds still more strongly in regard to quartans. Indeed in quodidians themselves there is much difficulty in applying the explanation, for though by anticipating, or postponing, they *may* come on at different hours of the day, yet their usual and natural paroxysms occur, not in the evening, but in the morning, when, on the principle of diurnal habit, there

should be the *least* tendency to exacerbation of febrile action*.

I pointed out, in yesterday's lecture, the favourite *habitats*, if I may so speak, of the malarious poison. I have still a few observations to make respecting its ascertained habits and properties. Some of the laws to which it is subject are of great practical importance, and ought to be popularly known : much more ought every medical man to be familiar with them.

The malaria most noxious at night.—In the first place, all malarious places are (as I have already hinted) much more dangerous *at night* than in the day time. Whether the poison be then more copiously evolved, or whether it be merely condensed and concentrated by the diminished temperature, or whether the body is at that time more susceptible of its influence, it certainly is most active and pernicious during the hours of darkness. To *sleep* at night in the open air in such places is almost to ensure an attack of the fever. Lancisi was quite aware of this, and devotes a chapter to the question. “*Cur juxta paludes noctu præsertim indormientes magis quam vigilantes lædantur ?*” It has repeatedly been observed among the crews of ships, when off a malarious coast, that the sailors could go on shore in the day to cut wood, or for other purposes, with impunity ; while the men who remained on shore through the night guarding the water casks, were many or all of them seized with the fever. Take one instance as a sample of many. It is recorded by Dr. Lind. In 1766 the Phoenix ship of war was returning from the coast of Guinea. The officers and ship's company were perfectly healthy till they touched at the island of St. Thomas. Here nearly all of them went on shore. Sixteen of the number remained for several *nights* on the island. Every one of them contracted the disorder, and thirteen of the sixteen died. The rest of the crew, consisting of 280 men, went in parties of twenty or thirty on shore in the day, and rambled about the island, hunting, shooting, and so on : but they returned to the ships at night ; and not one of those who so returned suffered the slightest indisposition. Exactly similar events occurred the following year, with the same ship, at the same place, where “she lost eight men out of ten, who had imprudently remained *all night on shore* ;” while the rest of the ship's company, “who, after spending the greatest part of the day on shore, always returned to their vessel before night, continued in perfect

* I would beg to refer the reader to Dr. Holland's interesting chapter (in his *Medical Notes and Reflections*, published since these lectures were delivered) “On Morbid Actions of Intermittent kind.”—T. W.

health." Many more examples of the same kind are stated or referred to by Dr. Bancroft in his book *on the Yellow Fever*: a book which is rich in information respecting the malaria.

The reapers in the "Campo Morto"—a well-named part of the Maremma which I yesterday mentioned—are permitted to sleep for two hours about noon. They do so at that time without danger: but when the dews of evening have fallen down upon the earth, which serves them for their bed, it is then that the poison puts forth its most deadly power. Upon this principle Lancisi admonishes those who in summer travel through the Pontine marshes, not to do so *at night*, as many had been accustomed to do, in order to avoid the greater heat of the day: and similar advice is still given at Rome to all strangers. Though the passage requires but six or eight hours, there are numerous instances of travellers who, in consequence of their having crossed these fens during the night, have been attacked with violent and mortal fevers.

The practical lesson to be derived from a knowledge of this fact is too obvious to dwell upon. In malarious countries the open air at night must be avoided. "Early to bed" is always a good and wholesome rule; but the other half of the proverb "early to rise" becomes, in such countries, an unsafe precept. At least it is hazardous to *leave the house early*.

It tends downwards.—Secondly, the malaria loves the ground. Whether this results from its specific gravity; or from its adhering to the moisture suspended in the lower strata of the atmosphere; or from some peculiar attraction for the earth's surface; I cannot tell you. There is reason to suppose that the poison combines somehow, or becomes entangled, with fog: and fogs usually brood and settle, at night especially, upon the surface. This may be one reason why *lying down* to sleep in the open air at night is so very perilous. The lower rooms of the same house may contain the noxious effluvia, while the upper are free. "In all malarious seasons and countries," says Dr. Ferguson, "the inhabitants of *ground floors* are uniformly affected in a greater proportion than those of the upper storeys. According to official returns during the last sickly season at Barbadoes, the proportion of those taken ill with fever in the lower apartments of the barracks exceeded that of the upper by one-third, throughout the whole course of the epidemic. At the same time it was observed that the deep ditches of the forts, even though they contained no water—and still more the deep ravines of rivers and water-courses—abounded with the malarious poison." Dr. Hunter, in his work on the dis-

eases of the army in Jamaica, says "the barracks of Spanish Town consist of two floors, the first upon the ground, the second on the first. The difference in the health of the men on the two floors was so striking as to engage the attention of the Assembly of the island; and upon investigation it appeared that *three* were taken ill on the ground floor, for *one* on the other. The ground floor was not therefore used as a barrack afterwards." Mr. Ralph, in a table printed as an appendix to a paper of Dr. Ferguson's in the eighth volume of the *Medico-Chirurgical Transactions*, states the result of an enquiry into the comparative healthfulness of the upper and lower apartments of barracks in Barbadoes, to have been that the individuals residing in the lower apartments were attacked in the proportion of two to one of those living in the upper: and with certain apparent exceptions, which I shall notice presently, experience is uniformly in favour of the proposition that the poison is most prevalent and destructive near the surface of the earth, and does not rise high into the atmosphere.

To specify the sanitary precautions dictated by an acquaintance with this property of the malaria, must be quite superfluous.

Is carried along by currents of air.—Thirdly, the malaria is *moveable by the wind*. It is capable, therefore, of being carried *from* the spot where it was generated; and *to* other places which might else be free from it and healthy. In this respect it is analogous to a heavy fog or vapour: and, in some cases, it is accompanied by a palpable mist; to which, perhaps, it may cling. The following passage relative to this subject occurs in Bishop Heber's Journal. "From Cheeta Talao our road lay through a deep and close forest, in the lower parts of which, even in the present season, the same thick milky vapour was hovering as that which I saw in the Terrai, and which is called *essence of owl*." This Terrai is the region which I mentioned in the last lecture as being so pestiferous, that it is deserted, during certain parts of the year, by every living creature.

This conveyance of the poison, like a cloud or fog, from one part of the surface of the ground to another, it is very important to attend to in all places; and especially in tropical climates, where the wind blows for a long time together from the same quarter. We are thus enabled to account for the apparent exceptions to the last-mentioned property of the malaria, viz. its preference of low to elevated situations. You will readily understand how the miasmata may roll up, and hang accumulated upon, the side of a hill towards which a current of air sets steadily from or across a neighbouring marsh. Nay, the poison may be thus blown

over a hill, and deposited on the other side of it. In this way, I presume, are to be explained the following curious facts, related in Dr. Ferguson's paper.

"The beautiful port of Prince Rupert's, in the island of Dominica, is a peninsula which comprehends two hills of a remarkable form, joined to the main land by a flat and very marshy square isthmus *to windward*, of about three-quarters of a mile in extent. The two hills jut right out on the same line into the sea, by which they are on three sides encompassed. The inner hill, of a slender pyramidal form, rises from a narrow base nearly perpendicular, above and across the marsh from sea to sea, so as completely to shut it out from the port. The outer hill is a round-backed bluff promontory, which breaks off abruptly, in the manner of a precipice above the sea. Between the hills runs a very narrow clean valley, where all the establishments of the garrison were originally placed; the whole space within the peninsula being the driest, the cleanest, and the healthiest surface conceivable. It was speedily found that the barracks in the valley were very unhealthy; and to remedy this fault, advantage was taken of a recess or platform near the top of the inner hill, to construct a barrack which was completely concealed by the crest of the hill from the view of the marsh on the outside, and at least three hundred feet above it: but it proved to be pestiferous beyond belief. In fact no white man could possibly live there, and it was obliged to be abandoned. At the time this was going on, it was discovered that a quarter which had been built on the outer hill, on nearly the same line of elevation, and exactly five hundred yards farther removed from the swamp, was perfectly healthy; not a single case of fever having occurred in it from the time it was built."

There is a striking anecdote given by Lancisi, showing, on a small scale, the effect of the wind in carrying the malaria with it. Thirty ladies and gentlemen had sailed to the mouth of the Tiber on an excursion of pleasure. Suddenly the breeze shifted to the south, and began to blow over a marshy tract of land situated to windward of them. Twenty-nine of the thirty were immediately after attacked with tertian ague. So also Humboldt informs us that the town of Cariaco is afflicted with intermittents by the north-west wind conveying across it the miasmatic emanations of the Laguna of Campona.

And as the wind will thus transport the malaria to a distance, and thereby render a spot unhealthy which naturally would not be so: it also is often of service in clearing the poison from other places, and preventing its concentration.

A knowledge of these facts ought to be

valuable in determining the choice of encampments, and of sites for dwelling houses in aguish districts. Settlers in hot climates, especially where trade-winds prevail, would do well to avoid founding towns on the lee side of any swampy or suspicious ground. The outlets of rivers are commonly selected, for the convenience of commerce: and there is often a right and a wrong bank. I believe that most of the principal towns in the West Indies are built, for the advantage of the outward-bound vessels, upon the western, or lee side of the islands.

It cannot pass across water.—Fourthly, it is a singular, but well-ascertained fact, that the miasmata lose their noxious properties *by passing over even a small surface of water*. Probably they are absorbed by it. And this is another proof of their tendency downwards. Many instances have already been referred to, where some of the crew of a ship have landed on a malarious coast, and have all been attacked by the fever; while the rest of the sailors, who remained on board, continued all healthy and well, though the ship was close to the shore. You could not have a better or more striking example of this than what took place at Walcheren. "Not only the crews of the ships in the road of Flushing were entirely free from the endemic; but also the guard-ships which were stationed in the narrow channel between this island (Walcheren) and Beveland. The width of this channel is about six thousand feet, yet, though some of the ships lay much nearer to one shore than to the other, there was no instance of any of the men or officers being taken ill with the same disorder as that with which the troops on shore were affected." This Sir Gilbert Blane has told us; and it is curious that Sir John Pringle made the very same remark in the very same place in 1747. He is speaking of the diseases of the campaign in Dutch Brabant; and especially of four battalions which had remained for some time in Zealand: and he says, "But Commodore Mitchel's squadron, which lay all this while at anchor in the channel between South Beveland and the island of Walcheren, in both which places the distemper raged, was neither afflicted with the fever nor the flux, but amidst all that sickness enjoyed perfect health; a proof that the moist and putrid air of the marshes was dissipated, or corrected, before it could reach them."

It is probable that this peculiarity has led to an erroneous and contracted estimate of the space through which the poisonous effluvia can be carried, upon land, by the wind. Although the distance to which they are capable of being so conveyed, without losing their morbid power, has never been precisely defined, there can be no doubt that it is considerable. In Italy, according to

Dr. Macculloch, the poisonous exhalations of the lake Agnano have been ascertained to reach as far as the convent of Camaldoli, situated on a high hill three miles distant.

Attaches itself to trees.—Fifthly, another remarkable property of the marsh poison, is its attraction towards, and its adherence to, the foliage of lofty umbrageous trees: so that it is very dangerous, in malarious places, to go under large thick trees, and still more dangerous to sleep under them. But this property, thus a source of danger to those who are ignorant of it, affords, when known and rightly made use of, a mode of protection and remedy against the influence of the miasmata. In the territory of Guiana, where large trees abound, the settlers live fearlessly, and unhurt, close to the most pestiferous marshes, and to leeward of them, provided that a screen or belt of trees be interposed. New Amsterdam, in Berbice, lies on the lee side of an immense swampy forest, in the direct tract of a strong trade wind that blows night and day, and pollutes even the sleeping apartments of the town with the stench of the marshes; yet it brings no fevers. The inhabitants are well aware that it would be almost certain death for an European to sleep, or even to remain after nightfall, within the verge of the forest. To cut down the trees would not only be a perilous operation in itself; but would let in pestilence to the town.

This property also of the malaria, as well as the use to which it may be turned, was known to Lancisi. He describes the vast increase of agues and remittent fevers in Rome during the summer of 1695, after a great overflowing of the Tiber, by which the lower part of the city, and the fields adjacent, had been inundated in the preceding winter. The bad effects of this flood were felt throughout the whole of Rome, with the exception of one particular quarter, which was protected by a belt of trees around it. Lancisi even addressed a remonstrance to the Pope against a project which was entertained of felling some wood near the Pontine marshes, between them and the city. He endeavours to show that woods and groves were first made sacred on account of their conservative influence in this way, to prevent their ever being cut down.

It would appear, from the facts I have just been detailing, that dwellings unfortunately built in the vicinity of marshes, might sometimes be rendered safe and salubrious by encircling them at a little distance by a hedge of trees—or (perhaps) even by drawing round them a broad moat of water. Such experiments deserve at least a fair trial.

It is diminished by cultivation.—Sixthly, the production, and consequently the effects, of the malaria are prevented, or lessened, by cultivation. It is to this that the diminution

of agues in this country is mainly attributable. The fenny lands have been drained; and many of them brought under the plough. Dr. Craigie states that East Lothian, in Scotland, was at one time so productive of malaria, that it was quite an expected thing that the reapers in harvest should be attacked with ague: but that now, in consequence of the perfect tillage, and the numerous tracts of wood with which the country is covered, that disorder is quite unknown there. Conversely, in regions which have been suffered to fall out of cultivation, intermittent and remittent fevers multiply. The more thoroughly any country is cultivated, the more fully, in general, is it *peopled* also: and in many places the prevalence of these fevers has been observed to diminish and increase with the increase and diminution of the population. *Ceteris paribus* agues are much less common in large towns than in country villages. This has been oddly enough accounted for by saying that populous cities are so full that there is no room for the malaria. A much more rational and probable explanation is that which ascribes the freedom of crowded towns, and thickly inhabited districts, to the number of fires burned in them.

Many instances might be adduced to shew that the more any place, naturally productive of malaria, is depopulated, the more evident does the power of the poison become. The Italians date the introduction of the malaria into the Maremma, from the great plague in the sixteenth century, since which period the inhabitants of that district have never been sufficiently numerous to counteract the bad air which increases as population and agriculture diminish.

Bishop Heber, in the narrative I quoted before, bears testimony to facts of the same kind with those I have now been stating. He says, “At the foot of the lowest hills, a long black level line extends, so black and level, that it might seem to have been drawn with ink and a ruler. This is the forest, from which we are still removed several coss, though the country already begins to partake of its insalubrity. It is remarkable that this insalubrity is said to have greatly increased in the last fifteen years. Before that time, Ruderpoor, where now the soldiers and servants of the Police Thanna die off so fast that they can scarcely keep up the establishment, was a large and wealthy place, inhabited all the year through, without danger or disease. The unfavourable change is imputed by the natives themselves to depopulation. The depopulation of these countries arose from the invasion of Meer Khân, in 1805. He then laid waste all these Pergunnahs, and the population, once so checked, has never recovered itself.”

Ultimate effects of the poison.—When

persons having the intermittent fever are unable to leave the unhealthy situation in which they have been exposed to the influence of the malaria—and especially when they are placed under unfavourable circumstances in respect to food, and clothing, and shelter—the disease is apt to become exceedingly serious, leading to disorder of the sensorium, and great disturbance of the abdominal viscera, even in the intermissions; sickness, diarrhoea, dysentery, diseases of the liver. In Zealand, the biliary functions suffer so much during the complaint, that it is commonly known among the inhabitants of that country under the name of the *gall fever*. The frequent unnatural concentration of the blood in the internal parts may afford a reasonable explanation of these phenomena. When death takes place, morbid appearances present themselves such as might be expected: hepatic alterations; inflammation and ulceration of the mucous membrane of the alimentary canal: but the most characteristic morbid condition produced by repeated attacks of intermittent fever consists in enlargement of the spleen; with or without induration of its substance. That viscus is sometimes enormously increased in bulk, so as to be *felt*, and even its outline *seen* through the integuments of the abdomen. It has been known to weigh nearly eleven pounds. So common is this state of the spleen, that it is familiar to the observation of the vulgar, who have even given it a name: it is called among the inhabitants of the fenny parts of this country, the *ague cake*. I believe that whenever the abdominal circulation is much embarrassed, and the abdominal veins gorged, as they must be during the cold stage of an intermittent, the spleen in particular becomes distended with blood. Constantly we see this happen when the passage of the blood through the portal vessels is impeded by disease of the liver. Now this distension may, perhaps, not thoroughly subside at once. If the paroxysms of ague be frequently repeated, we may understand how the spleen may become fuller of blood on each successive occasion. It may be that a portion of the blood coagulates; or that inflammation of a slow kind is set up in the stretched covering of that organ. At all events, this is a very common sequel of ague: and it can scarcely be doubted that the repeated congestions of the internal vessels and viscera are the determining causes of the ague cake. Independently, however, of the paroxysms of ague, there is abundant evidence to shew the injurious influence of the malarious districts upon the general health. In this country such effects are not much seen; but in places where the malaria is more constantly and abundantly present, the race of the inhabitants deteriorates. Their stature is small;

their complexion sallow and yellowish; they are prematurely old and wrinkled; even the children early acquire an aged aspect; and the spirits and intellects of those who dwell in these unhealthy spots are low and feeble, and partake of the degeneration of their bodily qualities. It is a strange thing, therefore, that a notion should ever have prevailed, of the *salutiferous* effects of an attack of ague. But such a notion may be traced from very nearly our own times up to the earliest records of physic. “The late Dr. James Sims, who was a physician of some note in this town, felt convinced, at the commencement of the illness that terminated his life, that he should recover if he could catch an ague: and he went down into one of the marshy districts expressly for that purpose; but returned to London without having succeeded, complaining that the country had been spoiled by draining, and that there were no agues to catch. The superstitious Louis the XI. entertained a similar opinion, and prayed to the Lady of Selles that she would confer upon him a quartan ague. Our monarch, James the First, had more sensible notions on that score. There is an old English proverb which says, ‘An ague in the spring, is physic for a king;’ and when this was repeated to him by his courtiers, he being then ill of that disease, he answered that the adage might be applicable to a young man, but that it would not do for an old one like him. In fact, as I mentioned before, he died of his ague. The same doctrine has, however, been handed down to us by the father of physic himself. Hippocrates says, in the 57th Aphorism of his 4th Section, *υπο σπασμον, η τετανου ενοχλουμενω, πυρετος επιγενομενος λυει το νοσημα*. And Celsus, in his capital digest of the medical knowledge of his time, preserves the same opinion, with some apparent astonishment that it should be true. ‘Denique ipsa febris, quod maxime mirum videri potest, sæpe præsidio est.’ I recollect hearing Dr. Graham, the professor of botany in Edinburgh, relate the following anecdote in one of his clinical lectures:—His brother was intimate with the professor of natural history at Cremona; and this gentleman was resolved to put the truth of the aphorism that I have quoted from Hippocrates to the test. Accordingly he sent a patient afflicted with *epilepsy*, to pass a night or two in a marshy place, where the malaria was known to be so abundant, and so powerful, that few escaped ague who were there exposed to its influence; and the two-fold design succeeded admirably. The patient got an ague, and lost his epilepsy. The worthy professor contented himself with moderating and keeping in check the new complaint, thus intentionally produced, for a period of six months, when he administered its *coup de grace* in a

few doses of Peruvian bark ; and the epilepsy never returned. If I had believed that this could have been any thing more than a mere coincidence, I should have told you of it before, when I was speaking of the treatment of epilepsy. I should rather imagine that the notion thus prevalent for so long a time, that ague had a salutary tendency, and that it was wrong to stop it too soon, originated in the difficulty which physicians found in stopping it, before its cause was so well understood, and the specific for it was discovered. They found it obstinate under the feeble and inert methods then employed, and therefore they endeavoured to persuade their patients, and perhaps themselves also, that the disease had better proceed a certain length.

I have very little to say, in addition to what you must have inferred from what I have already said, as to the *prognosis* in intermittent fevers. In cold countries, such as ours, it is almost always favourable. Of course it will be modified by the previous condition of the patient : if he was beforehand the subject of serious organic visceral disease ; or if he be very old or infirm ; the supervention of ague may destroy him. But to persons of tolerable health and strength, prior to the setting in of the ague we may confidently promise a *cure*. In warm countries intermittent fevers are much more dangerous : they are sometimes very rapidly fatal. They are often accompanied by most severe affections of the head, stupor, delirium, convulsions ; and of the alimentary canal, diarrhoea, sickness, and not unfrequently the black vomit. They are prone also, in those climates, to run into the remittent or continued form ; and this tendency is shewn by long protracted paroxysms, or by the anticipation or doubling of the paroxysms. In all countries quartans are cured with more difficulty than either tertians or quotidians. And quartans are most common in the autumn : and accordingly autumnal intermittents are more pernicious and intractable than the vernal. This fact has passed into a proverb in Italy ; which proverb has been thus translated into Latin, "*Febris autumnalis—vel est longa, vel lethalis.*" The longer intermittents have lasted, the more difficult also are they to cure ; and certainly there is much more danger of *visceral disease* in those that are of long standing.

It is probable that agues, such as we see in this country, would generally terminate under favourable circumstances, in spontaneous recovery, provided that the patient could be put beyond the further operation of the malaria, protected from exposure to wet and cold, and suitably nourished. But possessing as we do a specific cure for ague, if there be such a thing as a specific, there

would be no sense in our allowing the experiment of a spontaneous recovery to be made : or rather we should be inexcusable, knowing as we do that the complaint is the more obstinate the longer it has lasted, and that it tends to the establishment of organic visceral disease, we should be inexcusable if we did not stop it as quickly as we can. The disease is always distressing to the patient, and always debilitating. It may be dangerous, even in these climates, to weak or old persons : and it is dangerous to all persons in hot climates. "If the first fit (says the wise and observant Heberden) has been marked so clearly as to leave no doubt of its being a genuine intermittent, the remedy should be immediately given in such a manner as to prevent, if possible, a second." There needs very little preparation of the patient before administering the specific substance which is to cure him ; and which every one here knows before he hears me say so, is the celebrated Peruvian bark, or its active principle as presented by the salts of quina. The old practice was to wait a few returns of the fits, either till some hypothetical ferment had taken place, or until supposed morbid matter had been expelled by vomiting or purging. There is, however, one very simple and short preparative which I am in the habit of using, and which I learned at Cambridge. You are aware that Cambridge is situated on the very edge of the fenny country which extends along that part of the east coast of the island. Numerous patients afflicted with ague come in from the surrounding villages ; and Dr. Haviland found that many of these had taken quina before they applied for assistance as out-patients at the hospital ; but with very poor success. Now these cases readily gave way—the patient remaining in all other circumstances as before—after the operation of a calomel purge. I have adopted this practice, therefore, upon his recommendation ; but it does not delay the specific treatment. I generally prescribe three grains of calomel with six or eight grains of rhubarb at bed-time, and commence with the quina the next morning. Very lately, in perusing the late Dr. Baillie's posthumous volume, I met with the following passage :—"I have known a good many cases in which bark alone would not cure an ague. In all these cases, as far as I now recollect, when a grain of calomel was given every night for eight or ten nights, bark cured the ague in the course of a few days. This practice I learnt from my friend Dr. David Pitcairn."

I believe that calomel given once in a purgative dose is enough.

But first of all, what is to be done for the patient while he is *in* the fit. I confess to you that I seldom give myself much concern on that head. In ague, as we see it in this country, nature generally prompts to the

patient what to do : to cover himself up in bed, and apply warmth to his feet, and to take some hot drink, during the rigors ; to adopt a cooler regimen during the hot stage ; to wipe his skin dry, if the sweating should be very profuse or protracted. But in hot countries, and in severer forms of intermittent, the patient really requires some help ; and therefore I must consider shortly in the next lecture the management of the paroxysm ; and I am the more bound to do so, because certain measures which I do not think necessary or judicious, at any rate for the complaint as we see it here, have lately been strongly recommended during the ague fit.

LECTURES
ON THE
FUNCTIONS OF THE NERVOUS
SYSTEM.

BY W. B. CARPENTER, M.D.

LECTURE VII.

Special Functions of the Spinal Cord, continued. Its influence on various parts of the alimentary canal. Action of the Sphincters. Protective power, &c.

THE influence of the spinal cord, and of its system of nerves, on the movements of respiration, as explained in the last lecture, affords an excellent example of the importance of this organ, as supplying the conditions immediately requisite for the maintenance of the organic functions. Strictly speaking, the act of respiration, as we commonly understand it, is not respiration itself ; for *this* consists in the interchange of ingredients between the blood and the surrounding medium, which is effected in the air-cells of the lungs, and which takes place in the lower animals (as in plants) without any muscular effort. But, in proportion to the necessity for the energetic exercise of this function, do we find a special provision in the higher classes, for the constant renewal of that portion of the surrounding medium which is in contact with the aerating surface ; and this comes to be so necessary, that asphyxia might be produced, without any interruption to the ingress of air through the trachea, by merely breaking the circle of nervous action through which the movements of respiration are effected. It is an interesting circumstance, however, which shows the provision made in the animal frame to meet its necessities, that a very small portion only of the nervous centres is involved in this action ; and that, even in the highest animals, all the rest may be removed, or be rendered functionally inactive, without checking it. This fact, which was ascertained by Legallois, harmonizes well with that which comparative

anatomy has brought under our notice ; for it was shewn in a former lecture, that, in the lowest group of Mollusca, but a single ganglion exists ; and that this is almost exclusively concerned in regulating the entrance and egress of the currents of water, the most constant office of which is the aeration of the blood.

Another very important function of the spinal cord (and of the ganglia corresponding to it in the Invertebrata), is the control which it exercises over the entrance and termination of the alimentary canal ; and this reflex action might probably be traced in some animals in which the necessity for that already described does not exist. In all beings which are unequivocally of an animal character, a stomach or digestive cavity exists ; and a means must be provided for the introduction of food into it. This is partly provided for by the power with which its entrance is endowed, of contracting, and of attempting to draw inwards whatever comes in contact with it, as we may readily observe in the star-fish, or sea-anemone, where the mouth is simply the aperture of the stomach. From the analogy of the higher animals, as well as from what has been observed in the lower, it seems probable that *this* action is of a reflex character, depending upon an impression conveyed to the nervous centres, and reflected back to the muscular fibres. But we almost always find some more special apparatus than this, for bringing food within reach of the orifice of the stomach. In the sea-anemone, the hydra, and other polypes, for example, we find that aperture surrounded by tentacula, which have an evident tendency to lay hold of any thing that touches them, so as to bring it, by their contraction, within reach of the muscles immediately surrounding the orifice. This is just the purpose of the pharyngeal muscles. The lower part of the œsophagus, near its termination in the stomach, has the same simple tendency to contraction from above downwards (so as to convey into the stomach any thing which is brought within its grasp) as have the muscles surrounding the mouth of the polype ; but there is need of some more complex apparatus, for the purpose of laying hold of the food, and of conducting it into its grasp. This is provided for, in the higher animals, in the muscles of that funnel-like entrance to the œsophagus, which is called the pharynx. The actions of these are most distinctly reflex ; and it is interesting to remark, that the movements can neither be caused nor controlled by the direct influence of the will. In the case of the movements of respiration, we found a sufficient provision made for their constant maintenance ; and yet, for secondary purposes, they were placed in a considerable degree under the control of the brain. But

here there are no secondary purposes to be answered; the introduction into the stomach of food brought by the will within reach of the pharyngeal muscles, is the only object contemplated by them; and they are accordingly placed under the sole government of the spinal cord. No attempts, on your part, will succeed in producing a really voluntary act of deglutition. In order to excite it, you must supply some stimulus to the fauces. A very small particle of solid matter, or a little fluid, (saliva, for instance,) or the contact of the back of the tongue itself, will be sufficient; but without either of these, you cannot swallow at will. Nor can you restrain the tendency, when it is thus excited by a stimulus; every one knows how irresistible it is when the fauces are touched in any unusual manner; and it is equally beyond the direct control of the will in the ordinary process of eating,—voluntary as we commonly regard this. The only way in which the will can influence it, is by regulating the approach of the stimulus necessary to excite it; thus, we voluntarily bring a morsel of food, or a little fluid, into contact with the surface of the fauces, and an act of deglutition is then involuntarily excited; or we may voluntarily keep all stimulus at a distance, and no effort of the will can then induce the action. Moreover, this action is performed, like that of respiration, when the power of the will is suspended, as in profound sleep or apoplexy affecting only the brain; and it does not seem to be at all affected by the entire removal of the brain in a cold-blooded animal, being readily excitable, on stimulating the fauces, so long as the nervous structure retains its functions. This has been experimentally proved by Dr. M. Hall; and it harmonizes with the natural experiment sometimes brought under our notice in the case of an anencephalous infant, in which the power of swallowing seems as vigorous as in the perfect one. But, if the nervous circle be destroyed, either by division of the trunks, or by injury of any kind to the portion of the nervous centres connected with them, the action can no longer be performed; and thus we see that, when the effects of apoplexy are extending themselves from the brain to the spinal cord, whilst the respiration becomes stertorous, the power of deglutition is lost, and then respiration speedily ceases.

Our knowledge of the nerves specially concerned in this action is principally due to the very careful and well-conducted experiments of Dr. J. Reid. The distribution of the glosso-pharyngeal evidently point it out as in some way connected with it; and Sir C. Bell, misled by imperfect knowledge of its anatomy, pronounced it to be a *muscular* nerve, whose function was to perform the combined movements of the tongue and

pharynx, which are required for deglutition, and also in some acts of respiration. He was not aware that such a combination of movements may be due as much to the *excitor* nerve, and its termination in the spinal cord, as to the *motor*, and its particular distribution to muscles. The function of the glosso-pharyngeal nerve has been for some time one of the *questiones vexatæ* of physiology; and the results obtained by different experimenters are so strangely at variance, as almost to lead to the belief that they have operated on different nerves. In this dilemma, we may advantageously have recourse to anatomical examination of its distribution; and this, when carefully conducted, discloses the important fact that the nerve scarcely sends any of its branches to the muscles which they enter; but that these mostly pass through them, to be distributed to the superjacent mucous surface of the tongue and fauces. Further, when the trunk is separated from the nervous centres, no irritation will produce any muscular movements. Hence it is not an efferent or motor nerve in any degree; and its distribution would lead us to suppose its function to be, the conveyance of impressions from the surface of the fauces to the medulla oblongata. This inference is fully confirmed by the fact that, so long as its trunk is in connection with the medulla oblongata, and the other parts are uninjured, pinching, or other severe irritation of the glosso-pharyngeal, will excite distinct acts of deglutition. Such irritation, however, may excite only convulsive twitches, instead of the regular movements of swallowing; and it is evident that, here as elsewhere, the impressions made upon the extremities of the nerves are much more powerful excitors of reflex movement, than those made upon the trunk, though the latter are more productive of pain. It was further observed by Dr. Reid, that this effect was produced by pinching the pharyngeal branches only; no irritation of the lingual division being effectual to the purpose. If, then, the muscles of deglutition are not immediately stimulated to contraction by the glosso-pharyngeal nerve, it remains to be inquired by what nerve the motor influence is conveyed to them from the medulla oblongata; and Dr. Reid has been equally successful in proving that this function is performed by the pharyngeal branches of the par vagum. Anatomical examination of their distribution shews that they lose themselves in the muscles of the pharynx; and whilst no decided indications of suffering can be produced by irritating them, evident contractions are occasioned when the trunk, separated from the brain, is pinched or otherwise stimulated. It appears, however, that neither is the glosso-pharyngeal the sole excitor nerve, nor are the pharyngeal

branches of the par vagum the sole motor nerves concerned in deglutition; for after the former has been perfectly divided on each side, the usual movements can still be excited, though with less energy; and, after the latter have been cut, the animal retains the means of forcing small morsels through the pharynx by the action of the muscles of the tongue and neck. From a careful examination of the actions of deglutition, and of the influence of various nerves upon them, Dr. Reid draws the following conclusions:—The impressions are conveyed to the medulla oblongata chiefly through the glosso-pharyngeal, but also along the branches of the fifth pair distributed upon the fauces, and probably along the branches of the superior laryngeal distributed upon the pharynx. The motor influence passes chiefly along the pharyngeal branches of the vagus; along the branches of the hypoglossal, distributed to the muscles of the tongue, and to the sterno-hyoid, sterno-thyroid, and thyro-hyoid muscles; along the motor filaments of the recurrens, ramifying upon the larynx; along some of the branches of the fifth supplying the elevator muscles of the lower jaw; along the branches of the portio dura ramifying upon the digastric and stylo-hyoid muscles, and upon the muscles of the lower part of the face; and probably along some of the branches of the cervical plexus, which unite themselves to the descendens noni.

When the food has been propelled downwards by the pharyngeal muscles as far as their action extends, its further progress down the œsophagus is effected by the peristaltic movement of the muscular coat of the tube itself. This movement is not, however, due to the *direct* stimulus of the muscular fibre by the pressure of the food, as it seems to be in the lower part of the alimentary canal; for Dr. J. Reid has found, by repeated experiment, that the continuity of the œsophageal branches of the par vagum with the spinal cord, is necessary for the propulsion of the food; so that it can scarcely be doubted, that an impression made upon the mucous surface of the œsophagus, conveyed by the afferent fibres of these nerves to the medulla oblongata, and reflected downwards along the motor fibres, is the real cause of the muscular contraction. If the par vagum be divided in the rabbit, on each side, above the œsophageal plexus, but below the pharyngeal branches, and the animal be then fed, it is found that the food is delayed in the œsophagus, which becomes greatly distended, and that no more passes into the stomach than is absolutely forced down by the contractions of the pharynx above. Further, if the lower extremity of the par vagum be irritated, distinct contractions are seen in the œsophageal tube, proceeding from above downwards, and extending over

the cardiac extremity of the stomach. We have here, then, a distinct case of reflex action, without sensation, occurring as one of the regular associated movements in the natural condition of the animal body; and it is very interesting to find this following upon a reflex action *with* sensation (that of the pharynx), and preceding an action which is altogether connected with the spinal cord, (that of the lower part of the alimentary canal.) The use of sensation in the former case will presently appear. It is by no means impossible, however, that the muscular fibres of the œsophagus may be *also* excitable, though usually in a less degree, by *direct* stimulation; for it appears that, in some animals (the dog, for example,) section of the pneumogastric does not produce that check to the propulsion of the food which it gives to the rabbit*. Moreover, there are many cases in which no such excitability manifests itself in the ordinary condition of the system, but in which it becomes evident when the muscular structure has gained an increase of irritability by diseased action, as we frequently have to notice in the intestinal canal. For example;—in many cases of disease or injury of the spinal cord, the bladder ceases to expel its contents, through the interruption of the circle of reflex actions hereafter to be described; but, after a time, it ceases to become necessary to draw off the urine by the catheter; for the fluid is constantly expelled as soon as it has accumulated in small quantities. In such cases, the mucous coat is found after death to be thickened and inflamed; and the muscular coat is greatly increased in strength, and contracted upon itself. Here, then, the muscular coat, which is not excited to contraction as long as the mucous coat is in a healthy condition, acquires a degree of abnormal irritability, which is sufficient to enable it of itself to expel the urine.

It will be desirable here to revert for a short time to the actions which, in the higher animals, precede those of deglutition. There can be no doubt that, in the human being, the motions adapted to the ingestion and mastication of aliment originally result, in part at least, from distinct operations of the will; but it would appear almost equally certain that, in time, they come to be of so habitual a character, that the will only exerts a general controlling influence over them, each individual act being excited through the shorter channel alluded to in the last lecture. Every one is conscious that the act of mas-

* It is possible that this may be due to the fact that the par vagum anastomoses with the sympathetic at the upper part of the neck, more freely in the dog, than in the rabbit and many other animals; so that the influence of the œsophageal nerves may be propagated through the sympathetic, when the trunk of the par vagum is divided.

tication may be performed as well when the mind is attentively dwelling on some other object, as when it is directed to it; but, in the former case, one is rather apt to go on chewing and re-chewing what is already fit to be swallowed, simply because the will does not exert itself to check the action, and to carry the food backwards within the scope of the muscles of deglutition. We now see why sensation should be associated with the latter process. The conveyance of food backwards to the fauces is a distinctly voluntary act; and it is necessary that it should be guided by the sensation produced there by the contact which it induces. If the surface of the pharynx were as destitute of sensation as is the lower part of the oesophagus, we should not know when we had done what was necessary to excite its muscles to operation. The muscles concerned in the mastication of food are nearly all supplied by the third branch of the fifth pair, a large proportion of which is well known to have a motor character. Many of these muscles, especially those of the cheeks, are also supplied by the portio dura of the seventh; and yet, if the former be paralyzed, this cannot excite them to the necessary combined actions. Hence we see that the movements are of an associated character, their due performance being dependent on the part of the nervous centres from which the motor influence originates. If the fifth pair, on the other hand, be uninjured, whilst the seventh is paralyzed, the movements of mastication are performed without difficulty; whilst those connected in any way with the respiratory function, or with expression, are paralyzed. Comparative anatomy supplies us with the key to the explanation of these phenomena. It has been seen that, in the lower animals, the respiratory organs are completely unconnected with the mouth, and that a very distinct set of muscles is provided to keep them in action. These muscles have distinct ganglia as the centres of their operations; and these ganglia are only connected indirectly with those of the sensori-volitional system. The same would appear to be the case in regard to the introduction of the food into the digestive apparatus. It has been shewn that the muscles concerned in this operation have their own centres,—the stomato-gastric and pharyngeal ganglia,—which are not very closely connected with the cephalic, or with the respiratory, or with those of general locomotion. Now in the Vertebrata, the distinct organs have been so far blended together, that the same muscles serve the purposes of both; but the different sets of movements of these muscles are excited by different nerves; and the effect of division of either nerve is to throw the muscle out of connection with the function to which it previously ministered through it, as

much as if the muscle were separated from the nervous system altogether. There is an apparent exception to this view of the matter in the case of the portio dura; this being the source of those movements of the upper lip, which, in many animals, are essential to the prehension of food. These movements, however, are dependent upon *sensations* excited through the fifth pair*, and they are completely checked by division of its infra-orbital trunk; and it can scarcely be doubted, from their general character, that they are of a strictly *voluntary* nature, and are not to be regarded as part of the reflex associated movements in which that nerve is concerned.

Now although, in the adult human being, the movements required to convey the food to the pharynx are under the control of the will, if not constantly dependent upon it, there is good reason to believe that this is not the case in regard to those remarkable associated movements which constitute the act of suction in the infant. The experiments provided for us by nature, in the production of anencephalous monstrosities, fully prove that the nervous connection of the lips and respiratory organs with the spinal cord is alone sufficient for its execution; and Mr. Grainger has sufficiently established the same, by experiment upon puppies whose brain had been removed. He adds that, as one of the puppies lay on its side, sucking the finger which was presented to it, it pushed out its feet in the same manner as young pigs exert theirs against the sow's dugs. On the whole, however, the act of suction belongs more to the respiratory ganglion (so to speak) than to the stomato-gastric system of nerves; and hence we can understand why, even in the highest animals, it should be purely instinctive, the movements of respiration being so from the first, whilst those ordinarily concerned at a later period in the ingestion of the food are more directed by the will. The actions of the mammary foetus of the kangaroo, described by Mr. Morgan, furnish a very interesting exemplification of the same function of the spinal cord; this creature, resembling an earth-worm in appearance, and only about fourteen lines in length, with a brain corresponding in degree of development to that of a human foetus of the ninth week, executes regular, but slow, movements of respiration, adheres firmly to the point of the nipple, and moves its limbs when disturbed. The milk is forced into the oesophagus by a compressor muscle, with which the mamma of

* Hence originated one of Sir C. Bell's early errors. He found that an ass, in which the infra-orbital branch of the fifth was divided, would not pick up oats with its lip, although they were in contact with it; hence he concluded that its power of motion was destroyed, when it was in reality only the sensation necessary to excite the will to cause the motion, that was deficient.

the parent is provided. "Can it be imagined," very justly asks Mr. Grainger, "that in this case there are sensation and volition in what can be proved anatomically to be a foetus?"

We now return to the question of the influence of the spinal cord upon the lower part of the alimentary canal. It has been already stated, that the motor function of the par vagum appears to terminate at different points in different animals; and this may in part explain the great variety in the results obtained by different experimenters, in regard to the effect of section of the par vagum upon the function of digestion. Valentin agrees with Dr. Reid in stating, that distinct movements of the stomach may be excited in the rabbit by irritation of the par vagum; and he adds, as a precaution, that the experiment should be performed very soon after death, as the irritability of the stomach is soon lost; and that the stimulation of the nerve should not be performed too high up, but rather in the lower part of the neck, or in the thorax. Various experiments upon living animals have led to the belief, that the motions of the muscular parietes of the stomach, which perform a very important part in chymification, are due to the influence of this nerve; food taken in shortly before, or subsequently to, its division, having been found to be only dissolved on the surface of the mass, where it was in contact with the mucous membrane. But these experiments have been made for the most part upon herbivorous animals, such as horses, asses, and rabbits, whose food is bulky and difficult of solution, requiring to be constantly changed in its position, so that every part of it may be successively brought to the exterior. On the other hand, Dr. Reid found, in his experiments upon dogs, that, after the first shock of the operation had gone off, solution of the food in the stomach, and absorption of chyle, might take place; and hence that no influence of this nerve upon the muscular parietes of the stomach is essential to digestion in that animal. This conclusion harmonizes well, therefore, with the fact already stated respecting the absence of such influence in the lower part of its oesophagus; and it may, perhaps, be explained by the consideration, that the natural food of the dog is much less bulky and more easy of solution than that of the animals already named, so that there is not so much need of the peculiar movement which is in them so important an aid to the process of reduction.

In regard to the functions of the afferent portion of the gastric branches of the par vagum, there has also been considerable difference of opinion; some physiologists maintaining that it is by impressions on them alone that the sense of hunger or

satiety is occasioned; whilst others deny that it has any power of transmitting such impressions, and maintain that they do not originate in the stomach at all. Dr. Reid has arrived at the conclusion, from his numerous experiments, that the par vagum is the channel through which the mind becomes cognizant of the condition of the stomach; but that it is not the sole excitor of the sense of hunger. Animals which have sustained section of the nerve on both sides will eagerly take food, if they have not received too great a shock from the operation; but they seem to experience no feeling of satiety when the stomach is loaded. This inference is confirmed by Valentin, who mentions that puppies after the operation will take three times, and even more, the same quantity of milk as uninjured individuals of the same age; so that the abdomen is greatly distended. The other sources of the sense of hunger will be hereafter considered.

The act of vomiting has been now sufficiently shewn to be excitable through the par vagum; an impression propagated through which to the medulla oblongata, excites to contraction a considerable number of muscles. But, as in the case of hunger, although the sense of nausea and the tendency to vomit may be excited by various irritating causes operating through this nerve only, it may be produced also through other channels. Thus severe vomiting has been excited by the injection of a solution of tartar emetic or of emetin into the blood-vessels—a fact of which it has been proposed to take advantage in extreme cases of narcotic poisoning, when the nervous system has become so torpid, that emetics administered in the ordinary manner are of no avail.

That the ordinary peristaltic movements of the intestinal canal, from the stomach to the rectum, may take place without any connection with the nervous system, being due to the direct stimulation of the contact of food, there is now ample evidence; and though some may still be found who deny the Hallerian doctrine, that muscular fibre possesses in itself the property of contractility, the evidence of that fact appears to myself so clear and convincing, whilst the fact itself is so conformable to the analogy supplied by others, that I cannot hesitate in adopting it as an ascertained truth. Mr. Grainger and some other physiologists have supposed, that the peristaltic movements of the alimentary canal are due to a sort of reflex action taking place through the ganglia of the sympathetic system of nerves, especially, of course, the semilunar. This supposition, however, has little or no evidence to support it; for it has been fully proved that the muscular contractions will continue long after the tube has been separated from

its nervous connections through its whole extent; and the only evidence in its favour is derived from the contractions which may sometimes be induced in parts of the tube which are at rest, when the sympathetic nerves supplying them are irritated. Some very interesting experiments have been recently published by Valentin, by which the fact that such contractions may be induced (which has been denied by some) is clearly substantiated; but it is also shewn that the motor influence does not originate in the sympathetic ganglia, but in the spinal cord. The following are the general results of upwards of three hundred experiments, so far as they apply to this subject.

The pharynx may not only be excited to contraction by irritation of the pharyngeal branches of the par vagum, or of the roots of the spinal accessory, from which their motor power is derived (as explained in the last lecture) but also by stimulating the roots of the first two cervical nerves; and the lower part of the oesophagus in the neck is made to contract peristaltically from above downwards, by irritation of the roots of the first three cervical nerves, and of the cervical portion of the sympathetic, through which last the former evidently operate. The thoracic portion of the oesophagus is made to contract by irritation of the lowest sympathetic ganglion of the neck, and of the higher thoracic ganglia, and also of the roots of the lower cervical spinal nerves. Muscular contractions of the stomach are produced by irritation of the roots of the 4th, 5th, 6th, and 7th cervical nerves, and of the first thoracic, in the rabbit; so that a distinct furrow is evident between the cardiac and pyloric portion of the viscus; and the lower the nerve irritated, the nearer the pylorus do the contractions extend. Irritation of the first thoracic ganglion of the sympathetic produces the same effect. Contractions of the intestinal tube, varying in place according to the part of the spinal cord experimented on, may be excited by irritation of the roots of the dorsal, lumbar, and sacral nerves, and of the trigeminus; and similar effects are produced by irritation of the lower part of the thoracic portion, of the lumbar, and of the sacral portions of the sympathetic,—also of the splanchnic, and of the gastric plexus.

From these facts it is evident, that the movements of the intestinal tube may be influenced by the spinal cord, and that what is commonly termed the sympathetic nerve is the channel of that influence by the fibres which it derives from the spinal system. But it by no means thence follows, that the ordinary peristaltic actions of the muscles in question are dependent on a stimulus reflected through the spinal cord, rather than on one directly applied to themselves. It is clear that, although these movements are of the

first importance to the welfare of the system, the means of sustaining them are feeble, compared to those which we find provided for the maintenance of the distinctly reflex actions of deglutition, respiration, &c. The difficulty with which any evidence can be obtained of the connexion is a sufficient proof of this. On the other hand, we do know that these peristaltic movements are *influenced* by particular states of mind, or by conditions of the bodily system; and the connexion just traced satisfactorily accounts for this, and is itself sufficiently explained. The intestinal tube, then, from the stomach to the rectum, is *not dependent* upon the spinal cord for its contractility, but is enabled to propel its contents by its own inherent powers; still we find that here, as in other instances, the nervous centres exert a general control over even the organic functions, doubtless with the purpose of harmonising them with each other, and with the conditions of the organs of animal life.

ON
SOME POINTS IN THE PATHOLOGY
OF THE CIRCULATION.

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*Being the substance of the Gulstonian
Lectures for 1841.*

CONGESTION of blood, determination of blood, and inflammation, are terms so commonly used, and the proximate elements which they imply, are so fundamental to pathology, that it is the more needful that they should be used definitely, and with an understanding of their nature as clear as the present state of science will supply. The ground is one so much trodden that it is hard to find a spot in it untrampled by those who have gone before, either with the steady footsteps of patient observation, or with the irregular and uncertain strides of hypothesis; yet the ground, though beaten, is not yet plain—there are in it many different and devious tracks, and many a young traveller in the way to medical knowledge has turned aside discouraged at the perplexities of this part of his route. I do not expect to remove these perplexities, but I would endeavour to diminish them. We cannot anticipate an easy way through so intricate a subject; but if we be guided only by the simplest and most certain facts, we

may hope to approach truth by a plainer and straighter path: and here, as in many departments of medicine, we shall find a knowledge of simple physical laws essential to the right understanding of physiological and pathological science.

The subjects chosen are so extensive, and the time for discussing them so brief, that it will be impossible to enter into much detail; and as many well established facts in physiology and pathology which bear on them may be fairly taken as fundamental, I shall generally introduce the subjects synthetically, deducing the phenomena from these facts.

The genius of Haller and of Hunter quickly perceived that the processes of inflammation and its effects could be understood only through a knowledge of the vital and physical properties of the tissues and systems of the living body; and their researches early led these great men to trace to the organs of circulation the chief share in these processes. These researches threw much light on the healthy and diseased properties of the blood-vessels. But in the days of Haller and Hunter, neither physiology, nor even physical science, was sufficiently advanced to explain all the phenomena of the healthy circulation, and still less those in disease; and although much has been done in the way of both experiment and reasoning since those days, yet a glance at the discrepant facts and opinions that have been and are still held, must convince us that if some have approached nearer to the truth, others have gone further into error; and that the advances of knowledge have been by no means steady. In illustration of these remarks, I will introduce our subjects by a few observations on the chief forces concerned in the circulation of the blood.

No one disputes that the chief moving power is the heart; but the absolute and comparative amount of this power has been a subject of much dispute; some, as Borelli, estimating it at 180,000lbs.; Bernouilli at 3000lbs.—forces more suited to produce an explosion, than to effect the quiet circulation of a fluid. Poiseuille, guided by more direct experiment, calculates the ordinary force with which the blood is propelled from left ventricle into aorta at 4lbs. 3oz.—a result

varying but little from that of Hales, who, from somewhat like methods of experiment, estimated it at 4lbs. 6oz.; and the pressure on the interior of the whole heart to amount to about 50lbs.

The share which the arteries have in the circulation has been a matter of much question, but should scarcely be considered so at present; for the vital and physical properties of these tubes are now pretty well ascertained. They are found in the dead body to possess physical elasticity and extensibility in a high degree; this property residing chiefly in their outer or cellular coat. Parry first clearly showed that they also possess the vital property of tonicity, referable to their middle coat; by virtue of which they tend continually during life to contract on their contents; being prevented from contraction only by the repeated injected supplies from the heart. When this supply is intercepted by tying an artery, the tube beyond the ligature contracts nearly to obliteration; so, too, when the heart's motions cease at death, this vital property continuing awhile after, empties the arteries of their contents; and in cases of temporary suspension of the heart's action, continues to propel the blood onwards in the capillaries and veins. It has been much questioned whether the arteries possess irritable contractility; whether their contractions can be determined or increased by the application of a stimulus; but I think their title to this property is fully established by the positive results of experiments of Thomson, Verschuir, Parry, Hastings, Kaltenbrunner, and Wedemeyer.

The question has been, however, in regard to the larger arteries, the irritability of which is still doubted by Nysten, Wedemeyer, and Müller. To the results obtained on this point by Hastings and Thomson, I will add some from my own experiments.

I have, on several occasions, irritated portions of the carotid of the living ass by the point of a scalpel, by ammonia, and by galvanism, with the effect of causing, in the course of a minute or two, a gradual contraction of the artery at the irritated point; which contraction continued for many minutes, and, in one instance, was afterwards followed by a dilatation at the same point.

A less tardy contraction of an artery under the influence of galvanism, was

detected in the mode which I adopted for testing the irritability of the bronchi. Immediately after the death of a large dog, ligatures were passed around the arch of the aorta and the left common iliac, and a hæma-dynamometer adapted to the right common iliac. A galvanic current was then passed along the artery; the fluid of the dynamometer immediately began to rise, and steadily rose two inches. On stopping the current, it still rose for a few seconds, and then sank progressively. The galvanism once again raised it slightly, but no more. Here were the true characters of low irritability, slow contraction on the application of a stimulus, followed by relaxation after its removal, and exhaustion on its continuance. But no correct experiments give any ground for supposing that the irritable contraction of the arteries is any other than slow and gradual. In no recorded experiment, nor in any of my own, have I ever found evidence of sudden motions in the arteries, independent of those communicated by the heart.

We may therefore at once infer, what is now the received opinion of most physiologists, that the contraction of the arteries is too slow to be concerned in the rhythmical movements of the pulse; neither does it produce any perceptible vermicular or progressive motion, but it promotes the circulation chiefly by the continued regulated pressure which it exerts on the blood—a pressure not of mere physical elastic reaction, but of this together with a vital tonicity, tending to regulate or alter the flow of blood through the arteries in the whole or in parts of the body, in obedience to vital as well as physical influences exerted on this vital property. The arterial pulse in no way depends on this property; for it may be perfectly imitated in the arteries of the dead body by the strokes of a syringe, or by connecting the arteries of a dead with those of a living animal (Bichat); but the vital contraction, in other words, the tone of the arteries, is an important element in giving quality to the pulse. For example, in the hard, tense pulse of inflammatory fever or other highly sthenic or tonic states of the system, we recognise the excess of the tonic contractility increasing the tenseness and diminishing the usual yielding elasticity of the artery, which therefore

approaches more to a rigid tube. The same quality in vessels with little blood may give hardness even to a small pulse. On the other hand, defective tone gives that softness to the pulse which we find in all those states of the system which long have been, for practical reasons, and now for physiological also may be, called *atonic*. We sometimes find these extremes follow each other. After the subsidence of a fever in which the pulse has been unusually hard, it is found not merely soft but gently springing with a back stroke, so as to produce what has been called the *dicrotous* pulse; the absence of tone reducing the artery to a mere physically elastic tube, softly rebounding after each pulse of the heart.

Another physical effect of tonicity of the arteries, is on the velocity with which the pulse is transmitted through them; excess of tone, by increasing the tension of the tube, accelerates the transmission of the pulse-wave to the extreme arteries: whereas a defect of this property leaves them in that state of soft elasticity which yields before it reacts, and therefore is more tardy in propelling onwards the pulsewave. Hence, with a hard pulse there is scarcely an interval of time between the heart's impulse and first sound and the pulse of distant arteries. It is so much otherwise with a soft pulse, that I have often found the radial pulse to occur after the second sound of the heart.

Tone of the vascular system, of which this tonicity is the element, is a very important constituent of health; and in excess, defect, or irregular distribution, a most common integral part of disease. So, too, is it most important as a sign of disease, and guide in employment of remedies. This long has been recognised in the characters of hardness, softness, and inequality of the pulse; and in doubtful cases I have often been usefully led to decide between a tonic and an antiphlogistic remedy by observing more or less of an interval between the heart's beat and the pulse of a distant artery. Many other illustrations from varieties of pulse might be adduced, but of these our time does not admit.

The experiments of Hales and Poiseuille have shown that the force with which the blood is propelled is generally the same in the whole arterial

system, being equalised by the elastic tension of the tubes composing it. This tension, reacting on the blood, continues to propel it in the intervals of the heart's beats; and in the extreme arteries and capillaries so completely equalises the motion, that the pulses are no longer perceptible, and the blood passes in a continuous stream. When the arterial tension is diminished, the heart's pulses may be not less propagated to distant arteries; nay, the pulsatory movement becomes more distinct than usual, the heart's jet being less resisted, and therefore more forcible; and it is exactly in these circumstances that the pulsation sometimes is seen to extend to the capillaries and even to the veins. This may seem somewhat paradoxical, but it is quite intelligible if we consider the manner in which the heart and arteries act in the circulation.

It is now generally recognised that the heart's contraction is the chief moving power, whilst the arterial tension is the great distributive of this power; but no writer has sufficiently attended to the different results of varied proportions in the moving and the distributing agencies. At the cardiac end of the arterial tube the force of the heart and the pressure of the arteries are opposed to each other; at the capillary end they are in the same direction. In a full, tense state of the arteries, the heart has, at each systole, to overcome the resistance of the arterial pressure, and in doing this, much of the force of each jet issuing from the heart is lost. But each jet adds to the pressure in the arteries, and thus sustains the continuous stream in their capillary ends, where the flow is less pulsating. In a lax or ill-filled state of the arteries, on the other hand, the heart's jets being less resisted by arterial pressure, are more forcible and abrupt. They are most violent near the heart; but if the vessels be not too empty, they communicate their jerky pulses even to the capillaries, the motion in which is now more pulsating than continuous, and often more oscillatory than progressive. I need not point out how this corresponds with the throbbing heart and arteries, yet weak and imperfect circulation, after great losses of blood and other circumstances diminishing the arterial pressure.

This brings us to the *capillaries*:—a

conventional term applied to the fine vessels communicating between the arteries and veins; in many tissues distinguished only by their size from the minute arterial and venous ramifications which they connect, as in the frog's web and mesentery, serous membranes, muscle, &c.; in other structures, remarkable for a uniform arrangement, network, or vascular plexus, quite different from the corresponding arteries and veins, and also peculiar to the structure in which they are found, as in synovial and mucous membranes, some glands, skin, and especially in the choroid coat of the eye.

It is impossible to look at the varieties of rete mirabile that these capillaries exhibit, without a feeling of awe as well as of admiration, when we reflect that we are inspecting the machinery of some of the most wonderful processes of life—secretion, nutrition, and functional renovation in general. And whilst we can thus follow with the eye the beautiful loops and nets of vessels until we seem to have made them all out, how humbled must we still be when we reflect how little they will explain the magnificence of God's work—the great and wonderful changes of which these are the unintelligible instruments.

We have not time to describe the phenomena of the capillary part of the circulation, which forms so interesting an object under the microscope. The best descriptions are those of Kaltenbrunner and Poiseuille. It can be seen that the flow through the capillaries is less rapid than through the arteries or even the veins. This must be ascribed in part to the admitted fact, that the aggregate area of the capillaries is much greater than that of the arteries which supply them; just as that of the arteries is greater than that of the aorta. Their enlarged area is not, however, the only cause of the slow motion of the blood in capillaries. M. Poiseuille observed the same slowness of motion in a capillary vessel (admitting only single globules) given off from an artery of some size, in which the current was rapid; yet the pressure must have been the same in this branch as in its arterial trunk. The additional cause of the tardy flow through the capillaries seems to be of the nature of capillary attraction—a mere physical property between liquids and solids, which is increased in proportion to their extent of contact.

Thus, too, Poiseuille found that in all vessels admitting several globules abreast, the current was most rapid in the axis of the vessel; it was slower towards the circumference, and in contact with the walls of the vessel there was quite a motionless layer. Several anomalies and irregularities, described by many observers, in the motion of the globules, may be traced to their getting more or less into this motionless layer, where they become either stationary or revolve on themselves, or are whirled spirally in the layer by the occasional impulse of one of the faster moving globules nearer the axis. So, too, in narrow vessels admitting only one or two globules abreast; if one get into the motionless layer, it may stick there and cause an obstruction to the current. As soon as the current is stopped, the pulsatory motion from the heart, before hidden by the continuous motion, often becomes apparent at the point of obstruction next the heart; and these pulses, after making the globules oscillate awhile, again set them free, and the continuous current is restored. These phenomena I have repeatedly observed.

The capillaries, even more than the arteries, have been supposed to be the seat of a power which assists greatly in the circulation; and this supposition has been formed chiefly on the assumption that the other forces are insufficient, and will furnish no explanation of several phenomena in health and in disease. But we have met with no evidence in favour of any independent action of the capillaries further than that which has been already described to belong to the small arteries, elasticity, and tonic or slow irritable contractility; and the increased operation of these would be to obstruct rather than to promote the flow of blood through them. The only hypothesis which would in any degree furnish to the capillaries an additional power aiding in the circulation, is that which assumes the existence of vital attractions and repulsions between the blood and its vessels, by virtue of which the capillaries attract arterial blood and repel venous*. But this hy-

pothesis involves so large an assumption, nothing less than a new principle of attraction, distinct from every kind hitherto known, that it should not be admitted without far more evidence than has been yet adduced in its favour.

Further, I am supported by several distinguished physiologists, when I state my belief that the chief facts which have been adduced in favour of the existence of these new powers, can be sufficiently explained by reference to simpler and well-established properties, vital and physical, of the vessels and of their blood. And on the point immediately before us, whether the heart's force, directed freely by the arteries and capillaries, is sufficient to effect the circulation, I would refer to the experiments of Hales and Wedemeyer, who found that no great force was required to cause bland fluids to pass by injection from the arteries through the capillaries to the veins.

This experiment has been reduced to greater accuracy by my colleague Dr. Sharpey. A syringe with a hæma-dynamometer attached, was adapted to the aorta of a dead animal, the vena cava being divided; warm water was then injected into the aorta, the hæma-dynamometer shewing the amount of force used in the injection. With a force that raised a column of mercury three inches, the water passed through the capillaries and veins. When the pressure was raised to support a column of six inches the flow was very free; and by another hæma-dynamometer attached to the vein, was found to raise the pressure in the veins to three inches. This pressure (six inches of mercury) was greater than natural pressure in the arteries of the living animal; and the venous pressure (three inches of mercury) is more than natural; shewing that a power not exceeding that of the heart, is sufficient*.

sions, will be found in his "*Recherches sur les Causes du Mouvement du Sang dans les Vaisseaux Capillaires.*"—Paris, 1835.

* In addition to the observations cited in the essays before referred to, I would state, that in many long and careful microscopic examinations of the circulation in the frog's web, variously modified by different agents, I never witnessed any movement of the blood particles that was not plainly referable to the action of the heart or of the vessels. The share which the arteries have in regulating the flow of blood through the capillaries and veins is most visible. When the arteries increase in size, the flow becomes very rapid and general; when they diminish the flow is tardy, and may cease in many capillaries; and when the arteries contract so as to permit no

* The chief arguments for and against the existence of vital properties of attraction and repulsion at sensible distances, have been ably stated by Professor Allen Thomson, in the article "Circulation," in the *Cyclopædia of Anatomy*, to which I would refer. The observations of M. Poiseuille, which furnish additional arguments against the vital attractions and repul-

We have not time to pursue further the physiological relations of these subjects, but shall proceed next to inquire how far the properties of the parts now described to be concerned in the circulation, will by their variations produce the phenomena of the diseased states—congestion and determination of blood, and inflammation.

ON LUSCITAS,

WITH SOME OBSERVATIONS ON THE PATHOLOGY OF THE OPHTHALMIC MUSCLES, AND ON SOME OTHER POINTS CONNECTED WITH STRABISMUS.

To the Editor of the Medical Gazette.

SIR,

SINCE medical men have been seized with the monomania for squint-cutting, much has been written on strabismus, but little or nothing, to my knowledge, on luscitas. As I have lately met with a case of luscitas spastica, and found in the last number of von Ammon's *Monatsschrift* for 1840, p. 550, the observation made by Dr. Rigler, that luscitas never admits of operation, and that Dr. Melchior, in his "*Dissertatio de Myotomia Oculi*," [Hauniæ, 1841, mense Martio], p. 21, regards an operation in these cases in the light of an experiment, I felt induced to give this subject a little closer consideration, and more especially with respect to treatment by operation.

A kind insertion of this paper on the subject in your valuable journal will much oblige,—sir,

Your obedient servant,

AUG. FRANZ, M.D. Leipsic,
M.R.C.S. &c.

19, Golden Square, June 30, 1841.

Although the term luscitas has been applied by authors to a variety of ophthalmic affections, Prof. Beer restricts it to those cases where the eye is turned, as in strabismus, to the one or other side, but cannot be moved at the will of the patient; when the sound eye is

closed, the eye affected remains completely fixed in an abnormal position.

As the principal causes of this affection may be considered a deficient or faulty formation of the orbit, the eyeball, its muscles, or of the eyelids; enlargement of the lachrymal gland, adventitious growths or abscesses in the orbit; symblepharon affecting a squinting eye to a great extent; idiopathic or metastatic inflammations of the eye or its appendages; atrophica, malacia, paralysis, hypertrophy, or spasm of one or more muscles.

If a deficient or faulty formation of the orbit, the muscles, &c. is the cause of luscitas, it is evident that little or nothing can be done for its relief; if enlargement of the lachrymal gland, or adventitious growths in the orbit, these diseases must be treated by pharmaceutical means, or operation, according to the indications in the case. An abscess in the orbit must of course be opened as early as possible, in order to prevent sinuses or accumulations of matter, which in this situation would create great mischief. If symblepharon exists in a squinting eye, the union of the lid with the ball must first be separated, by which proceeding the case is reduced to simple strabismus, and is then to be operated on in the usual manner. A case of this kind came under my treatment, and was cured on this plan. (See *MED. GAZ.*, vol. xxvii. p. 40.)

Atrophy of one or more muscles of the eye may be occasioned by want of exercise, when the eye has been affected with strabismus for a long time, and has not been used on this account; or by insufficient supply of blood to the muscles in consequence of disease, injury, pressure on the arteries, &c.; perhaps also by ossification of the arteries given off to the muscles, just as we find the arteria centralis ossified; or by suppuration being the sequel of a vehement inflammation. In these cases the nutritive functions are diminished, but they may also be deranged, for instance, after chronic blenorrhœa, and then malacia or softening of the muscle may take place. In both these instances the muscle loses its normal irritability, but more especially its power of contraction, and then becomes atonic and lax. The same diminution or total loss of muscular tone follows, when the innervation of the nerves of

blood to pass through them, the blood which still fills the capillaries and veins becomes quite stagnant, without a trace of spontaneous movement. When motion begins again, it may always be traced to an artery which admits first a file of single globules, which come few and far between, and in pulses; afterwards, as the artery enlarges, many, and in a continuous rapid stream—supplying proportionate motion to the blood in the vessels beyond.

motion is entirely or partially suspended, as in a paralytic state of the muscle from rheumatism, or from apoplexy, pressure on or softening of the brain, &c. Now when a muscle is affected with the maladies just mentioned, it is more or less unable to counteract the ordinary action of its healthy opponent, in which therefore the contractile power is permitted to gain the preponderance, and in time an actual or habitual contraction is established. From the diminution of power on the one side, and excess of it on the other, the eyeball has of course lost its proper position in the orbit, and is turned towards the contracted muscle, from which position it can only be slightly moved, or in which it is completely fixed.

As atrophy and malacia frequently come on slowly, and last for a long time, until the eyeball becomes absolutely fixed in an abnormal position, or luscitas is established, the time is entirely lost where the treatment by pharmaceutical means might have been advisable; and, in my opinion, an operation should be performed under such circumstances, provided the weakened muscle is not completely paralysed or otherwise totally disorganised—a circumstance certainly not easily ascertained beforehand. If in such cases the muscle which draws the eye out of its proper position is divided, and all additional impediments are removed which might tend to limit the motion of the eye, the weakened muscle is placed in a position more favourable for acquiring its lost power by exercise, as also for the action of local remedies upon the muscle, since it is no longer kept in a state of continued extension. But as an operation in these cases has only been the first step towards a cure, the ophthalmic orthopedie becomes now of the greatest importance. The healthy eye must therefore be bound up with a light bandage during the whole day for the first fortnight. After this time this plan must be persevered in at intervals for several weeks. While the eye is bound up a small shade must be fastened before the operated eye, in such a manner that the patient is forced to turn his eye in a direction exactly opposite to that it held before the operation, to be enabled to see. He must moreover be ordered to fix his eye firmly on a distant object. I have pur-

sued this plan in several cases of inversion dependent on a paralytic state of the external rectus. The most severe case of this kind which came under my care, and which I beg to relate more fully, occurred in a young gentleman from Staffordshire, recommended to me by Dr. Ingleby of Birmingham.

H. L., aged 9, of strumous habit, was born with his eyes perfectly straight, but three months after birth both were observed to turn in, and in time became more inverted. This double inversion appeared to be owing to an early hydrocephalus, as I was led to believe by circumstances elicited by a strict examination, and by the whole formation of the head. When I for the first time saw this patient, both eyes were inverted to such an extent that only about five-sixths of the right and two-thirds of the left cornea could be seen. The pupil of the right was normal in size and action, but the left rather contracted and slow in action. In ordinary or inattentive vision, both eyes remained in their usual inverted position; the patient was in the habit of constantly moving the body laterally, and the head in a semirotatory or circular manner, by which complicated movements of head and body (endeavours to compensate for the limited motion of the eyes) all objects presented themselves to the right eye, on account of this being in a more favourable position than the other, but were of course indistinctly seen. If now any object struck his attention, he kept this eye upon it, and turned the head so as to bring the pupil, which could be moved from the internal canthus a little beyond the centre of the orbit, into its proper position; keeping then the head stationary, he saw even small objects very distinctly; but as soon as his attention was distracted the eye resumed its usual abnormal position. With the left eye, which could be moved but slightly and with difficulty when the other was closed, the patient saw objects almost as well as with the right, when the head, eye, and object, were brought in such a position that the visual axis was in a line with the object viewed. The patient used only one eye for seeing, generally the right, leaving the other entirely inactive, on which account no double-vision was present.

On the 2d of July, 1840, I divided the internal rectus on the left side,

which produced no effect in the position of the eye or in facilitating its motion outwards. I now proceeded to separate the cellular tissue between the muscle and the ball, extended the wound in the conjunctiva, and divided the inner border of the superior and inferior rectus, but also without effect, and at last divided the superior oblique, after which proceeding the eye certainly could be turned further outwards, and with greater facility, although it could not be kept in this position, as on cessation of the will the inversion was as great as before the operation. Having thus freed the eye from all mechanical influence which tended to keep it inverted, the right eye was bound up, and a shade fastened before the left, so as to oblige the patient to turn the eye as much as possible outwards in order to be enabled to see; he was moreover ordered to fix the eye thus shaded frequently on a distant object. Ung. Hydr. Mit. was rubbed into the neighbourhood of the inner canthus, to retard the healing of the muscle. An ointment composed of Phosphorus dissolved in Ol. Rosmar. and Bals. Peruv. was rubbed around the external canthus and the temples of both eyes. Ung. Ant. Pot. Tart. was applied to the vertex, and the bowels were regularly kept open. A week after the operation the ophthalmic fountain (described MED. GAZ., vol. xxvii. p. 444,) was used. This treatment was continued until the patient was again brought to town, when I found the left eye not half as much inverted as before the operation, and that both eyes could be turned outwards beyond the orbital axis.

On the 22d of August the rectus internus of the right eye was divided, and the superior and inferior rectus partially. Immediately after this operation the eye assumed its proper position for a few minutes, after which it was again somewhat inverted. By closing the left eye and fastening the shade before the right, in the manner above described, the external muscle of this eye was chiefly exercised up to the 26th of August, when the rectus internus of the left eye was again divided, after which operation both eyes daily improved by judicious use, so that when the patient left town on the 31st of August, the right eye was perfectly straight, and the left nearly so. In walk-

ing, the carriage was more steady, and the movements of the head diminished and less awkward. He was ordered to look frequently with both eyes at distant objects (as in shooting at a target) to exercise principally the external rectus of the left eye, to continue the strengthening ointment to the temple and lids on both sides, as also the ophthalmic fountain, to go to the sea-side, and to take frequent exercise in the open air.

The mother, who bestowed the greatest care upon the patient, he being an only child, wrote to me from time to time that the left eye slowly improved in position, and that both could be freely moved towards the external canthus, and in her last letter of the 20th of March, 1841, she observes, "My little son's eyes are now perfectly straight; there has not appeared the least inversion for I should think the last two months; before that time fatigue or change of weather considerably affected them, but such is not the case now. They have been examined by several professional men, who are unanimous in the opinion of its being a perfect cure. He can turn the eyes out much further than the generality of persons can."

There still remains another class of causes for consideration which may produce luscitas. The circumstance of three distinct cerebral nerves being distributed exclusively to the six comparatively small muscles of the eye accounts sufficiently for their great predisposition to nervous excitement and spasmodic affections. Certain injuries and inflammations of the eye, irritation of the brain from mental or organic causes during dentition, or reflected from more distant parts of the body, as the abdominal or genital organs, or from the peripheric nervous system, as, *e. g.* after scarlatina, &c. frequently give rise to convulsions of the ophthalmic muscles. The local irritation accompanying these convulsions augments the muscular irritability, but chiefly the contractile power, and in time produces some change of structure in the muscle while contracted, the result of which is absolute and permanent shortening. Together with this increase of muscular irritability, the innervation or the action of the arteries may be augmented—conditions on which hypertrophy of the muscle depends, which I have

principally met with when the strabismus was preceded by inflammations of an acute character, directly affecting the eye or its muscles, but which I think may also be caused in cases where inflammatory affections never existed, merely by too copious an afflux of blood to the muscle. If the convulsions have been caused by a purely nervous irritation reflected from distant parts of the body, *e. g.* in worms, I have found the muscle rather thin, hard, and more tendinous. After protracted ophthalmia, which begins with great vehemence, and is the effect of an exanthematic or rheumatic metastasis, the innervation may either gradually become diminished or the nutritive functions impaired, so that paralysis or atrophy may be added to structural shortening, which originated with the metastasis at the commencement of the inflammation.

Under all these circumstances the eyeball is turned towards the shortened muscle, and in time becomes permanently and more or less immoveably fixed in an abnormal position; in the same ratio as the shortened muscle, from change of structure, loses its elasticity, and its antagonist its energy, from being in a constant state of extension. Luscitas, or strabismus from convulsions of the muscles, is, therefore, generally slowly established; but it may also occur suddenly after hysteria, hypochondria, epilepsy, violent mental emotions, or external agents acting chemically or mechanically on the eyeball. I once saw a case, under Prof. F. Jaeger, of Vienna, where an hypochondriac, after some errors of diet and mental excitement during dinner was suddenly affected with a complete luscitas of his left eye. As this person sought medical advice directly, the complete immoveability of the eye soon entirely ceased after the administration of an emetic and some other remedies, which rectified the functions of the digestive organs.

I am of opinion that, with few exceptions, almost every recent case of luscitas or strabismus, and even several of some time standing, which have been brought on by a spasmodic state of the muscle, may be cured by pharmaceutical means, provided the real cause of the spasm can be detected, and proper means are employed. From my own observations it appears to me,

that in all recent cases of this character, whatever plan of treatment the remote cause may require, those means which are locally applied should be antiphlogistic or antispasmodic, as no doubt can exist that stimulating applications will increase the mischief. I have attended several cases of squinting of this kind, brought on by dentition, worms, or disorders of the stomach, and irritation of the brain, where I succeeded in accomplishing a cure by therapeutic means; and in a late number of the *MEDICAL GAZETTE* I have related a case of complete luscitas, caused by spasm from an acute rheumatic attack, which was cured in a short time without operation. Some time ago a lady asked my advice for a slight degree of strabismus affecting each eye alternately, and being of eight years' duration. While speaking with her I observed the left eye affected when she was looking at me, and the right when she looked at a more distant object; I therefore concluded that in this case the strabismus might be dependent on an inequality of the power of vision of the two eyes, and on some irregular action of the muscles, and, on close examination, found a considerable degree of myopia in the right eye, and a rather presbyopic state of the left. In looking at a near object she was in the habit of using the right eye; and in viewing distant objects she used the left, leaving in each case the other eye to the casual action of its muscles, which always drew it slightly outwards. This case of alternating strabismus was cured by local bleeding of the right eye, the use of gentle aperients, by binding up the left while the myopodiorthoticon was employed to the right eye. (See *MED. GAZ.* vol. xxi. p. 866, and vol. xxii. p. 442.)

If proper local pharmaceutical means have been employed for some time without effect, and if the remote cause has been removed, or at least has ceased to act on the ophthalmic muscles, or if the luscitas or strabismus has lasted for a considerable length of time, I think we may firmly conclude that the spasmodic contraction of the muscle has become habitual, or that structural shortening is already established, which now remains as secondary effect, and therefore, even if the case is a complete luscitas, this cannot be considered as a contra-indication to the

performance of an operation. As the muscle is already absolutely shortened, I think it not advisable to divide it in its muscular fibres, much less to excise a portion, as some do, or to push the posterior part back into the orbit, as by these means the muscle would be made still shorter; but it is more proper to divide it in its tendinous portion close to the sclerotica, so that the muscle may retain its whole length, and thus be allowed to attach itself to the sclerotica as far anteriorly as possible, since then the movements of the eyeball towards the divided muscle will be less limited. If, after the tendon and all additional frænula have been completely cut across, the abnormal position is still considerable, this will generally be remedied either by separating the cellular tissue between the muscle and the globe more or less, or by enlarging the wound in the conjunctiva (especially if this membrane is thickened, tough, or inelastic), or by partial division of the inner border of the superior or inferior rectus, as the case requires; but in all these steps the greatest care must be taken, especially if the opposite muscle acts powerfully, to cut rather too little than too much, as otherwise the strabismus will not be cured, but squinting towards the opposite side will be substituted for it. I have never operated on the muscles of a sound eye in order to set its fellow straight; but in cases of double strabismus, if the one eye was not rendered quite straight by a simple division of the muscle, I at once proceeded to operate upon the other eye, which plan was generally attended with success without the additional steps above related. The first of these plans should be pursued in all cases where the antagonist of the divided muscle is more or less paralyzed, since it then has a better chance of regaining its tone by free exercise. If after these proceedings, in cases of inversion, the eye remains still in an abnormal position to a great extent, I divide the superior oblique.

Although we do not even yet perfectly understand the action of the obliques, I think it will be admitted that if either or both are shortened in structure, which they may be as well as the recti, because both groups of muscles are subject to the same pathological conditions, they will increase

the inversion; and that if after the rectus internus, and all other attachments which may retain the eye in its unnatural position, have carefully been cut, some degree of inversion still continues, these muscles, but principally the superior oblique, may be the cause of it, since this (being the longest of the six) is subject to become more shortened than the rest. This I have found to be the case in several instances. I do not mean to say that we should thoughtlessly cut across the superior oblique in all obstinate cases of inversion, since by the proceedings above detailed we frequently succeed in removing the affection completely; but where all these proceedings have had no satisfactory effect, I am convinced that a division of this muscle will either rectify the position of the eye directly, or when some degree of paralysis of the external rectus is the cause of the continuation of the inversion, and if then in such cases the shortened superior oblique is an impediment to the motion of the eyeball towards the external canthus, its division will remove this impediment, and thus the external rectus will be placed in a situation far more favourable for the recovery of its proper action by free exercise.

Besides the two cases related in the *MEDICAL GAZETTE*, vol. xxvi. p. 689, and the one in this paper, I have divided the superior oblique in three more cases, in which, I presume, the result of perfect cures was obtained solely by this additional step in the operation. I therefore think that the omission of the division of the superior oblique, under circumstances above described, is one of the causes of failure, into which a late author on the cure of strabismus has given us a lengthened inquiry; in which, however, he says, "in no case have I ever dreamed of cutting the superior oblique, &c., and I would advise no one to attempt the operation as a cure for strabismus without perusing the observations I have to offer upon the subject." His reason for giving us this advice is a strong fear that on division of this muscle the eyeball would protrude in an extraordinary manner, and that it would be drawn too much outwards. I can, however, assure the reader that none of these consequences occurred in the above six cases, nor in

a few others where a friend of mine divided this muscle; neither have I in my experiments* on animals ever

* I made these experiments, in October last, with the kind assistance of Dr. A. Ure and Mr. F. Fowke, on dogs, rabbits, sheep, and young pigs. Before commencing upon living animals, I examined the action of the ophthalmic muscles in the dead, (the orbit was opened from the skull sufficiently for this purpose, without disturbing the anterior aspect of the eye and its appendages), by pulling them from their points of origin, and then made a clean dissection of them to enable me to divide them in living animals with more certainty.

From these experiments, and from what I have observed in cases of rather complicated strabismus, as also from the published remarks of some recent authors on this subject, together with the experiments and observations made by Prof. Volkmann (*Neue Beitræge zur Physiologie des Gesichtssinnes*. Leipzig, 1836, and in his paper published in *Müller's Archiv*. 1840, No. 4, p. 475.) and by Dr. A. Hueck (*Ueber die Achsendrehung des Auges*, Dorpat, 1838), I might almost draw the inference, that in man the ordinary movements of the eyes inwards, upwards, outwards, and downwards, or in a direction between any two of these, or in circles intersecting these different directions, are performed by the straight muscles only, and independently of the obliques; but that while the straight muscles are moving the eyes in one of these directions, or after they have moved them towards a certain object, the obliques assist the straight in keeping the axis of vision stedfastly fixed upon the object, which either moves while the person is at rest, or which is at rest while the person is moving, or while both the object and the person are in motion, or while the person, perhaps, at the same time turns his head towards one side or the other; secondly, that the obliques act the principal part in accomplishing those minute alterations in the inclination of the axis of vision between their parallelisms and their greatest convergence, which are necessary for their decussation in a given point, (if, while the head is kept in a vertical position, this point is situated on a level with the eyes, the convergence of the visual axis is effected by the four obliques, if situated above by the two inferior only, and if beneath by the two superior only); and thirdly, that by means of the obliques the refraction in the eye is made dependent on the convergence, or the convergence on the refraction. When I made the experiment pointed out by Hueck, according to his plan, in my own eyes, or in those of others, I could never discern so great a rotation of the eyeball upon its axis as that which, amounting at least to 45-50 degrees, Prof. Volkmann and Dr. Hueck attribute to the obliques, each acting alone; nor can I understand why this rotation shall be necessary for single vision when the head is inclining laterally downwards; it, on the contrary, appears to me that such a rotation would twist the optic nerve, and thus considerably interfere with distinct vision, and perhaps cause photopsia. I can, however, not deny that in animals, when I pulled one of the obliques at its origin in the dead, or divided one of them in the living, and then irritated the eye, I have certainly observed a somewhat greater rotation of the ball on its axis than in man; but we must remember that in these the obliques are arranged differently to those in man. I am not inclined to think that the adjustment of the eye to distance is accomplished by a mechanical action of the recti, although it is not impossible that the obliques may exert some indirect influence by effecting the convergence of the visual axis; this adjustment appears to me to exist in a delicate motion of the

observed any difference in the position of the eyeball when I cut across either of the obliques or both of them; but I only then observed a slight alteration in the position of the eyeball, when with one of the obliques one of the recti was divided at the same time. A proof of this fact gives the eighth, tenth, and eleventh of the author's own experiments†, as also those of Sir Charles Bell‡, Mr. B. Cooper§, and the observations of several recent authors on this subject. Dr. Melchior§ says, "*me nullum discrimen in situ oculorum observasse, sive obliquum superiorem alterius modo, sive amborum oculorum secuissem.*"

There remains another ophthalmic affection to be mentioned in connection with this subject, viz. tetanus oculi, or that state of the eye where several or all muscles of the eye are affected with tonic spasm, so that the ball is immovably fixed rather in the middle of the orbit. This may be caused by a blow, by lacerating wounds, by rheumatic metastasis, by organic irritation of the brain, &c.; and it is also said to occur in several nervous diseases. I saw it once, although not quite complete, in a case of trismus; and in another of catalepsia, both eyes were absolutely fixed during each paroxysm by that singular kind of spasm peculiar to that disease. In all these cases therapeutic treatment must be directed towards the primary disease, from which the local spasm arises, or of which it is only a symptom. The local application of proper means is not to be neglected; but the performance of an operation will never be indicated in these cases, except, perhaps, where the tonic spasm affecting only two or three muscles is caused by rheumatism, and has become chronic; but fortunately these cases are rare, and if they occur, a thorough investigation and due con-

lens in the antero-posterior axis of the eye, and to be dependent on the ciliary nerves and body. If the above opinion upon the action of the ophthalmic muscles is correct, I fear we shall never succeed in demonstrating the automatic action of the obliques, by which they fulfil so delicate and minute functions, in themselves means and end, by pulling these muscles at their origin in the dead body, or by dividing them in the living. Some other experiments besides even those by means of galvanism must therefore be thought of.

† *MED. GAZ.* vol. xxvii. p. 349.

‡ *Phil. Trans.* 1833, p. 176.

§ *Guy's Hospital Reports*, vol. iii. p. 475.

§ *Dissertatio de Myotomia Oculi*, 1841, p. 56.

sideration of each particular case, like in all others of a severe nature, will enable us to decide upon the plan of treatment.

PTOSIS RELIEVED WITHOUT AN OPERATION.

To the Editor of the Medical Gazette.

SIR,

THE plan recommended by Mr. Curling for the cure of ptosis is very ingenious, and well deserves the attention of the profession; but Mr. Hunt, of Manchester, has ably pointed out several causes which will often militate against the operation; and in cases where it is not applicable the following contrivance may always be adopted.

Some years ago I was professionally consulted by a family, and felt much interested by the appearance of one of the daughters, who had apparently lost an eye. Upon inquiry, I found that some years previously she had been operated upon for a tumor on the eyelid, and that ever since the operation she had had complete ptosis of that eye. Mr. Curling's operation did not suggest itself to me; but I felt anxious to relieve the poor girl, who withal, independent of this deformity, had a very pretty face; and was at that age when the loss of an eye might probably lose her a husband. The plan I adopted was as follows:—A very thin and narrow piece of ivory, forming the segment of a circle, was riveted upon a narrow piece, about eight inches long, of the main-spring of a watch; the loose end of the spring being carried through the hair over the crown of the head to the occiput. The piece of ivory was then placed upon the eyelid so as to keep it open, and, being very narrow, was completely hidden in a fold of the eyelid. The spring was painted accurately to imitate the colour of the skin; and by the skilful adjustment of a ringlet, which I observed my fair patient always allowed to fall furtively in that direction, it could not be observed, unless the observer was very near. As the eyelids occasionally required closing, in order to keep the eye moist, she soon acquired a knack of raising the spring, allowing the eyelid to fall down, and then replacing it again,

without any one perceiving that she was doing more than just touching her eyelid. In order most effectually to prevent the spring being seen, I advised that she should get one made much narrower and rather thicker, something similar to the frames of the very fine steel spectacles which are at present used; but as I shortly after left that part of the country, I lost sight of the case; and as I afterwards heard the girl was married, probably the gain of a husband might make her indifferent about further hiding the contrivance.

I am, sir,

Your obedient servant,

JAMES MACKNESS, M.D.

Physician to the Hastings Dispensary*.

Hastings, 5th July, 1841.

MEDICAL GAZETTE.

Friday, July 9, 1841.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”

CICERO.

ANIMAL MAGNETISM IN FRANCE AND ENGLAND.

IN England, animal magnetism has almost ceased to be a subject even of popular interest, and has been long dismissed from the list of subjects of scientific inquiry; for we suppose that its now sole professor will hardly put forward a claim for a love of science in his occasional public exhibitions of his own powers and his patients' weakness. But in France it still flourishes in all its folly; and we wish we could say that folly is the least fault with which it can be branded; for of late, more than ever in its career hitherto, impudent falsehood, bold chicanery, and even blasphemy, have been chief features in its doctrines and practice.

A few notices of the contents of the Parisian journals, during the last months, will show what folly, men, serious, and, if the expression may be

* The offered Report will be very acceptable.—
ED. GAZ.

professing themselves reasonable and honest, can be brought to believe or to assert, even in the nineteenth century; and, if they serve no other purpose, will at least suffice to confirm the incredulity which every student of medicine should maintain, by proving what the height of enthusiasm can mistake, or the depth of *charlatanerie* can pretend, to be truth. To prove in one sentence how little magnetizers are capable of fulfilling, before competent judges, the pretensions which they make to the whole public, or even of repeating those marvels which (when they are not well overlooked) are easy enough—to prove this, let it suffice to say, that the sum of three thousand francs, offered by M. Burdin, to any one who would bring a somnambulist that could read with her eyes fairly bandaged over, or could tell what was written on a paper locked up in a box, has never been won, though often tried for in the presence of impartial judges: nay, not one has ever been found who could see to read through a single layer of blank paper.

Now we do not mean to say that a somnambulist being unable to see without her eyes is a proof that animal magnetism is untrue; but the fact we have mentioned is a proof that these French magnetizers are (not to waste words) either fools or impostors: for this sight through bandages and dealboards is what they put forward as being often acquired under magnetic influence, and as having been frequently demonstrated to exist. Nay, they seem to prefer it as a test of the truth of their system. M. Gerdy (who might certainly have occupied himself more to his own dignity and to the public advantage) has been induced to investigate this power of eyeless sight in the choicest somnambulist of M. Pigeaire,

his own daughter. And what was the result of his inquiries? Why, that Mademoiselle Pigeaire could never succeed in reading, till, by her struggles, she had managed to displace the bandage that had been put, with much form and much pretence of caution, over her eyes, and to make a gap above or below its borders sufficient to peep through and use her ordinary sight as other people do. Nor was there any better success with M. Frappart and his somnambulists: all the trials entirely failed: all proved quite plainly that the blindfolded could not see till they had made a chink below, or above, or on the inner side of the eye. A series of experiments, by M. Gerdy, on himself and on his friends, proved how small, how almost imperceptible, a space is sufficient for some persons to see obscurely through or beneath a bandage; and yet in spite of all this, the magnetizers still believed, or pretended to believe, that the power of extraordinary vision was possessed by their patients. What can be said to such men? Plainly nothing; nor can much be done, till, having determined whether they deceive or are themselves deceived, they can be consigned to the care of their friends, or of the public officers of justice. One thing, however, may be done already: every one may refuse not only to comply with, but even to listen to, the statements such men make; their evidence is altogether inadmissible; they plainly either cannot, or will not, tell the truth.

But their feats are far from being confined to simply seeing without eyes. We find a M. Teste professing with unequalled boldness that a somnambulist of his can tell by the mere touch, and, it seems, by one touch only, all the pathological circumstances of any patient presented to her; and M. Ricard, director of the *Journal du Mag-*

netisme, not to be outdone, swears that he can either take to himself or dismiss in an instant the pains and diseases of any other person. Travelling once in Belgium with a brother-magnetizer, who was at the time afflicted with gastritis that he had received from a lady whom he had cured, M. Ricard, for a grand experiment, relieved him of his pain, and took it himself. But, not anxious to keep his new guest long, he again made it go into his companion's stomach; and then, having made the gastritis pass several times from one to the other, they finished, by common accord, with giving it a definitive *congé*.

Of all possessors of personal magnetic power, M. Ricard is certainly the most nobly endowed. The story just told is far from being the chief marvel that his *Traité du Magnetisme Animale* contains. He can make himself sweat at any given part of his body at and with pleasure; and make others do the same: he can alter the rapidity of any man's pulse in any desired mode and degree: he can make any one at will paralytic or cataleptic: he can give magnetic properties to bars of iron by the influence of his epigastrium, and can exercise his powers, and even cure diseases, at miles' distance from him: he can remove affections of the severest kind in half an hour or a little more, though they have lasted for years. But he does not always exercise his powers for good: sometimes he employs them to inflict terrific punishments, and especially he does this to those his somnambulists who would mischievously injure him or the patients put under their influence. On these he at once, and without pity, inflicts the sufferings they intended for others. Nor are his magnetic influences powerful on man alone; they extend over all nature, animate and inanimate: a shrivelled, languishing, and etiolated shrub grew green under the power of

his passes; and on every side threw out strong and luxuriant branches. On the other hand, a beautiful tree, magnetized by his opposed will, shrivelled, lost its leaves, and soon after perished. Nay, he can influence the atmosphere and command meteors; and he has several times, at Montpellier, amused himself by dissipating clouds and stopping rain.

These things are actually gravely and boldly asserted as truths by M. Ricard, who seems a great authority among this extraordinary class of dupes or knaves. To believe a man to be deceived into the idea that he possesses such powers (if he be not obviously a madman) is to be nearly as credulous as he; and it would not be difficult, as the next step, to imagine one's self possessed of them. He is, no doubt, a gross impostor, and might fairly and wisely be left to the usual end of such characters, were it not for the mischief that his proceedings would do. Their influence on the health of the public, and on their intellectual condition, if they are brought to believe in them as realities, is bad enough; but it is not the worst that they can exercise. It is impossible not to perceive that the miracles recorded in Holy Writ are the objects of imitation in all these proceedings. Constituting as they do a chief evidence of the Christian faith, which regards them as possible to God alone, it becomes a matter of no small interest whether men are to be allowed to profess that they possess powers sufficient for their performance; and thus to reduce the wonder of a miracle to nothing more than the crafty exercise of an influence which any man may acquire. And that this is not a strained foresight of what may result from these things, is too evident from what magnetizers themselves advance as the history of their art. M. Gauthier, said to be the most

used, the most scientific of the modern professors, states his conviction that magnetism has been practised from all times, and in all places, either for religious or for medical purposes. He follows traces of it among the Egyptians, Jews, Persians, Greeks, Gauls, and Romans. To him, as to all magnetizers, the sacerdotal physicians of Egypt and of early Greece, the thaumaturgi of all times, the APOSTLES, and the CHRIST himself, were only magnetizers; the PROPHETS and the Sibyls were all magnetizers.

One can scarcely without a shudder read such a blending of the holy and the profane—such an assertion that the divine power of the inspired, and the craft of heathen priests, had all one common origin in what any man may even now acquire. And yet, if the public are made by fraud to believe that magnetizers can do the things that they pretend, we do not see but that in the very next place they must be led to regard one of the chief evidences of Christianity to be nothing more than a cunningly devised fable. That such things should be looked on calmly in France—that the writer from whom we quote the above passages should make no remark of disgust at them—is not astonishing. But we shall be glad if, having thus shewn to what animal magnetism may easily be perverted, we can produce the feeling in England that it is not to be regarded as a harmless folly—as a thing with which a man may fairly amuse himself and gain notoriety. Its evil moral influence on the patients subjected to it we long ago pointed out; and it is now brought freely into the service of the foulest irreligion. With the exception of walking on the waves, and raising the dead, there is not a miracle which it is not deemed competent to perform.

Regarding, however, all these pretences to supernatural power as impudent falsehoods, we cannot find that animal magnetism has made any real progress towards meriting, even in its statements of a lower and more credible character, the name and position of a science. Of all that has been done in France, we can find nothing resembling, or which is not altogether opposed, to probability. With no more incredulity than every man should exercise when new and strange things are proposed to him, we can find no fresh or better grounds than heretofore for the belief that magnetism operates in any way except through an extraordinary influence upon the mind, exercising itself with the more power, and to the production of more strange effects, in direct proportion to the morbid state into which the system has been brought by genuine disease, or by the repeated excitement of the magnetizing process. Such, we believe, is the case with the two girls who we regret to say are still exhibited in London; nor can we learn that the gentleman who operates on them has arrived at any results that are fit for the records of science,—at any thing, in short, more than *wonders*, which, however, we are bound to add, bear no comparison with those that are said to occur in France.

But whether this be the case or not, it is surely much to be regretted that these things should be made, by one who might hold a good place in science, subjects of public exhibition rather than of private and calm study. Who ever heard of a science being advanced or improved by constituting the public the judges of its embryo truths? by exhibiting its unexplained phenomena to the ignorant gaze of women and children, or even of men not educated to appreciate the value of a new truth? We believe that, in following such a

course, Dr. Elliotson only mistakes the way to study magnetism. It will be greatly to his own credit, as well as to that of his profession, if he will take this hint that we offer him in all good feeling, and again enter the ranks of true science, which his talents and acquirements are so well calculated to adorn.

UNIVERSITY OF LONDON.

BACHELOR OF MEDICINE.—FIRST EXAMINATION.—1841.

Monday, July 5th.—Morning, 10 to 1.

Anatomy and Physiology.

Examiners, Mr. Kiernan and Prof. Sharpey.

1. Describe briefly the atlas and vertebra dentata, the mode of ossification of these bones, and their articulations with each other and with the skull. State the movements of which the joints in question are susceptible, and the muscles by which they are effected.

2. Describe the dissection required to show the course and distribution of the axillary artery; mentioning the parts cut through or exposed in the order they are met with, and describing the artery and its branches with their relations to the adjacent parts.

3. The skin being removed to the extent of four inches above, and to the same extent below the knee, at the posterior surface of the limb, describe the parts brought into view in the order in which they present themselves in proceeding with the dissection from the integuments to the bones.

4. Describe the surfaces of the cerebellum, tuber annulare and medulla oblongata, and the cavity of the fourth ventricle. (The nerves not required).

5. Describe the form, situation, connexions, and structure of the pancreas, and state generally the nature of its secretion.

6. Give the structure and chemical composition of the osseous tissue.

Afternoon, 3 to 6.

Examiners, Mr. Kiernan and Prof. Sharpey.

1. The os innominatum, and the muscles, nerves, vessels and fasciæ of the same side being removed, describe the pelvic viscera in the male as they are seen *in situ*; and the bladder and rectum being turned aside, describe the parts which lie between them and the bones in the order in which they are met with in dissecting from within outwards.

2. Describe the parts successively brought into view in dissecting a portion of the back,

limited above and below by the first and eighth pairs of ribs, and laterally by the bases of the scapulæ. The dissection to be carried as deep as the surface of the ribs and intercostal muscles.

3. Describe the structure of the bronchial tubes and lungs. Do these structures take any, and if any what, share in the production of the respiratory movements? Enumerate the respiratory nerves, mention the muscles to which they are severally distributed, and the effects on the respiratory movements of injuries, whether accidental or experimental, of different parts of the cerebro-spinal axis.

4. Describe the tympanum as it appears in the skeleton.

5. Describe the intimate structure of a nerve, a plexus, and a ganglion.

PASS EXAMINATION, M.B. DEGREE.—1841.

Tuesday, July 6th.—Morning, 10 to 1.

Chemistry.

Examiner, Prof. Daniell.

1. Two solutions will be placed before you marked A and B. Describe and explain the changes which take place upon testing one with the other. What do the solutions contain? If any ambiguity should occur with respect to either, select some other test from those before you which will determine the point.

2. What salt is contained in the solution marked C? The appropriate tests will be placed before you: explain the changes which take place upon their application.

3. What is the Newtonian theory of colours? What is the experimental evidence upon which it is founded?

4. What distinction may be drawn between the temperature of a body and the heat which it contains? Refer to experiments.

5. Explain the analogy between a flash of lightning and the discharge of a Leyden jar.

6. What is the origin of the force in the Voltaic Battery? How is it accumulated?

7. What are the laws which limit the combinations of chemical affinity? Illustrate them by examples.

8. State the general views which may be taken of the constitution of salts.

9. What are the respective weights at mean pressure and temperature of 100 cubic inches of the following gases and vapours?

Hydrogen.	Carbonic Oxide.
Oxygen.	Carbonic Acid.
Nitrogen.	Ammonia.
Iodine.	Sulphurous Acid.

10. What is the general constitution of the class of bodies denominated *Ethers*?

Illustrate the subject with particular examples, both by words and symbols.

BACHELOR OF MEDICINE.—FIRST EXAMINATION.—1841.

Tuesday, July 6th. — Afternoon, 3 to 6.

Materia Medica and Pharmacy.*

Examiner, Dr. Pereira.

1. Describe the mode of preparing Corrosive Sublimite; and explain the theory of the process. Enumerate the tests for this salt; and state how you would proceed to detect it when mixed with Calomel. What is the nature of its chemical action on the animal tissues? What are its antidotes?

2. What is the most expeditious mode of preparing Hydrated Sesquioxide of Iron for exhibition in cases of poisoning by Arsenious acid? What reaction takes place when the former is mixed with a solution of the latter substance?

3. Give the botanical characters of *Papaver somniferum*. Mention its Linnean class and order, as well as its natural order. Describe the method of procuring Opium. Briefly state the distinguishing characteristics of Meconic Acid, of Morphia, of Narcotina, and of Codeia. Describe the effects of Opium, and point out in what respects they differ from those of other narcotics, especially Hyoscyamus. What is the immediate cause of death in poisoning by Opium? What are the principal therapeutical indications which this substance is calculated to fulfil? Mention some of the principal diseases in which it has been found serviceable, and point out what circumstances permit or forbid its use. What are the doses of solid Opium, and of *Tinctura Opii*, Ph. L.? What quantity of *Tinctura Camphoræ composita*, Ph. L., contains one grain of opium?

4. In what cases would you prefer Emetic Tartar, as a sudorific, to Dover's powder, and *vice versâ*? Under what circumstances would the latter be preferable to the former? What are the respective doses of these substances when employed to produce sweating? What means would you adopt to promote the operation of sudorific medicines?

5. What are the characteristics of good Extract of Sarsaparilla? With what substances is the *Hydrargyri Ammonio-chloridum*, Ph. L., frequently adulterated, and how would you recognize their presence?

6. What is the class and order, in Cuvier's arrangement, of *Moschus moschiferus*? From what part of the animal is musk procured? What is the dose of this substance?

Botany.

Examiner, Rev. Prof. Henslow.

1. Define the terms Cordatus, Obcordatus, Induplicatus, Trijugus.

2. Describe Pyxidium, Vittæ, and distinguish between Cotyledones accumbentes and incumbentes.

3. What are the principal kinds of Nervation in Leaves?

4. Explain what are the Lacunæ, and Meatus intercellulares of the cellular tissue.

5. Upon what does the Etiolation, and the Fall of leaves depend?

6. What are the functions of the root? and the chief phænomena attending the germination of the seed?

7. Describe the structure of the different parts of the specimens marked No. 1, 2, 3.

RESTRAINT ON LUNATICS.

IN the Report of the Gloucester County Lunatic Asylum for the past year the medical officers (Drs. Baron, Shute, and Hitch, and Mr. Hitch), the following opinion is given with regard to the application of restraint to lunatics:—

1st, That to avoid personal restraint, so far as may be considered compatible with security, has always been a standing and peremptory rule of the Institution, and strictly attended to under all circumstances.

2d, That the combined and corresponding evidence of several large lunatic establishments has clearly shown that personal restraint may be avoided to a much greater extent than was formerly considered either possible or advantageous in the treatment of the disease.

3d, That they dissent from the general proposition that personal restraint is, *under all circumstances*, prejudicial; and are of opinion that, notwithstanding all that has been said upon the subject, the experiment has not yet been virtually and really tried in any large establishment: the confinement of refractory patients in their cells being not only an obvious personal restraint, but most inimical to the future recovery of the patients.

4th, That the moral restraint of example, that is to say, the disposition of lunatics to fall into the habits of a large asylum, in compliance with, as it appears, and in imitation of, the examples of those with whom they associate, is a most influential agent in the treatment of the disease, and gives to large establishments a power of avoiding personal restraint to an extent which can with difficulty be accomplished in smaller institutions, and has always been found impracticable in private families.

* Number of candidates, 80.

LUNATIC ASYLUMS.

(Circular.)

DEAR SIR,

It having been long felt desirable that the medical gentlemen connected with lunatic asylums should be better known to each other—should communicate more freely the results of their individual experience—should co-operate in collecting statistical information relating to insanity—and, above all, should assist each other in improving the treatment of the insane,—several gentlemen who have the conduct of lunatic asylums have determined on making an attempt to form “an Association of the Medical Officers of Lunatic Asylums.”

For this purpose they propose to meet *annually*, at the time and place “the British Association for the Cultivation of Science” shall select for holding their meetings; and to hold a first or preliminary meeting this year, on the 29th of July next, at Devonport.

I have been requested by these gentlemen to learn how far their brethren will co-operate with them; and I shall feel it a personal kindness, therefore, if you will, as soon as possible, give me your opinion upon this proposed Association, and also inform me if you will give it your support.

I beg to remain, dear sir,

Your obedient and faithful servant,

SAMUEL HITCH,
Resident Physician, Gloucester
General Lunatic Asylum.

Gloucester, June 19, 1841.

AMERICAN MEDICAL SCHOOLS.

NUMBER OF STUDENTS.

THE following are the numbers of medical students at the American Universities during the past season.

University of Pennsylvania.—The entire numbers of those attending the medical classes, 410.

Albany Medical College.—The entire number of students 122, of whom 29 have taken degrees in medicine.

Institute of Louisville.—The medical class numbered 205.

Geneva College.—Total number of medical students 136, of whom 34 graduated in medicine.

Harvard University.—Number of medical students, 88.

Transylvania University.—Number of medical students 254, of whom 62 received the degree of M.D.—*American Journal of Medical Science.*

SPECIMEN OF THE EFFECT OF A
LIGATURE UPON THE FEMORAL
ARTERY.

PROFESSOR HARRISON said he had lately had an opportunity of examining the artery of a man who had been operated on for popliteal aneurism about eight or nine years ago in the Richmond Hospital. The case had been under the care of the late Dr. M'Dowell, who took up the artery in the upper third of the thigh, and the man was discharged, cured, in the course of a few weeks. He continued to enjoy good health until a short time since, when he was attacked with bronchitis, for which he was taken into Sir Patrick Dun's Hospital. At the time of his admission he was in a state of great exhaustion, and survived only a few days. On dissection, the lungs were found to be emphysematous, and exhibited the anatomical characters of bronchitis. There was nothing remarkable in the heart, except some hypertrophy of the right ventricle, but the aorta shewed numerous traces of organic alteration. It retained its cylindrical form, without any tendency to collapse of its walls, and at some points the sides of the vessel could not be approximated without considerable force. Its interior was found to be thickly studded with calcareous scales, covered in most places by the lining membrane, but in some the lining membrane was deficient over the calcareous deposit. At the place where the ligature had been applied upon the femoral artery, there was obliteration of the canal of the vessel to the extent of an inch and a half. Half an inch above this obliterated portion, the profunda artery arose, and appeared to be considerably dilated. Below it the femoral artery was pervious down to the ham, where it again became obliterated to the extent of about two inches. The articular arteries were enlarged.—*Dublin Journal of Medical Science*, July, 1841.

POISONING WITH ANTIMONY.

DR. LOHMEIER, of Schönebeck, has related some interesting cases of poisoning with antimony which occurred to workmen employed in the manufacture of anatomical preparations. They were exposed, in their operations, to the vapours of oxide of antimony, antimonious and antimonie acids, and hydrochlorate of antimony. The symptoms were, slight pain of the head, with tightness of the chest, gradually increasing to pain, and severe stitches, and accompanied with a dry, racking cough. To these symptoms succeeded swelling of the cervical glands; burning and lancinating pains at the back of

the neck and head; scanty expectoration, accompanied with sibilous râles; nocturnal sweats, and diminished appetite. Diarrhoea, with griping pains, and enlargement of the abdomen, came on, and subsequently stranguary, pains of the testicles, with loss of desire, proceeding to actual impotence, and ultimately shrinking of the penis, and atrophy of the testicles. The symptoms, after disappearing under treatment, broke out afresh on the individuals being again exposed to the anatomical vapours. Dr. Lohmeier recommends local bleedings, and the administration of bark, for the treatment, and strong currents of air through the manufactories, for the prevention of the injurious effects to which the workmen are exposed.—*Casper's (Wochenschrift, April and May 1840;)* *Edinburgh Monthly Journal of Medical Science.*

THE ART RATHER THAN THE SCIENCE OF SURGERY.

WE find from a late number of "L'Expérience," that in the short time since the first performance of the operation for squinting, no less than 49 different instruments for its performance have been invented in France and Germany, and are to be found at M. Charriers's. We fear that a considerable addition to their number, if not to their utility, might be made from our own country. The chief inventors are MM. Dieffenbach, Phillips, Jules Guérin, Velpeau, Leroy d'Etiolles, and Lucien Boyer.

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Monday, June 28.

G. H. Williams.—H. B. Greene.—G. Canney.—J. Harrison.—J. Lithgow.—T. B. Anstie.—S. Beecroft.—S. J. Boulter.—R. Sharpe.

Friday, July 2.

C. M'Carthy.—J. S. Collins.—T. Beale.—G. Codd.—G. F. Blacker.—H. P. Haydon.—E. Vise.—T. Guy.—F. Wood.—G. B. Portus.—R. R. Perry.—M. Spotswood.—R. Buchanan.—W. G. Tiley.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, June 10.

Edwin Fennel, Brighton.—James Thompson, Clitheroe.—Thomas John Blake, Salisbury.—Richard Henry Bonner, Spalding.—Henry Lang.—Edward Halford.—Benjamin Arthur Brickwell, Amersham Bucks.

Thursday June 17, 1841.

George Wakefield, London.—Hugh Eccles Walker, Chesterfield.—William Evan Moss, Lancaster.

PROVINCIAL MEDICAL ASSOCIATION.

WE beg to direct attention to the announcement from the Provincial Medical and Surgical Association, by which it appears that the next meeting is to be held at York, on Tuesday, the 3d of August, and the two following days. Dr. Steed, of Southampton, is President for the present year, and Dr. Goldie, of York, the President-elect.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 26th June, 1841.

Small Pox	13
Measles	24
Scarlatina	14
Hooping Cough	30
Croup	3
Thrush	9
Diarrhoea	3
Dysentery	0
Cholera	0
Influenza	5
Typhus	14
Erysipelas	2
Syphilis	0
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	143
Diseases of the Lungs, and other Organs of Respiration	263
Diseases of the Heart and Blood-vessels	18
Diseases of the Stomach, Liver, and other Organs of Digestion	54
Diseases of the Kidneys, &c.....	5
Childbed	6
Ovarian Dropsy	0
Diseases of Uterus, &c.	1
Rheumatism	1
Diseases of Joints, &c.	1
Ulcer	0
Fistula	0
Diseases of Skin, &c	0
Diseases of Uncertain Seat	75
Old Age or Natural Decay	46
Deaths by Violence, Privation, or Intemperance	23
Causes not specified	2

Deaths from all Causes 755

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N. Longitude 0° 3' 51" W. of Greenwich.

	THERMOMETER.		BAROMETER.	
<i>June.</i>				
Wednesday 30	from 50 to 66		29.88 to 30.02	
<i>July.</i>				
Thursday . 1	51	66	30.02	29.98
Friday . . . 2	58	68	30.02	30.10
Saturday . . 3	58	75	30.10	30.00
Sunday . . . 4	56	72	29.94	29.96
Monday . . . 5	53	73	30.02	29.89
Tuesday . . 6	56	67	29.61	29.80

Winds variable, S.W. prevailing.

On the 30th, ult. generally clear. The 1st inst. and following day, overcast, with frequent showers of rain. The 4th, and following day, generally cloudy; rain on the afternoon of the 4th. The 6th, morning cloudy, with heavy rain; otherwise clear.

CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, JULY 16, 1841.

LECTURES ON THE PRINCIPLES AND PRACTICE OF PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XLII.

*Treatment of Intermittent Fever: during
the paroxysm; during the intermission.
Prophylaxis.*

I WAS about, when we last separated, to consider the treatment of ague: first, during the paroxysm; secondly, during the intermissions.

Treatment during the fit.—In this climate we need not, I say, encumber a patient in an ague-fit with too much help. But in hot countries, where the disorder is apt to run into the remittent, or even the continued form, and where, during its violent and rapid course, internal organs are liable to sustain serious damage, the best and indeed almost the only time for the effectual interference of the physician is in the first assault or paroxysm of the disease.

The objects of treatment during the paroxysm are, to alleviate the uneasy sensations of the patient; to abridge, if possible, their duration, by shortening the fit; and to avert the danger which, under certain circumstances, may arise from intense internal congestion long continued, or from the severity of particular symptoms.

Now in the cold stage of ague, *diluent drinks* have been recommended, and *cordials*, and *external warmth*, and *opium*, and *emetics*, and *blood-letting*. One would suppose that if some of these expedients were useful, others could scarcely be so too. The *diluent drinks* are very proper: and I should allow the patient to use his own discretion in the choice of them. It was customary,

formerly, to prescribe medicated drinks of this kind; and one pleasant, but neglected ptisan still lingers in our Pharmacopoeia, the decoctum hordei compositum. Now-a-days we are contented with the simple barley water, toast and water, weak tea, gruel, and the like. These diluents should be taken warm, and for persons who are very feeble or exhausted, they may be made gently *cordial*; weak negus, for example, or white wine whey, may be given.

External warmth, being what nature and common sense would suggest, is certainly advisable and beneficial in the cold fit; even the warm bath, if it can be procured. In some places it is the custom to await an expected fit *in* the warm bath. When this cannot so conveniently be obtained, the pediluvium may be employed; or the patient may be put into a warmed bed, and have bags of hot salt or bran applied to his epigastrium; and a hot bottle, or a hot brick, wrapped up in flannel, to his feet. Or, what perhaps is best of all, he may have a hot air bath *applied to him*, as he lies in bed. This may be very easily done, by means of a semicylinder or cradle of wicker work, closed at one extremity by a board. This is laid over the patient, and then covered with blankets. Through a hole in the centre of the board one end of a curved iron tube is passed; the other end, expanded into a bell, looks downwards; and a spirit lamp being placed beneath it, the air between the wicker work and the sick person is soon made very hot. This apparatus was constructed, and introduced into the Middlesex Hospital many years ago, by Dr. Gower; and we often find it of the greatest utility. External warmth applied in some one of these ways, affords singular comfort oftentimes, and contributes to shorten the cold stage. And the same may be said of friction, with stimulating liniments, along the course of the spine. Lind found that, in children, rubbing the spine with an embrocation composed of equal parts of soap liniment, and laudanum, at

the approach of the cold stage, often prevented the paroxysm.

Opium has often been exhibited in the cold stage, with the view of cutting short the fit; and not without some success. The strongest evidence of its usefulness in that stage of the paroxysm is furnished by Dr. Trotter, in his *Medicina Nautica*. Agues being very frequent among the crew of the *Vengeance*, he resolved to try the full effect of opium in preventing the fit. At its first approach, a dose of laudanum (never less than thirty drops) was given; if this did not bring on some warmth within ten or fifteen minutes, from twelve to twenty drops more were administered. In most cases, "in a few minutes an exhilaration of spirits was perceived: the pulse from being weak, quick, and sometimes irregular, became less frequent, full, and equal; an agreeable warmth was diffused over the whole frame, and every unpleasant feeling vanished, sometimes in a quarter of an hour. The patients were themselves surprised at the sudden change in their sensations." Dr. Trotter speaks of these as being the completest cures that ever came under his observation. If, at the next period, the paroxysm threatened to recur, the opiate was repeated always with the same success. "Few instances were met with where any indisposition indicated a third attack at the expected period of accession." Notwithstanding this testimony, it appears that opium is still better adapted to another stage of the paroxysm.

Emetics were formerly much prescribed in the cold stage, at its earliest approach. Cullen recommends them; and they may sometimes be useful, in spite of Chomel's assertion that they are always hurtful. That they have gone so much out of fashion is, however, a proof that they cannot be depended upon for cutting short the paroxysm. Vomiting is itself no small distress to many persons; and for my own part, I should not think of giving an emetic unless some indications of a loaded and oppressed state of the stomach were present; such as nausea, an ill taste in the mouth, a coated tongue, and foul breath. A scruple of ipecacuan will even then be sufficient. The object is to empty the stomach effectually, but mildly. I would not give antimony. Irritability of the stomach, in the severer of these fevers, is too apt to arise spontaneously. Sir Gilbert Blane tells us that the greatest impediment to the cure of the severer intermittents at Walcheren, in their early stages, proceeded from the extreme irritability of stomach, which made it difficult to administer the requisite medicines. In hotter climates nausea and vomiting are still more common and more urgent; and we have to guard against the risk of inducing or aggravating these

symptoms. "Emetics (says Dr. Mackintosh, in his *Practice of Physic*) have been often extolled, but I believe every experienced tropical physician will agree with me in cautioning young practitioners against their indiscriminate employment. Irritability of the stomach is one of the most frequent and troublesome symptoms; and once excited, it is always difficult, and in many cases impossible, to restrain it. I have seen emetics exhibited and the vomiting has continued till death, in spite of every remedy."

Lately, the practice of *blood-letting* in the cold stage has been revived (for it is not a new practice), and strongly recommended by the physician whose name I have just mentioned; and whose opinion carries with it the more weight from its having been founded on much personal experience in the treatment of these fevers. Dr. Macintosh affirms that bleeding, performed in the cold stage, will often stop at once the paroxysm, and with it the disease: that even when its curative effects are less decisive, it will generally stop the cold stage, and shorten the paroxysm, and mitigate its severity, and afford speedy and great ease to the distressful sensations of the patient; and that any subsequent paroxysms that may occur will be mild and few. One bleeding, he says, is commonly sufficient; sometimes two are required; seldom more than two. The blood is to be suffered to flow till the patient feels relief: which usually consists in liberation from pain of the head and loins; freedom of respiration; the departure of the painful sensation of cold; and the cessation of the tremors and of the debility. Most of the patients fall asleep after the operation. These effects have been produced by the abstraction of an ounce and half of blood; they have sometimes (but rarely) required for their production twenty ounces.

Now this is the piece of practice to which I adverted at the close of yesterday's lecture, as being, in my humble opinion, inexpedient, and not to be recommended; at least in the agues of this country. I have seen a good many cases first and last, and certainly I have never seen one in which I could have thought such an heroic remedy necessary, in the cold stage; if indeed it be, in that stage, a remedy at all. But I do not desire to oppose my experience alone, or my judgment, to that of Dr. Macintosh. His method has been tried, since he first made it public, by various practitioners in this country. Drs. Townsend and Law, of Dublin, found it fail in the majority of cases. In Dr. Stokes's hands, the most usual effect of blood-letting in the cold stage was, to check the shivering; and, next to this, to mitigate its severity, without

abridging its duration. In most instances, no modification was produced of the hot and of the sweating stages. In Dr. Kelly's experience, the general effect was, to shorten the cold stage, and to render the hot one milder; but in some cases it seemed to aggravate the symptoms. Mr. Gill found that, although the blood-letting might cut short the cold stage, it appeared to lengthen the period of febrile disturbance.

Confining myself, then, to intermittents as they shew themselves in this climate, I cannot advise you to adopt the practice introduced by Dr. Macintosh — of bleeding in the cold stage. I object to it because it appears to me quite unnecessary; because it is not such as the nature of the symptoms would suggest; because it tends to produce subsequent debility, which we should not needlessly inflict; and because the experience of other sober-minded men, who have given the method a fair trial, does not bear out the statements made by Dr. Macintosh in respect to its usefulness.

At the same time, after a careful perusal of nearly a hundred cases adduced by Dr. Macintosh to illustrate the efficacy of this measure, I think it highly probable that blood-letting may constitute the most important part of the treatment, in the very outset of the severer malarious fevers of hot climates; attended as they are with a degree of internal congestion and disturbance which is dangerous to the integrity of vital organs.

If, in this country, bleeding be requisite at all, it is in the *hot* stage. But it is not requisite at all, except when there appears to be danger of some internal inflammation. The best remedy of the hot stage is undoubtedly opium. Dr. Lind, who wrote after large experience, says that he never saw a person die in the cold fit, but had known several carried off in the hot one, with strong convulsions and delirium. He happened to notice the beneficial effect of an opiate given while the patient was very hot and feverish. He determined therefore to make further trial of opium in the paroxysm. "Having at that time (says he) twenty-five patients labouring under intermitting fevers, I prescribed an opiate for each of them, to be taken *immediately after* the hot fit, provided the patient had then any inquietude, headache, or any such symptom usually subsequent to the fever. The consequence was, that nineteen in twenty-two received immediate relief; the other three had no occasion to take it."

"Encouraged by this surprising success, I next day ordered the opium to be given *during* the hot fit. In eleven patients out of twelve to whom it was thus administered, it removed the headache, abated the fever, and produced a profuse sweat; which was soon followed by a perfect intermission.

Since that time I have prescribed an opiate to upwards of three hundred patients labouring under this disease; and I observed that if taken during the intermission, it had not the least effect, either in preventing or mitigating the succeeding fit; when given in the cold fit, it once or twice seemed to remove it; but when given half an hour after the commencement of the hot fit, it generally gave immediate relief."

Dr. Lind goes on to state that he found the influence of opium more uniform and constant in intermitting fever than in any other disease; and more quick and sensible than that of any other medicine.

Very little need be said in regard to the sweating stage. Up to a certain point the perspiration is to be promoted and encouraged. When the uneasy feelings of the patient have abated, it should be restrained; not suddenly, but with caution. Now the sweating may be promoted by diluents; by keeping the patient in bed, and covered with moderately warm clothes; by sippings of hot gruel, or of hot chicken broth. On the other hand, when the sweating has continued long enough, it may be stopped by drying the patient carefully with towels, changing his linen, and getting him up, out of bed.

It is well to bear all this in mind; but I repeat once more that in agues, such as you are likely to meet with in this country, it is unnecessary, and therefore objectionable, to be *over-busy* during the paroxysm. Wherever the disorder assumes a distinctly intermitting form, the most important part of the practice is that to be employed during the intermissions. Now there are certain general remedies advised for adoption in this period; and there are certain specific remedies. The general remedies are bleeding, emetics, and purgatives. They need not detain us a moment. Blood-letting may be used if there be any apparent tendency to local inflammation, or any marks of severe topical congestion; especially in young and robust subjects. Barring such circumstances, there can be no occasion to bleed your patient in the intermissions.

An emetic given a short time before the expected paroxysm has been known to prevent its accession; and even has sometimes cured the disease. But we can stop the paroxysms by gentler and better means; so that I should not prescribe an emetic unless I saw symptoms of a foul and loaded stomach.

Purgatives should always be given at the outset. They clear the stomach and intestines of hurtful accumulations, which are apt to impede the beneficial operation of the quina, or of other drugs given to check the disorder. I mentioned in the last lecture my own custom in this matter; viz. to give a couple or three grains of calomel with

eight or ten of rhubarb at bed-time ; and the specific remedies may be commenced with the next day.

Of these specific remedies, bark and arsenic are by far the most certain and important ; but a multitude of others have been highly praised for possessing similar virtues. I shall by and by say a word or two about *some* of these, because bark is dear, and arsenic is scarcely a safe drug to be entrusted to the hands of unprofessional persons ; and it is often expedient, in country places, where agues are rife, to provide the poor with remedies which they may have at hand ; and which should both be reasonably cheap, and perfectly safe.

I shall not detain you with any account of the difficulties and objections which were thrown in the way of the Peruvian bark, upon its introduction into the *materia medica* about the middle of the seventeenth century. Its use met with the most violent opposition, even from physicians of the highest authority. It was resisted by Stahl and Hoffman ; and Boerhaave was never quite reconciled to it. Sydenham, by his example and recommendation, greatly promoted its adoption in this country. All this history is sufficiently curious and interesting, but I have no time for it : and you will doubtless hear it from one of my colleagues. I will merely say that in the Peruvian bark we have one of the very few *specifics* that we can boast of possessing ; and that, unlike most other highly vaunted substances, so far from falling off from the accounts first given of its virtues, it has acquired in the lapse of time an increase and stability of reputation.

Neither shall I enter at all into the consideration of the qualities of the several species of cinchona ; nor of the several principles that may be educed from them ; nor of the modes in which the quina even may be best procured. This would not belong legitimately to my province. I must suppose that the professors of chemistry and of *materia medica* have furnished you with the sulphate of quina, which is the only preparation of the bark I intend particularly to notice : and *my* business is to tell you what I know in respect to its employment as a *remedy for ague*.

I may observe, however, that this is a remedy to which we could never have been led by any process of reasoning. It is a matter of pure empiricism. We know nothing of the seat or the essential nature of the disease ; we are equally in the dark as to the *modus operandi* of the quina in curing it ; yet our knowledge of ague, upon the whole, estimated in reference to its precision and practical bearing, is more satisfactory than of many other complaints, with the seat and nature of which we are much

better acquainted. The group of symptoms is so distinct, that we have no trouble or doubt as to the diagnosis ; and experience has taught us a remedy which is all but infallible.

The discovery of quina and its salts formed a great era in the history of the *materia medica*. As far as my own experience goes, the sulphate of quina has quite superseded the necessity for exhibiting any other form of cinchona for the cure of ague. Before quina was unshrouded by the chemist, the bark in substance was the only form in which the remedy could be confidently relied upon ; and I am old enough to be aware of the infinite superiority of the salt, over the actual bark. To obtain the desired effect, it was often necessary to give it in such quantities as almost justified Mr. Abernethy's sarcastic way of speaking of it and of physicians. He said the doctors talked of throwing in the bark, as if it were to be pitched into the stomach with a shovel. The sulphate of quina lies in a much smaller compass, and a more commodious form ; and it does not cause that insupportable nausea which the woody mass of the powdered bark was so apt to occasion.

I am in the habit of giving two, and sometimes three, grains of the sulphate of quina every four or six hours during the intermission, to those patients whom I have occasion to treat for ague. This plan has succeeded so well, that I have never been tempted to try any other. I may, indeed, say that I have never known it fail to stop an ague ; and that in a short time : so that very few paroxysms have occurred after the patient has begun to take the medicine. You may give it in the infusion of roses, which contains a convenient quantity of sulphuric acid to ensure the solution of the sulphate of quina. It changes the colour of the infusion, however, and renders it pinker and opaque. Whether the draught be more or less elegant on that account, I will not take upon me to say ; I know that the virtue of the quina is not interfered with by the change. In private practice, I commonly prescribe as many drops of dilute sulphuric acid as there are grains of the quina, with a drachm of the tincture of orange peel, and a drachm of the syrup of the same ; completing the draught with water. This I find my patients commonly approve of, except in its bitterness, which, in solution, nothing can disguise. Or the salt *may* be administered in the shape of a pill : it is best, however, and surest in solution.

A question has been raised, whether this remedy should be given in repeated doses during the intermissions, or whether one very large dose should be given a short time before the paroxysm is expected. Dr. Home made some experiments on that point in the

clinical wards of the Edinburgh Infirmary, some time ago; and he thought that the result was in favour of the plan of giving the bark regularly at short intervals. I have told you the amount of my own experience, which, however, is not very great; nor have I had any severe cases to deal with. I think it not improbable that my patients would have been cured quite as soon if I had given the remedy in half the strength. Dr. Barker, of Dublin, has found small doses equally effectual with large ones; and this is very likely to be the case with *specific* remedies. It would appear, however, that in some quartans it is better to give large doses before the return of the paroxysm. Dr. Elliotson gives large doses just *after* the paroxysm; and then smaller doses during the remainder of the intermission, at regular periods. A great majority of those who suffer ague are poor persons. Of course the first object is to make the cure as *speedy* as possible; the next to make it as *cheap* as possible. So that it is not a matter of indifference, or mere speculative curiosity, to ascertain with how small doses of quina you may cure an ague. I repeat that it has not happened to me to be disappointed, when I have given the medicine in small doses, as already described; which amount to about twelve grains in twenty-four hours; but, then, I suppose my cases have been well behaved and submissive. Dr. Elliotson states that he is continually obliged to give twenty or thirty grains in the twenty-four hours, before he can cure the complaint; sometimes in obstinate quartans, forty-five grains; and he mentions one case in which a scruple of the sulphate of quina, with ten minims of the liquor arsenicalis, were given every eight hours in vain, but succeeded perfectly when given every six hours.

It appears also, upon the testimony of careful observers, that in warm climates larger doses *are* required; and that it takes a *larger quantity*, upon the whole, to repel the complaint. In the aguish tracts of Italy, in the Maremma, small doses are said to be inadequate; and the physicians there are in the habit of giving twelve, twenty-four, or even thirty grains at a time: and in one recorded instance, the dose, in seven days, was got up to 108 grains, before the ague was arrested. The medium dose, in many parts of America, seems to be eight grains.

It sometimes happens that the irritability of the stomach is so great as to make it difficult to introduce a sufficient quantity of the remedy into the system. This difficulty was very much felt at Walcheren: it is in a great measure removed since the discovery of quina. But even the quina sometimes sits ill on the stomach; and it is often very difficult to get children to swallow any preparation of bark, on account of its bitter

taste. It is an important thing to know, therefore, that it has been found scarcely less effectual, in curing the disease, when thrown up into the rectum. The menstruum in which it is dissolved should not exceed two or three ounces, lest the bowel should reject it. Its expulsion may sometimes be prevented by adding a few drops of laudanum to the enema.

It is said that bark in substance will sometimes cure the disease when quina fails. I have never witnessed this: but in obstinate cases I would give the quina in the *decoction* of bark.

You must not be satisfied with merely stopping the paroxysms. Patients will often be too ready to give up their medicine, as soon as the paroxysm has once missed. But the disease is very apt to recur; and it will always be right and prudent to go on with the quina for ten days or a fortnight after the patient *seems* cured, gradually diminishing, after the first week, the amount and the frequency of the doses.

There have been some curious facts observed in regard to the relapses that are apt to take place after the bark or the quina has been omitted. Clark, of Dominica, states that if no more of the remedy be taken, in the West India ague, than is barely sufficient to stop a fit, and then the bark is suspended, a relapse may take place on the eighth day, in the case of a quotidian; on the fourteenth or fifteenth in the case of a tertian or double tertian; and on the twenty-first or twenty-second in the case of a quartan: thus making (you see) in each type, seven periodical revolutions from the time the fit was suppressed to the next attack; and the fit was found to return on the proper day, at the same hour at which it would have returned if its course had not been interrupted by the administration of the remedy. All this is very curious, and inexplicable; but it points clearly to the propriety of continuing the remedy for some time after the disease appears to have vanished.

Arsenic is another substance which has great and unquestionable power over ague. It carries with it these marked advantages: it is efficacious; it is cheap; and it is tasteless. It is well adapted by these qualities for the poor, and for children, and for patients of every age and rank in whom there is much irritability of stomach present; but then it has also the serious disadvantage of being an active poison. One over-dose may be fatal: and even its long-continued use in minute doses leads sometimes to evident and lasting disorder of the health. Arsenic, therefore, is an unsafe remedy to be trusted in the hands of the ignorant. It should never be administered except under the immediate supervision of a medical eye; and

even then it requires to be given with much caution. Its bad effects may be very certainly prevented, however, by care and attention; and it becomes a valuable instrument of cure, and should be adopted without scruple, in cases where its operation can be watched, and where the quina does not agree with the stomach, or fails to stop the disease. I often prescribe arsenic for other complaints; but, as I said before, I do not recollect ever having been foiled in removing ague by the sulphate of quina. Some persons are of opinion that relapses are less frequent after the cure by arsenic than after the cure by bark. It would require a large induction of particular facts to make that point clearly out.

When substances, which even in small quantities are active poisons, are used as remedial agents, it is convenient to have some definite form in which they may be administered at all times, and in all places. The liquor potassæ arsenitis of the London Pharmacopœia supplies such a form. This is the form in which it was recommended to the public by Dr. Fowler: and it is therefore sometimes called *Fowler's solution*. It was founded upon an analysis of the *tasteless ague drop*, which had been in considerable repute in some parts of England. The pharmacopœial preparation is an arsenite of potass in solution. There are 80 grains of arsenic in the new or imperial pint, and therefore four grains in an ounce of the solution. Ten minims two or three times a day is a full dose for an adult; and you had better commence with not more than five minims. Ten minims contain one-twelfth of a grain. Twice that quantity has been administered at once; but this ought never to be done except when the system has been gradually inured to the arsenic, and thereby enabled to bear such a dose. It is a good precaution not to give this corrosive substance on an empty stomach.

The poisonous or hurtful effects of the mineral that we have to look out for are loss of appetite, nausea, and sometimes vomiting; griping pain of the stomach and bowels, and diarrhœa; and if the medicine be continued, *fainting* is often added. Other symptoms, less constant perhaps, and less important, are painful and hot tumefaction and stiffness of the face and eyelids, or even a tingling eruption something like nettle rash. These effects may, I believe, be controlled by adding a few drops of laudanum to each dose; but I would rather advise you to suspend the use of the arsenic; or to leave it off altogether. When this is done, the unpleasant symptoms will readily yield to mild laxatives, followed by opiates.

When the paroxysms continue to recur in spite of the bark, it has been recommended (and I think the plan a good one) to try to

stop them by arsenic; and then, the periodic recurrence having been broken, to employ sulphate of quina to prevent a relapse.

These, then, quina and arsenic, are the two sheet anchors to which we trust, in the cure of ague. A host of other remedies, I say, have had their praises sung. I do not intend to enumerate them. But there are a few which I think it right to mention, for reasons already assigned. There is strong evidence of the efficacy of some of them; they are cheap, and easily accessible, and above all safe; and therefore, in aguish districts, they may with much propriety and benefit be recommended to the poorer classes, or distributed by Lady Bountifuls.

One of these is willow-bark; in substance, or in decoction. If this does cure agues, as it is affirmed to do, it would seem as if Providence had placed the antidote alongside of the poison; for these trees, as you know, abound and flourish in marshy places. The bark of the willow furnishes an alkaloid substance called *salicine*, in which the febrifuge property is believed to reside. Holly leaves, and *ilicine* derived from them, stand in much the same repute in France, as willow bark here.

Another curious remedy, said to be very successful, is the web of the black spider, which inhabits barns, stables, and cellars. This substance has been tried on a tolerably large scale, and the testimony to its influence in curing ague is very strong. Dr. Craigie has given this account of it. In the year 1760, a number of prisoners from the vanquished squadron of Thurot having been landed in the Isle of Man, Dr. Gillespie, who was practising there, found that many of the agues which came to prevail both among these prisoners and the inhabitants of the island, obstinately resisted bark, and such other remedies as he had recourse to. He was informed, by an old French physician belonging to the squadron, of the alleged efficacy of cobweb, in certain forms of the disease. He therefore made trial of cobweb, and found it to answer admirably. He was successful with it in more than sixty cases of different types, in the Isle of Man, and he had farther experience of its utility subsequently in Ayrshire.

After this, the same remedy was tested in the West Indies, by Dr. Jackson, to whom Dr. Gillespie had recommended it. Dr. Jackson's observations were made in the hospital of the army dépôt, in the West Indies, in 1801. Several cases of ague, on which bark, arsenic, or mercury, singly or alternately, had made either a very temporary impression or none at all, were selected for experiment. And in four of these cases, two pills, containing each five grains of cobweb, were given at intervals of two hours, commencing six hours before the anticipated

time of the return of the paroxysm. The fit did not return. On subsequent trials it was found not only to arrest the course of agues, but to remove various symptoms, such as pain, delirium, vomiting, griping, in ague and in continued fever, when these symptoms were unconnected with inflammation.

Charcoal is another substance which has been found effectual for the cure of intermittent fevers. You may find an account of it in the 10th volume of the *Edinburgh Medical and Surgical Journal*. It would seem to be especially useful in those cases in which there is a marked disturbance of the digestive organs; nausea, flatulence, hiccup, diarrhoea, or dysentery. It is said generally to cure the complaint by the time two drachms of it have been taken. It may be given in doses of ten or twenty grains, in arrow root; or with a few grains of rhubarb. If the power of this substance should be confirmed by future observations, a cheap remedy would thus be open to the poor. A clergyman of my acquaintance assures me that he seldom fails to cure agues among his parishioners by administering to them the snuff of candles, which he takes care to have collected. He does not inform them of what his black powder consists. I presume that its virtue may proceed from the charcoal it contains; unless it is derived from the confidence his flock is accustomed to place in his specific. The very same remedy, the snuff of a candle, is mentioned by Lind.

Piperine, the crystalline salt of pepper, has obtained a considerable reputation of late years, as a remedy for intermittent fever. It was largely tried by an Italian physician, Meli; and Dr. Gordini has repeated Meli's experiments at the hospital at Leghorn; and the following are the general conclusions at which these physicians have arrived:—1. Piperine, in doses of six or eight grains, cures intermittents. 2. It is more efficient in powder than in pills. 3. It succeeds in certain cases in which the sulphate of quina fails. And 4. It is more effectual in preventing relapses. I have seen letters from some practitioners in this country, bearing testimony to the power of the piperine. That pepper will cure ague, has long been the vulgar belief; and a very popular remedy for the disease is a tea-spoonful of pepper in a glass of gin.

I presume that the efficacy of chamomile flowers in the removal of intermittent fever is to be attributed to the piperine which they have been ascertained to contain. These flowers had been long in use for the treatment of ague, before the Peruvian bark was discovered; and they are said to have accomplished a cure, since that time, after the bark had failed; but this was before the quina had been educed from it. Heberden advises

us to have recourse to chamomile flowers, if the bark should disappoint us. I am always willing to embrace an opportunity of referring to his commentaries, for the exact observations they contain, but above all for the beautiful Latinity of which the whole book is an example. I recommend it strongly to you, next to Celsus, the best model you can study for good medical Latin. In reference to the point before us he says, “Cortex, quanquam rite sumtus, interdum parum efficax est: quo in casu suspicio erit ventriculorum sordibus onustum vim remedii impedire. Itaque vomere oportet; quo facto, febris raro non cedit. Quod si redire perseveret, confugiendum est ad flores chamæmeli, quorum contritorum scrupulus dandus est loco drachmæ cinchonæ, et ad idem præscriptum repetendus. Hos flores, sic sumtos, semel atque iterum profecisse expertus sum.”

Several mineral substitutes for the bark, or for arsenic, have been tried and found useful. Preparations of iron and of zinc. From 5 to 10 grains of the sulphate of zinc have been given several times a day; or 3 grains of the oxyde of zinc every three hours. Sir G. Blane says that both in the West Indies and in London, intermittents have been cured by the use of this oxide, when they had previously resisted the bark. Sir James Mac Grigor speaks of it also in terms of praise; from what he saw of its effects in the agues of the Peninsula during the war.

Some of the remedies of this mysterious disorder operate upon the mind, or rather upon the nervous system through the mind. Hence it becomes probable that the *drugs* which have such power over the disease, act also on the nervous system through the body. And hence also we derive a confirmation of the opinion, that the disease itself is essentially a disease of the same nervous system. Ague has often been cured by the agency of strong mental emotion, such as sudden and great joy, anger, terror, or eager expectation. Thus we read that Quintus Fabius Maximus was cured of an old quartan on the day of a great battle. Strong impressions upon the imagination, producing feelings of disgust and horror, have had the same effect: such as those caused by drinking blood; swallowing a spider gently bruised, and wrapped up in a raisin, or spread upon bread and butter; keeping a spider suspended from the patient's neck in a nutshell, till it dies; and the like. The undoubted success, in many cases, of charms, must be referred to the principle of *faith*. The patient recovers, because he firmly believes in your power to cure him. Dr. Gregory used to relate the case of a patient in the clinical wards in Edinburgh, who, with sundry ceremonies, swallowed some word, written on a slip of paper: the result was, that he had not another pa-

roxysm. And I perfectly recollect having a great awe, when I was quite a child, of my maternal grandmother, because she was reputed to have the power of curing agues by means of some charm. I believe all that she did was to assure the poor people who came to be relieved from their ague, that they should have no more of it *after such a day*; and their implicit reliance upon this prophecy brought about its fulfilment. There seems to be this general principle observable in respect to agues, and to all other diseases which occur in paroxysms, viz. that after they have continued for some time, their farther continuance depends more upon the effect of *habit* than any thing else; and this habit may be broken by strong impressions made upon the nervous system; and the cure of one paroxysm is thus often the cure of the disease. We have seen examples of the existence of this morbid habit in hysteria, and in some cases of epilepsy. *Cæteris paribus*, that physician will be the most successful in these disorders, who is best able to acquire the confidence of his patient, and to gain a powerful influence over his mind.

There is no disease in which the prophylaxis is of more importance; but this you will have gathered from the facts which were stated in the two preceding lectures. The disposition to relapse is strongest soon after the disease has been removed; but it generally continues long, perhaps even for life. The late Dr. Macmichael caught an ague many years before his death, by sleeping on a rock somewhere in Greece; and he was ever after subject to occasional attacks of periodic headache, and other aguish symptoms, for which he was obliged to have recourse to bark or arsenic. Of course one essential point in the prophylaxis is the withdrawal of the patient from the influence of the exciting cause; taking him away from the malarious locality. But this cannot always be done; and when it cannot, we must impress upon him those cautions which arise out of the facts ascertained in regard to the operation of the malaria upon the human body. Persons who have been exposed to the exciting cause, or who have once had the fever, should, in whatever place they may happen to be, avoid over-fatigue and exhaustion of all kinds; sudden exposure to cold or heat; and the neglect of changing wet clothes, wet shoes and stockings for instance. In a malarious district persons should bear in mind the facts, that the miasmata are much more virulent in the night-time than in the day; and close to the surface of the earth, than in a higher part of the atmosphere. They should refrain, therefore, from going out late in the evening, or early in the morning; and they should rather select the attic than any other

floor for their bed-chamber. They who are obliged to go out in the morning in countries where agues are rife, should take care not to go out fasting; a good hot breakfast should be first taken, or at any rate some moderate stimulus; a crust of bread and a glass of wine, or a small quantity of ardent spirit, will fortify the system against the pestilential miasma. Measures of this kind have been found extremely beneficial in the navy: the giving, for instance, the men a warm breakfast before going out in the morning on malarious shores in boats, whatever the hour of starting might be. Generous diet, and a fair allowance of fermented liquor, are proper also for all persons in aguish countries. The late Dr. James Gregory used to mention in his lectures an anecdote in point, told him by his father. The elder Dr. Gregory studied at Leyden, under Boerhaave; and there were twenty-four other English students there at the same time: that is, they were called English, on account of their common language, but they were in fact composed of English, Irish, Scotch, West Indians, and Americans. The celebrated John Wilkes and Charles Townsend were among the number. These twenty-five students lived a good deal together: in truth they were cut, as the phrase is, by the Dutch, for some raffish behaviour on their parts. However, of the twenty-five, one only was a water-drinker. The other twenty-four drank each a bottle of claret daily: and the water-drinker, and he alone, fell ill of ague there.

Persons who have recently become residents in aguish districts, or who even happen to be travelling through them, would do well to take moderate doses of quina by way of safeguard. And in regulating the bowels, which, of course, is of much importance, warm stomachic laxatives should be made use of, rather than cold aperients, such as the neutral salts.

There is just one more expedient which I would suggest as not unlikely to afford complete protection to those who are of necessity exposed to the malaria; and it is, that they should wear an *orinasal respirator*. It is possible that as a breeze is filtered of the poison which was mingled with it, by passing through a dense mass of foliage, so, on a smaller scale, the air inspired in breathing may be strained and purified, and rendered harmless, in its transit through the sieve-like structure of Mr. Jeffreys' ingenious instrument. The principle of the suggestion is not new; but this mode of applying it has not, so far as I know, hitherto been tried. It is said that by surrounding the head with a gauze veil, or conopeum, the action of malaria is prevented; and that thus it is possible even to sleep in the most pernicious parts of Italy without hazard of

fever. Dr. Macculloch says that in Malta, and elsewhere, this belief is universal : and hence the popular practice of covering the mouth and nose with a handkerchief in the morning on going out, or in other suspicious circumstances : a practice (he observes) the efficacy of which is attested, as far as popular belief can attest any thing.

LECTURES
ON THE
FUNCTIONS OF THE NERVOUS
SYSTEM.

BY W. B. CARPENTER, M.D.

LECTURE VII. (*concluded.*)

ON examining the outlets by which the excretions are voided, we find that they are placed, like the entrances, under the guardianship of the spinal cord ; subject, however, to some control on the part of the will. In the lowest animals, the act of discharging excrementitious matter is probably as involuntary as those immediately concerned in the introduction of nutriment, and is performed as often as there is any thing to be got rid of. In the higher classes, however, such discharges are much less frequent ; and reservoirs are provided, in which the excrementitious matter may accumulate in the intervals. The associated movements required to empty these are completely involuntary in their character ; and are excited by the quantity, or stimulating quality, of the contents of the reservoir. But, had volition no control over them, great inconveniences would ensue ; hence sensation is excited by the same stimulus which produces the movements ; in order that, by arousing the will, the otherwise involuntary motions may be restrained and directed. There can be little doubt, from the experiments of Dr. M. Hall, as well as from other considerations, that the associated movements, by which the contents of the rectum and bladder are discharged, correspond much with those of respiration, being in their own nature involuntary, but capable of a certain degree of voluntary restraint and assistance ; whilst the discharge of the contents of the vesiculæ seminales would seem to be completely automatic : thus corresponding with the act of deglutition. On the other hand, the sphincters, which antagonize their expellent action, are also clearly maintained in a state of moderate contraction, so as to afford a constant check to the egress of the contents of the cavities ; and this condition has been fully proved by Dr. M. Hall, to result from their connexion with the spinal cord, ceasing completely when this is interrupted. On the other hand, the sphincter is certainly in part controlled by the will, and made to act in obedience to

the warning given by sensation ; and this voluntary power is frequently destroyed by injuries of the brain, whilst the spinal cord remains able to perform all its own functions, so that discharge of the urine and fæces occurs. In their moderate action, the expulsors and the sphincters may be regarded as balancing one another, so far as their reflex action is concerned,—the latter having rather the predominance, so as to restrain the operation of the former. But when the quantity or quality of the contents of the cavity gives an excessive stimulus to the former, their action predominates, unless the will is put in force to strengthen the resistance of the sphincter ; this we are frequently experiencing, sometimes to our great discomfort. On the other hand, if the stimulus is deficient, the will must aid the expulsors, in order to overcome that resistance which is due to the reflex contraction of the sphincters ; of this also we may convince ourselves, when a sense of duty, or a regard to prospective convenience, occasions us to evacuate the contents of the rectum or bladder without a natural call to do so.

The muscular coat of the bladder is commonly regarded as having, like that of the intestinal tube, no connexion with the spinal cord ; but the experiments of Valentin have shown that a connexion exists, as in the former case, through the sympathetic nerve, affecting not only the bladder but also the ureters. That physiologist states that a very distinct and powerful peristaltic action of the ureter, proceeding from the kidneys to the bladder, may be produced by irritating the abdominal ganglia of the sympathetic, or the roots of the superior abdominal spinal nerves ; and that strong contractions of the bladder are excited by irritation of the inferior portion of the abdominal sympathetic, but especially of its sacral portion, and of the roots of the middle and inferior abdominal nerves of the spine. In these, as in former cases, no effect is produced by irritation of the spinal nerves, unless the portion of the sympathetic connected with the particular organ be entire. Corresponding facts have been ascertained by him in regard to the genital organs. Contractions were excited in the vas deferens and vesiculæ seminales, especially of the guinea-pig at the time of heat, by irritation of the inferior lumbar and highest sacral portions of the sympathetic ; and the Fallopian tubes, as well as the uterus itself, may be excited to contraction by irritation of the same nerves as those which excite the rectum,—namely, the lower lumbar and first sacral nerves of the spine. This fact is of much importance in regard to the rationale of the operation of certain medicines.

It may be as well here to mention, in connexion with the foregoing experiments,

that similar results have been obtained by Valentin in regard to the heart ; motions of which may be excited by irritation of the roots of the spinal accessory nerve, and of the first four cervical nerves, and also of the first cervical ganglion of the sympathetic. He thinks that he has also witnessed distinct contractions of the thoracic aorta, of the inferior cava, and of the thoracic duct, upon irritation of the neighbouring portion of the sympathetic system, which evidently derives its whole motor power from the spinal cord. The ductus choledicus has also been seen by him to contract on irritation of the right splanchnic nerve.

In regard to the act of parturition, there would seem reason to believe, from the evidence of cases of paraplegia, that, of the muscles whose operation is associated in it, the diaphragm, abdominal muscles, &c. are called into action (as in defecation) through the spinal cord ; but that the contractions of the uterus itself are independent of all connexion with the nervous centres. Of the reason why the muscles, which were up to that time inert, should then combine in this extraordinary manner, and with such remarkable energy, physiology can afford no certain information. There can be little doubt, however, that the stimulus usually originates in the uterus, or in some of the neighbouring organs which are incommoded by the pressure ; but it may also result from some condition of the general system, in which the uterus itself is but little concerned. It is an interesting fact, which has been more than once observed, that the foetus may be expelled from the dying body of the mother, even after the respiratory movements have ceased. This would appear due to the contraction of the uterine fibres alone, which, like those of the heart and alimentary canal, retain their irritability longer than those of the muscles supplied by the cerebro-spinal nerves ; and the power of these would be unopposed by the resistance which they ordinarily have to encounter ; since the tone of all the muscles surrounding the outlet would be destroyed, by the cessation of the activity of the spinal system of nerves.

From the foregoing details it appears, that one of the chief functions of the spinal cord is to control the orifices of the various open cavities of the body ; and this function evidently has safety as well as convenience in view. It has been evidently designed by the All-wise Creator, that the glottis should close against agents injurious to the organs within ; and that the effort to vomit should be excited by the attempt to swallow substances so nauseous as to induce loathing. There is another protective influence exerted by it, of a still more remarkable nature. It has been ascertained by Dr. M. Hall that, if the functions of the brain be suspended or destroyed,

without injury to the spinal system of nerves, the orbicularis muscle will contract so as to occasion the closure of the eyelids upon the tarsal margin being touched with a feather. This fact is interesting in several points of view. In the first place, it is an interesting example of pure reflex action, occurring under circumstances in which volition cannot be imagined to guide it, and in which there is no valid reason to believe that sensation directs it. Further, it explains the almost irresistible nature of the tendency to winking, which is performed at short intervals by the contraction of the orbicularis muscle, and which is evidently a spinal action, capable of being in some degree restrained (like that of respiration) by the will, but only until such time as the stimulus (resulting perhaps from the collection of minute particles of dust upon the eyes, or from the dryness of its surface in consequence of evaporation) becomes too strong to be any longer resisted. Again, we have in sleep or in apoplexy an example of this purely spinal action, unbalanced by the influence of the will, which in the waking state antagonizes it by calling the levator palpebræ into action. As soon as the will ceases to act, the lids droop, and close over the eye in order to protect it ; and if those of a sleeping person be separated by the hand, they will be found presently to return. Here, as in studying the respiratory and other movements, we are led to perceive that it is the brain alone which is torpid during sleep, and whose functions are affected by this torpidity. As Dr. M. Hall very justly remarks, the spinal system never sleeps ; it is constantly in activity ; and it is thus that, in all periods and phases of life, the movements which are essential to its continued maintenance are kept up without sensible effort.

The closure of the pupil against a strong light is another movement of the same protective tendency. The channel, through which that just named is performed, is completed by the first branch of the fifth and the portio dura of the seventh. The contraction of the pupil is immediately caused by the third pair, or motor oculi ; as is easily shown by irritating the trunk of that nerve and observing the result. But it is not easy to speak with certainty as to the afferent nerve by which the motor influence is excited. Although the contraction of the pupil is usually in close accordance with the sensation occasioned by the impression of light upon the retina, yet there is no want of evidence to prove that the sensation of light is not always necessary ; for, even when the sight of both eyes has been entirely destroyed by amaurosis, the regular actions have been witnessed in the pupil, in accordance with varying degree of light impinging on the retina. This fact may be explained in two

ways. It may either be imagined, as some have done, that the requisite stimulus is not that of light conveyed through the optic nerve; but that of heat conveyed through the fifth pair. Or it may be still supposed that the motion results from an impression upon the retina, which impression being ordinarily conducted to the brain produces a sensation; whilst in these curious cases no sensation is produced, on account of a disordered state of the part of the brain in which the optic nerve terminates; whilst some filaments of that nerve, being connected with the spinal cord, and not with the brain, can produce a reflex action through the third pair, although no sensation accompany it. In either view, the rarity of the occurrence is at once accounted for; since in most cases of amaurosis, the disease lies in the trunk of the nerve, and thereby checks both its spinal and its cerebral actions. A protective influence, similar to that exhibited in the iris, is also exercised by the orbicularis when the eye is exposed to very strong light, such as the direct rays of the sun; and it is remarkably shown, also, in cases of ophthalmia, in which the retina is in an irritable condition. Every oculist is aware with what force the orbicularis contracts in the strumous ophthalmia of children, in which photophobia is generally a leading symptom; and its protecting action is further aided by the muscles of the eye, which roll it beneath the upper lid farther than any voluntary effort could accomplish. The particular muscles and nerves concerned in this action will be hereafter inquired into.

The physiologist has not at present any knowledge of any similar protective movements, in the human being, designed to keep the organ of hearing from injury; but there can be little doubt that those which we are constantly witnessing in other animals, possessing large external ears, are reflex actions excited by the irritation applied to them. In regard to the nose, we find a remarkably complex action—that of sneezing—adapted to drive off any cause of irritation. This action, as far as the respiratory movements are concerned, is nearly the same as that of coughing; but the velum palati, at the moment of the expiratory blast, is stretched across the fauces in such a manner, that the whole force of the air is directed through the nostrils, and tends to carry off any irritating solid, fluid, or gas, which may have excited the mucous membrane. It will hereafter be shown that the stimulus is conveyed, in this case, not through the olfactory nerve, but through the fifth pair, so that it is not dependent upon the excitement of the sensation of smell.

The influence of the nervous centres in maintaining what is commonly designated as the *tone* of the muscular system, was men-

tioned in a former lecture, as having been adverted to by Whytt; but to Dr. M. Hall is due the limitation of this influence to the spinal cord, and the system of nerves connected with it. By the expression in question is meant that state of moderate contraction, which causes all the muscles to present a certain degree of firmness, by their antagonism with each other, when none of them are particularly contracted or relaxed. The following experiments by Dr. M. Hall clearly prove the influence of the spinal cord on this functional condition:—"Two rabbits were taken; from one the head was removed; from the other also the head was removed, and the spinal marrow was cautiously destroyed with a sharp instrument: the limbs of the former retained a certain degree of firmness and elasticity; those of the second were perfectly lax." The limbs and tail of a decapitated turtle possessed a certain degree of firmness or tone, recoiled on being drawn from their position, and moved with energy on the application of a stimulus. On withdrawing the spinal marrow gently out of its canal, all these phenomena ceased. The limbs were no longer obedient to stimuli, and became perfectly flaccid, having lost all their resilience. The sphincter lost its circular form and contracted state, becoming lax, flaccid, and shapeless. The tail was flaccid and unmoved on the application of stimuli." It is probable that this tonic contraction is strictly a reflex action; an impression of the condition of the muscle, corresponding with the muscular sense of Sir C. Bell, but not necessarily accompanied by sensation, being conveyed to the spinal cord, and producing the stimulus to contraction. The want of this tone is seen in the relaxation of the sphincters; and also in the distortion of the face produced by paralysis of the portio dura, and resulting from the tonic contraction of the muscles on one side of the face, unbalanced by that of the other side. Cases have occasionally presented themselves, in which the portio dura has been paralysed to the influence of the will, (owing to disease affecting its cerebral termination), whilst its spinal connexions have not been affected; so that the tone of the muscles has been preserved, and no distortion of the face has manifested itself, until the muscles were stimulated by a voluntary impulse, to which those of one side only would respond.

Nearly allied to this function of the spinal cord is that by which it is subservient to the maintenance of the contractility of muscles paralysed to the influence of the will. It is well known that, in ordinary cases of paralysis, the muscles lose their irritability in the course of a few weeks, so that no stimulus excites them to contraction; and it is also well known that their characteristic structure is so greatly affected, that, in pro-

gress of time, no true muscular fibres can be detected in their place. Experiments on animals, in which portions have been removed from the nerves supplying the limbs, conduct to exactly the same result as the experiments made for us by diseased conditions in man. Now Dr. M. Hall has pointed out that,—in cases where the muscles are paralysed to the influence of the will, through disease of the brain or of the upper part of the spinal cord, but retain their power of reflex action, the nervous circle which operates through the spinal cord not being interrupted,—the contractility of the muscles is not diminished, but appears to be sometimes even increased. Hence he concludes that the spinal cord is the seat of muscular contractility. This conclusion will scarcely bear a strict examination. It is completely inconsistent with the fact that muscles will retain their irritability for some time after their nervous connexions with the spinal cord have been completely severed, and will even recover it (as Dr. J. Reid has shown) after it has been exhausted by repeated stimulation. Further, it may be equally well explained in another way. It is well known that muscular structure, like others whose chemical constitution is such as to require constant renewal, requires, for its perfect nutrition, to be kept in a state of functional activity. If the muscles of the leg, for example, be disused for a long time, their nutrition is greatly impaired, and their contractility is almost suspended, even though they retain their connexion with the nervous centres, and the latter be in their normal condition. It is to be expected, then, that if a muscle be completely put out of the pale of nervous influence, its nutrition should be speedily impaired, and its contractility altogether lost; but if the influence of the will only be withdrawn from it, and its connexion with the spinal cord be uninterrupted, it will be in a state of continual action, by the operation of various reflected stimuli; and this action will be sufficient to maintain its nutrition, and to prevent the loss of its contractility*. Whichever explanation be adopted, the fact is an important one, as affording a means of diagnosis in some obscure cases of paralysis. Thus in some cases of paraplegia the reflex actions may be excited; in others they cannot be. In the former the disease must be in the dorsal or cervical portion of the spinal cord, leaving its lumbar portion free to carry on the reflex actions, though its connexion with the brain

is interrupted. In the latter, the disease is probably within the lumbar vertebræ, involving that portion of the spinal cord through which the reflex actions of the lower extremities are produced. In like manner, in paralysis of a single arm or of one leg, if the reflex actions, and the contractility of the muscles on the application of a direct stimulus (such as galvanism), remain unimpaired, the cause is probably seated in the brain; whilst, if the tone of the muscles is completely lost, and no contraction can be induced in them, the cause of the paralysis is probably somewhere in the neighbourhood of the roots of the nerves of the part affected.

It would not be right to conclude this account of the principal functions of the spinal cord, without adverting to some of the leading pathological applications of the physiological doctrines which have been developed in it. These applications were first pointed out by Dr. M. Hall; and they are receiving continual and important extensions from his own labours and those of other practical inquirers*. It may be remarked, in the first place, that the power of the whole spinal system is capable of being morbidly diminished or augmented. It may even be for a time almost completely suspended, as in syncope, which state may be induced by sudden and violent impressions, either of a mental or physical nature, which operate upon the whole nervous system at once, commencing, however, in the brain. It is to be remarked, however, that in recovering from these, it is the spinal system of which the activity is first renewed,—the respiratory movements recommencing, and the power of swallowing being restored, before any voluntary actions can be performed. A corresponding state may be induced in particular portions of the system by concussion, as is seen in severe injuries of the spinal cord, which are almost invariably followed for a time by entire suspension of its functions. Again, the power of the whole spinal cord may be diminished by various causes, such as enfeebled circulation, pressure, &c.; and then we have torpidity of the whole muscular system. If oppression exists in the brain, the functions of the medulla oblongata will be especially affected; and, if it be prolonged and sufficiently severe, asphyxia will result from the interruption of

* The truth of this view has recently been established beyond all doubt by the very ingenious experiments of Dr. J. Reid, who has shown that if a muscle, the nervous connexions of which have been completely divided, be frequently stimulated by galvanism, its nutrition remains normal, and its contractility is retained. See *Edin. Monthly Journal of Medical Science*, May, 1841.

* The student is earnestly requested to make himself well acquainted with the pathological portion of Dr. M. Hall's recently published work on the Nervous System, in which a new field of inquiry is opened, and the extent and importance of the applications of Dr. Hall's physiological doctrines are clearly shown. It is only to be regretted that a volume devoted to scientific investigation should be disfigured by so many personal attacks, many of them completely groundless in their nature, as this contains.

the respiratory movements which it occasions.

On the other hand, the excitability of the whole cord, or of particular parts of it, may be morbidly increased. This is especially seen in tetanus, hydrophobia, and the artificial tetanus induced by strychnine; so that the slightest external stimulus is sufficient to induce reflex actions in their most terrific forms. It is interesting to remark, that, in these formidable diseases, the functions of the muscles controlling the various orifices are those most affected; and it is by the spasms affecting the organs of respiration or deglutition, that life is commonly terminated. Various remedial agents will probably be found to operate by occasioning increased excitability in some particular segments of the cord; so that the usual stimuli applied to the parts connected with these, will occasion increased muscular action. This seems to be the case, for example, in regard to the influence of aloes on the rectum and uterus,

cantharides on the neck of the bladder and adjoining parts, and secale cornutum on the uterus. The mode of influence of cantharides is illustrated by a curious case, related by Dr. M. Hall, of a young lady who lost the power of retention of urine in consequence of a fatty tumor in the spinal canal, which gradually severed the spinal cord, and induced paraplegia. The power of retaining the urine was always restored *for a time* by a dose of tincture of cantharides, which augmented the excitability of the segment of the cord, with which the sphincter vesicæ is connected.

The researches of Valentin, when grafted (so to speak) on the doctrines of Dr. M. Hall, afford the key to the explanation of the numberless sympathetic influences of the organs of nutrition, &c. upon one another; by showing that they are all connected with the spinal cord; and that the muscular structure, with which they are all provided, may be excited to contraction through it.

ON SOME POINTS IN THE PATHOLOGY OF THE CIRCULATION.

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*Being the substance of the Gulstonian
Lectures for 1841.*

(Continued from p. 611.)

It will be our next object to consider how far the phenomena of the disor-

dered states of the circulation can be referred to modifications of the known properties of the blood-vessels and their contents. Of these states the most important to be noticed are those of local hyperæmia, or too much blood in a part; three varieties of which have been distinguished by several English writers, but with less precision than is desirable in points so fundamental in pathology. The most elementary character by which we may define these varieties is their relation to motion, as expressed in the following table:—

Local hyperæmia, or too much blood in a part,	} with motion	(a.) Diminished—Congestion or passive hyperæmia.	} Active hyper- æmia.
		(b.) Increased—Determination.	
		(c.) Partially in- creased, parti-	
		ally diminished	
		Inflammation.	

Congestion: an increase of blood in a part with diminished motion.—The physical causes of this condition are very simple. To hold an increased quantity of blood the vessels must be dilated; and if they are dilated without a corresponding enlargement of the arteries leading to them, the blood in the dilated vessels must move more slowly. Congestion, then, may simply consist in a dilated state of the small vessels without further change; but in some instances other conditions, such as venous obstruction, and change in the blood, contribute to the diminished motion which is its essential character. Let us examine how the small vessels become dilated by the circumstances which are known to cause congestions, as we meet

with them in diseases, or can produce them artificially.

The dilatation, which constitutes congestion, may be caused by *obstruction* to the onward course of the blood: thus congestion of the brain may be occasioned by a tumor pressing on the jugulars, or even by a tight cravat: the lungs become congested, from obstructive or regurgitant disease of the mitral orifice: the liver becomes congested from the same lesion, especially when combined with dilatation of the right ventricle, which causes free regurgitation also through the tricuspid orifice. The distinguishing character of congestion from these causes is that the veins are also dilated.

The dilatation which constitutes con-

gestion, may be due to *atony or weakness* of the capillary vessels themselves, so that they yield to and are distended by the blood which they receive. This weakness may be a part of *general* vascular debility, as in adynamic fevers, or extreme prostration from any cause, as in that which accompanies a slow death. The distinctive character of this variety of congestion is, that it is *hypostatic*, produced under the influence of gravitation in parts that lie lowest.

But the atony or weakness of the capillaries may be *partial*, without corresponding weakness of the rest of the vascular system. Continued *distension* may impair the tone of the capillaries, and thus the congestion arising from gravitation, or venous obstruction, may become fixed in the capillaries, and continue after the removal of its original cause. Thus if the arm be hung down below the level of the body, or tied up, as for bleeding, for a long time, the purple hue, which denotes congestion, will not disappear for some time after the change of the posture, or the loosening of the ligature. So too the dimness of vision and confusion of thought, which have been produced by a stooping posture or violent bodily efforts, may not cease with their exciting causes. Thus, too, in persons dead of diseased heart, we often find the liver and other viscera much congested, without any unusual fulness of the venous trunks.

Congestions of internal organs are occasioned by the application of cold to the surface; and this intropulsive operation of cold, if it continue long, so distends the internal capillaries, that the restoration of warmth to the surface will not remove the congestion. Again, malarious influences act like cold; they cause great internal congestion during the cold stage, (how they do so is uncertain): and the long operation or frequent repetition of this influence causes permanent congestive enlargements of internal organs. In both these cases, the first cause of the congestion acts generally on the vascular system; but its subsequent cause is in the congested capillaries, which by long distension lose their tone.

Other causes of congestion are less simple in their operation. The continued application of *stimuli* to a part is sometimes followed, not by inflamma-

tion, but by congestion: this especially happens in the liver—a chiefly venous organ; but it occurs also in other parts. It might be supposed that the stimuli act simply by exhausting the irritability of the small vessels, and thus leaving them weakened and distended by their contents. I have often most distinctly seen, under the microscope, the minute vessels of a frog's web contract on the application of an irritant*, such as a grain of capsicum, and afterwards expand beyond their usual size; but these changes chiefly took place in the minute arteries, and caused, first, diminished quantity of blood with diminished motion, and afterwards increased flow with increased motion, neither of which is congestion, but the latter determination of blood. But on the application of a stronger irritant, such as a minute drop of essential oil (which is equally without chemical action), the previous arterial contraction was scarcely apparent, and the enlargement was speedy and most obvious; causing extreme rapidity of motion in all the vessels, capillaries, and veins, which were also distinctly enlarged. In a few minutes the size of the arteries began to diminish, and with it the motion in the vessels beyond them. Many of the capillaries still retained their enlarged dimensions, and in them the motion was most sluggish, and in some cases ceased altogether. The retardation of motion was obviously due in part to the enlargement of the vessel with diminished supply from the artery, but it was partly owing to the cohesion of blood-particles to each other and to the vessel; for when, after fresh irritation has increased the flow through the artery, the blood stagnant in the capillaries was again set in motion, it moved almost in mass, and left many of the round globules adhering to the sides of the vessel. It is plain then that congestion, as we define it, follows certain degrees of irritation of vascular structure; but this adhesion of the particles is an additional character, which is, I have found, not present in congestion from venous obstruction. In this respect we shall see that it approaches to inflammation, the acknowledged result of continued or severe irritation.

* In this and all similar experiments the irritants used were such as have no chemical action on the tissues or blood.

its cause as well as its effect; but we have no facts to enable us decide the nature of the connection: it is probably complex.

I have already noticed *cold* as a cause of congestion, by its intropulsive operation. But cold is also a direct cause: witness the red and purple noses which we get in very cold weather. The several effects of cold on the extreme circulation may be partly explained by its constricting operation on the vessels, varying according to the mode of its application.

The first effect of cold on the surface, especially in those of weak circulation, is paleness, from the shrinking of all the vessels. An extreme result of this kind is seen in the waxy pallidity of fingers after cold bathing, in those of languid circulation: the cold reaches and contracts the artery, stops the supply of blood, and consequently of colour, heat, and function.

But external cold commonly acts more partially, constricting first the more superficial vessels, the veins, whilst the arteries continue to pour in blood, at first florid, but by stagnation becoming dark-coloured. Hence the congestion from cold is at first red; but soon becomes purple. When warmth is restored to the part, the red congestion remains for some time, because the capillaries having been unduly suspended, even when braced by cold, have lost of their tone, and yield to the force of the circulation through the now dilated arteries.

The influence of cold and heat on the capillary circulation is not, however, exerted on the vessels only. The observations of M. Poiseuille on this subject show that these agents operate in another way. On applying small pieces of ice to the mesentery or web of a frog, or to the mesentery of a very young kitten, he perceived no change in the size of the capillaries; and yet the blood in them moved much more slowly, and at last became quite stagnant. On the other hand, the application of very warm water was found greatly to accelerate the passage of the globules in the capillaries—it is said without equally augmenting the current in the arteries and veins: so that their motions through the capillaries became as rapid as in the axis of the larger vessels. Under the influence of ice, he observed that vessels which

usually admitted three globules abreast, gave passage to only one in the centre; those at the sides being motionless. Hence he was led to ascribe the obstructing influence of cold to its increasing the thickness of the motionless layer of serum which lines the vessels. Heat, on the contrary, diminished this layer, so that the vessels, especially the finest, became more permeable.

So far is M. Poiseuille from ascribing the operation of these agents to vital changes, that he views it as altogether physical: and he cites the experiments of M. Girard, a mathematician, who in 1817 demonstrated by experiment, that hot water passed through fine tubes with greater velocity than cold water; and this independently of the expansion of the tubes. This he attributed to heat diminishing the attraction of cohesion between the solid and the fluid.

The physical quality of cohesive attraction must then likewise be concerned in modifying the passage of the blood through its vessels; and even more than in the case of water; because the liquor sanguinis is more viscid, and has more cohesive attraction: the motionless layer must therefore be thicker, and more affected by heat and cold.

On repeating the experiments of Poiseuille, on the frog's web, I have obtained somewhat different results. It was quite evident that the arteries at least did contract on the application of ice, and did enlarge on the application of warm water: the changes in the rate of motion were, therefore, in part to be ascribed to these causes. Under the influence of cold the blood moved more slowly in the capillaries, and many round particles remained stationary, or nearly so, in contact with the walls of the vessel. The same result ensued after pressing the web with the finger; numbers of the small rounder particles were seen adhering to the vessels, totally obstructing many of the smaller ones, and moving slowly along the sides of the larger vessels, until on a return of a more rapid flow through the arteries, the adherent globules were gradually all swept on by the fuller and more forcible current. On another occasion I may attempt to explain further the relations between the motionless layer and the moving blood; but enough has been said to make it a question whether M. Poiseuille has not

Defective secretion is frequently associated with congestion, apparently as overrated its effects. One of his statements is obviously erroneous: that under the influence of heat the velocity of the current in the capillaries is increased without any corresponding augmentation or change of current in the connected arteries or veins. A current cannot be increased in one part of a channel without a corresponding increase of velocity, or of size, in other parts of the same channel.

Determination of blood: an increase of blood in a part with increased motion.—Healthy examples of this variety of active hyperæmia are presented in the case of blushing, the formation of the stag's antlers (Hunter), and in the development of the gravid uterus. The increased vascularity and redness in these cases proves the increase of blood, and the enlarged size and increased pulsation of the vessels leading to the parts, indicate the augmentation of the motion of this blood.

In disease we meet with many instances of determination of blood. Determination to the head is one familiarly known, and it affords the opportunity of displaying one of the characteristics of determination, in the enlargement and throbbing of the carotid arteries. I had lately a patient who was subject to attacks of this kind, which caused him so much suffering and loss of moral control, that he attempted to destroy himself by cutting his throat. When an attack came on, the beating of the carotids was most manifest, and was promptly followed by flushing of the whole face and head, suffusion of the eyes, and sensations in the head, which he called distracting. In the slighter attacks, these symptoms would all pass away in a minute or two. Fits of epilepsy and convulsive hysteria are often immediately preceded by throbbing of the carotids, showing that determination of blood is their proximate cause. Drs. Darwin and Parry relate cases in which convulsive paroxysms were prevented by pressure on one of the carotids; and I have adopted this expedient with success in two instances. Many of the epileptic patients whom I have questioned, have stated that the fit is always preceded by palpitation, which is usually accompanied by determination to the head: but without

palpitation there may be determination to the head, and this I believe to be the common immediate cause of the sudden paroxysms of various kinds which affect the nervous centres. But the most common cases of determination of blood are those caused by the application of stimuli: thus heat causes the flow of blood to the surface; snuff to the nose and eyes; spices in the mouth, to the salivary glands; food in the stomach, to the vessels of its membranes; purgatives to those of the intestines; diuretics to the kidneys, &c. In fact the operation of most medicines depends on their somehow causing an increased flow to particular vessels; and there are few diseases unconnected with local determination of blood. But Parry and Broussais were both wrong in confounding this with inflammation, of which it wants the additional element, obstruction and alteration of the capillaries.

Now what is the physical cause of determination of blood to a part? The cause is not in the heart; for increased action of this would not operate locally; and, as we have just noticed, local determinations often take place without any perceptible change in the heart's action. Is it (to use the common phrase) increased action of the vessels? The only active property, which we know the vessels to possess, is that of contraction, the operation of which would be to diminish instead of to increase the flow. We know by observation, as well as by reasoning, that determination to a part is effected by *enlargement* of the arteries; and this enlargement is the effect of the arterial pressure (*vis à tergo*) acting on a tube that has lost some of its contractile power. If the arteries are enlarged, the capillaries and veins leading from them will also be enlarged, and will partake of the increase of blood and motion thus supplied to them. Such is what we see under the microscope after the application of a moderate irritant; and in the tense hard pulse of arteries leading to inflamed or irritated parts, we recognise the effect of the coats of the vessel being stretched to tightness, and untempered by the usual elastic spring.

If we inquire further what is the physiological cause of the dilatation of the arteries, which thus causes determination of blood, we do not find a

satisfactory answer in the facts or opinions adduced by writers on the subject. We know that arteries become dilated (relaxed) by moderate heat, and after being contracted under the influence of cold or of a stimulus: but we cannot see how the same thing takes place in blushing, or immediately on the application of stimuli. The terms active dilatation (Hunter), inflammatory turgescence (Kaltenbrunner), mean nothing that has any parallel in animal physics. Others speak of nervous influence, which is certainly concerned; but how, is a mystery. The effect is, that the fibres of the affected artery are paralysed or weakened; so that they then become distended by the *vis à tergo*. But what evidence have we of nerves paralysing organic irritable fibres? Strong moral emotions do sometimes cause relaxation of the sphincters, and weaken the heart's motions; and these influences are probably allied to the one in question; but the true nature of the connection, and the laws which govern it, are yet to be investigated. Dr. Billing ingeniously conceives the nervous influence to be exhausted, drawn away from the vessels by the stimulation of the nerves, and that thus their contractile power is impaired; but, besides other objectionable points, this view assumes that muscular irritability even in its lowest form, tonicity, is a property derived from the nerves—an assumption unwarranted by the facts and views most generally received by physiologists.

We can see something of the *final* cause of determination of blood. "*Ubi stimulus, ibi fluxus.*" The flow is intended to support the well-being and function of the part. If any influence disturb its well-being, or excite its function, more blood is called for: the capillaries are dilated to receive more; and the arteries and veins are enlarged to keep the increased blood duly in motion. The result is, in moderation, to increase the redness, warmth, sensibility, secretion, nutrition, and other functions of the part: in excess, to disorder and alter them.

There is an important effect of local determination of blood which is the more worthy of notice, because it is both intelligible and applicable to therapeutics. When an unusual quan-

tity of blood is determined to one part, there must be less in other parts; and what is of more importance, the force of the heart's action, which before was equally divided among all the branches of an artery, becomes in great measure engrossed by those which are dilated. The other branches are, therefore, left minus, and so are all other vessels in the body; and this is more felt as the general circulation is weak. In those of languid circulation, the determination accompanying digestion often causes chilliness of the surface. Such persons, too, get their heads hot and their feet cold on going into a close warm room; and by cooling their heads warmth is restored to the feet. Attacks of local determination of blood are often accompanied by shivering fits, coldness of the extremities, and defective secretions, all indicating too little supply to other parts. The important therapeutic agency of derivation or revulsion depends on the properties under consideration; and, as a remedial measure, it is most directly suited to remove local determinations of blood. The distinction between derivants and counter-irritants has not been sufficiently observed; but they are as different in their action and suitabilities as the two states, determination and inflammation, are separate from each other. Thus, in cases of determination of blood, more relief may generally be obtained from cupping (even dry), hot pediluvium, or warm baths, than from blisters, and such like.

Local determinations of blood, like local inflammations, are sometimes followed by excitement of the action of the heart. Is the heart thus excited by a direct influence propagated from the seat of determination? or by a reaction of the vital powers against the prostrating influence of local determination on the general circulation. The first is the commonly received opinion; but it involves some difficulties: for instance, how does an influence which dilates the arteries increase the contractions of the heart? And, supposing this influence to be nervous, why is it transmitted in some cases of local determination, and not in others? Probably both modes operate; one as direct irritation; the other as irritation of reaction.

The cases hitherto alluded to are

cases of determination to the capillary terminations of arteries. Another instance of irregular distribution of tonicity in the arteries, causing imperfect distribution of the circulating force, is presented in cases of aortic pulsation so often met with in nervous subjects, or in those in whom the vascular system is much disordered. In many cases of palpitation, even without valvular disease of the heart, we hear and feel the thoracic aorta and its larger branches thumping violently, whilst the pulse at the wrist is small and weak. Hence, too, coldness of hands and feet often accompanies these attacks. What becomes of the force of the heart thus lost to the extreme arteries? It is expended on the dilated aorta. As the violent beats subside, the pulse at the wrist regains strength, the arteries now more equally distributing the force. Thus we see that alterations in the arterial tubes may not only misdirect, but even neutralize this force.

Another variety of aortic pulsation, more remarkable, because more isolated and independent of palpitation, is that in the abdomen. The coeliac axis, and the portion above the bifurcation into the iliacs, are the situations in which I have observed this pulsation strongest: and sometimes the temporary enlargement of the artery at these spots is so great as to have been mistaken for aneurism. In five cases of strong pulsation, a little below and to the left of the epigastrium, there was hæmatemesis with or without melæna. In a case of very strong pulsation opposite the umbilicus, hæmaturia occurred, followed by dysuria and much lithic deposit in the urine. All these cases were soon cured, local depletion having decidedly relieved them.

In many instances aortic pulsation is associated with symptoms referrible rather to the nerves than the vessels; such as gastrodynia, euteralgia, hypochondriasis, and various symptoms of nervous dyspepsia. Both the natural and the morbid relations between the nerves and the arteries have yet to be made out by experiment; and the investigation is a very important one in a practical as well as a pathological point of view.

CONTRIBUTIONS

TO THE

CHEMICAL PATHOLOGY OF SOME FORMS OF MORBID DIGESTION.

BY GOLDING BIRD,

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[For the London Medical Gazette.]

[Continued from p. 577.]

Sulphocyanogen considered as the characteristic element of saliva.

THE earliest mention of the existence of a substance in saliva capable of striking a blood-red tint with the persalts of iron, occurs in the elaborate work of Treviranus*; he appeared to have no idea of its true nature, and thus refers to it in his fourth volume—"Ich habe in speichel zwey Bestandtheile gefunden, die ohne Zweifel ein wichtige Funktion haben; der eine ist milchsaures natrium; der andern nenne ich Blutsäure."—p. 330. "Der Hauptcharacter derselben ist, mit ein gesättigten Auflösung des Eisens in Salpetersäure oder Schwefelsäure, ein Verbindung einzugiehen welche ganz die Farbe des Blut hat."—p. 332.

To observe this property of saliva, all that is necessary is to add a drop of a solution of any per-salt of iron, as the tinctura ferri sesquichloridi of our Pharmacopœia, to about a fluid-drachm of saliva: the whole will become of a deep red hue, nearly approaching that of blood. This reaction has by Leopold Gmelin been attributed to sulphocyanic acid, and for reasons which cannot be regarded otherwise than satisfactory. It is true that the alkaline acetates possess the property of turning the salts of iron deep red, providing the solutions are tolerably concentrated; and as the alkaline lactates, salts regarded by some chemists as acetates, masked by their combination with albumen, really exist in saliva, it has been supposed that these combinations constituted the colorific element of saliva: and this among others appears to be the opinion of Schultz. To this view there is one fatal objection—that, when the aqueous solution of the alcoholic extract of saliva is distilled with phosphoric acid, the fluid collected in the receiver pos-

* Biologie oder Physiologie, von Gottfried Reinhold Treviranus, Göttingen, 1814.

sesses the property of turning red on the addition of a drop of the tincture of sesquichloride of iron. Now if the extract contained only the alkaline acetates, they would, under the influence of phosphoric acid, be decomposed, and acetic acid would distil over, and the product would not produce the effect under consideration, which must necessarily result if even a small quantity of sulphocyanic acid were present. This has been corroborated by Van Setten*, and many others.

The property of striking a deep red colour with the salts of iron does not belong to the albuminous ingredient of saliva or ptyalin; the aqueous solution of this matter turns to the faintest yellow on the addition of a ferruginous per-salt: by exposure to the air for 24 hours the ptyalin becomes alkaline, being apparently partly separated into insoluble albumen and soda, the latter causing the separation of sesqui-oxide of iron; and hence a brick-red precipitate appears in the mixture. This fact has been recorded by Schultz, but without giving the rationale of the phenomenon. It has been supposed that the deep hue of the colour produced by mixing saliva with the per-salts of iron could not depend upon sulphocyanogen, because it is destroyed by the addition of a drop of fuming hydrochloric acid: this objection falls to the ground upon submitting it to the test of experiment; for I find that this very effect is produced when sulphocyanogen is indisputably the colouring agent. To prove this I added *guttatim* a solution of sulphocyanide of potassium to a quantity of saliva of mercurial ptyalism, which is nearly without action on salts of iron, until it acquired the power of producing with salts of iron a tint of the same intensity as healthy saliva. I then added to this prepared saliva a drop of the tincture of sesquichloride of iron: a nearly blood-red colour was produced; and when to the mixture a few drops of fuming hydrochloric acid were added, the colour vanished, and a pale yellow tint alone remained.

Another proof of the presence of sulphocyanogen in the saliva is based on the formation of sulphuric acid on the addition of an oxydizing agent; for this purpose it is generally suggested to use chlorate of potass, but nitric acid I find to be amply sufficient.

To shew this, mix a very dilute solution of any alkaline sulphocyanide with chloride of barium, and filter the fluid, to free it from any accidental presence of a sulphate; then add a few drops of nitric acid, and heat the mixture: long before ebullition occurs the sulphur of the sulphocyanide will become oxidized, and sulphuric acid will be formed; and this, by combining with the barytes, will render the change obvious, by a copious precipitate of the sulphate of that base, insoluble in nitric acid*. If then, any specimen of saliva capable of reddening the per-salts of iron be acidulated with nitric acid, mixed with chloride of barium, and filtered, no change will occur until the mixture be warmed, when sulphuric acid will be formed at the expense of the sulphur of the sulphocyanogen, and a copious precipitate of sulphate of barytes will occur. From these facts I think it is perfectly justifiable to admit that saliva depends, for its power of reddening salts of iron, upon the presence of sulphocyanic acid—the Blutsäure of Treviranus.

From these observations we may venture to conclude that when a nearly colourless fluid discharged from the mouth is submitted to inspection, and found to possess the property of striking a deep red with the tincture of sesquichloride of iron, of giving evidence of sulphuric after being oxydized by nitric acid, of yielding a matter undistinguishable from albumen after a few bubbles of gaseous chlorine have passed through it, and of a specific gravity ranging from 1.0043 to 1.0091, or perhaps rarely a little higher, it may be regarded as consisting chiefly or entirely of saliva; although, in the present state of our knowledge, we have no right to conclude that one or other of these properties may not be absent, and yet the fluid may have been secreted by the salivary apparatus. I shall have occasion to give several illustrations of the necessity of this caution in determining the nature of fluids voided from the mouth, especially in the case of saliva of artificially excited ptyalism.

Peculiarities presented by saliva when secreted in excess, under the influence of disease or of medicines.

This part of our subject has been comparatively neglected, notwithstanding

* Müller, Archiv für Anatomie, &c. Jahrgang, 1838, p. 164, (Annual Report).

* It is possible that even this reaction may occur without the presence of sulphocyanogen, for, as albumen contains sulphur as one of its

ing its interest, and I cannot but regret that the amount of information my experience enables me to offer is but small. It is perfectly well known that under certain exciting causes the secretion of saliva becomes extraordinarily increased, constituting the state of *ptyalism* or *ptyarhœa*. This is almost always accompanied by an inflammatory state of the mucous membrane of the mouth, together with considerable injection and tumefaction of the gums, and fulness about the parotid and submaxillary glands. This affection, which may, as every one is aware, be produced by the administration of mercury, also occasionally occurs as an idiopathic disease, and has been particularly described by Dr. Addison, under the name of *catarrhal stomatitis*. It has often been mistaken for mercurial ptyalism, as even the fœtor generally considered as being characteristic of the latter is equally present in a greater or less degree in this disease.

The state of ptyalism, accompanied by an unconquerable tendency to gaping, occasionally occurs to us as an hysterical affection; and in a case of this kind, related below, I had the opportunity of carefully examining the saliva. An increased secretion of saliva, amounting to ptyalism, is frequently produced by medicines not generally regarded as sialagogues. Thus, I have in more than one instance known creosote produce this effect: iodide of potassium frequently does the same thing. Temporary ptyalism is also produced by longing after food, by the nausea antecedent to vomiting, and by washing the mouth with irritating fluids, of which I have seen a remarkable instance, where between two and three pints of saliva were collected in a few hours after washing the mouth with a tolerably pungent solution of chlorinated soda. These several cases of excessive secretion of saliva become important objects of study, for they enable us to ascertain whether the fluid, when secreted in large quantities, differs from that obtained under ordinary circumstances. This question becomes invested with a peculiar interest in examining the source of the secretion in pyrosis and analogous affec-

tions; for, among other ideas given to the profession, it has been suggested that these fluids consist wholly or chiefly of saliva. To this opinion, which has been very ingeniously advanced by Dr. Burne, although I cannot help thinking on very insufficient data, I shall have occasion to revert when alluding to the source of pyrotic fluids.

It is well known that, as a general rule, whenever a secretion is increased in quantity an increase of its watery element occurs, with a constant diminution in the proportion of its saline ingredients, and consequently of its specific gravity, providing no new element, itself the product of morbid actions, be added. Of this state we have an excellent illustration in the urine: this secretion in health may be stated to be of specific gravity 1.020: under the influence of hysteria, mental emotions, or of diuretics, the quantity voided in twenty-four hours may be increased ten-fold, and its density will fall in a corresponding degree. If a new ingredient be added, as sugar in diabetes, we have the exception to the law, and with an increased bulk of secretion we find a higher specific gravity. In the case of the salivary secretion some anomalies exist which at present do not appear to admit of any ready explanation. The further progress of animal chemistry will, in all probability, soon clear up this difficulty.

In the following table are presented the results of the re-agents on seven different specimens of saliva; of these, six were the products of different forms of ptyalism. The first column presents the results obtained by the addition of the re-agents to healthy saliva, and enables us to draw the necessary comparison between the peculiarities of healthy saliva and that of ptyalism. It will be necessary briefly to revert to the cases from which the different specimens were obtained, for the sake of future comparison with others.

CASE I.—*Saliva of hysteric ptyalism.*

Mary Ann S—, æt. 23, admitted under my care at the Finsbury Dispensary on December 4, 1840. A tall and handsome unmarried woman, of dark complexion, engaged for eight years as a bookfolder, working eleven hours per day in a room with seven others. She first menstruated at the age of 18, and from that time has never been completely well: this function is regular

ultimate ingredients, the oxidizement of this substance by the nitric acid might generate sulphuric acid, and thus introduce an important fallacy in the examination of fluids containing albumen. Another proof of the caution necessary in researches of this kind.

in its performance, lasting three days with considerable pain; and in the menstrual intervals, profuse leucorrhœa has existed. She has frequent pains during micturition. For the last two years she has complained of debility, irregular appetite, distressing flatulence generally coming on an hour after dinner, with severe spasmodic pain referred to the lower part of the abdomen, which, however, is not tender on pressure. During the last year she has daily, about an hour after dinner, been seized with fits of gaping, accompanied with a flow of perfectly limpid saliva from the mouth. The quantity collected at each paroxysm exceeds an ounce, and possesses the slightest possible tendency to a bluish tint, resembling that presented by an aqueous solution of sulphate of quinine. This girl's bowels are irregular in the performance of their functions, often acting not more than two or three times in a week. Any mental emotion will, however, produce diarrhœa. Frequent headache, especially after the hysteric fits, which, although not severe, are numerous about the menstrual periods. Pulse 100, very weak, and difficult to count from its smallness. Heart's action accompanied by a loud musical bruit, nearly resembling the vibration of the tongue of a Jew's harp.

The quantity of saliva brought to me was the result of three different paroxysms of gaping; it was neutral, and nearly four ounces in quantity. A piece of litmus paper placed on the tongue turned red. The chemical properties of the fluid are shewn in col. B of the table.

It may be interesting to glance at the treatment of this case: it was sufficiently simple. The bowels were freely relieved during a week by the administration of a combination of aloes and soap; the strength was supported by the use of sesqui-carbonate of ammonia in a bitter infusion, and exercise was enjoined. The dysmenorrhœa was relieved by the exhibition of belladonna with small doses of ipecacuanha, and the tendency to gaping and ptyalism ultimately removed by having electric sparks drawn on alternate days from the region of the salivary glands. For this purpose I transferred her to Guy's Hospital, and in about a fortnight she was discharged cured.

CASE II.—*Saliva from mercurial ptyalism.*

Mary Samway, æt. 43, under my

care at the Dispensary, for hæmoptysis connected with tubercular deposit in the lungs, in November 1840. After she had been a patient for a short time diarrhœa appeared, for which, in addition to some other remedies, I ordered a grain of calomel, with one of opium, to be taken night and morning. She commenced this medicine on Nov. 19th, and on the 22d, having only taken four grains of calomel, profuse salivation commenced: the quantity of fluid spat from the mouth exceeded a quart in the twenty-four hours. I could not learn that she had ever previously been affected by mercury. On Nov. 27th eight ounces of perfectly limpid saliva were brought to me; this quantity had been secreted between tea-time and bedtime. The only food she had taken in the day consisted of a little boiled custard, and a cup of tea. The saliva easily passed through a wet filter, and after this operation preserved a slightly opaline hue: a small quantity of mucous flocculi and debris of epithelium was left on the filter: it was quite free from air-bubbles, and sufficiently cohesive to be capable of being drawn into short threads. The chemical properties of this are shewn in column C of the table given below.

CASE III.—*Saliva from Mercurial Ptyalism.*

Mary Johnson, æt. 38, admitted at the dispensary under my care, Dec. 7th, 1840, for acute pleuritis. After depletion she was ordered the Pil. Ant. Opiat. Fort. of Guy's Pharmacopœia, with two grains of calomel every six hours. In three days ptyalism commenced; and on the 11th it was profuse. A specimen of the saliva was then collected and submitted to examination. The results are shewn in column D of the table. This specimen resembled the last in its physical characters, and was important only in being the result of the action of a much larger quantity of mercury, about thirty grains of calomel having been taken before the fluid was collected, whilst in the last case but four grains had been administered.

CASE IV.—*Saliva from Creosote Ptyalism.*

This specimen, which equalled about five ounces by measure, was collected from a woman about forty-five years of age, who had been taking the creosote in doses of $\mathfrak{m}\mathfrak{j}$. thrice a day for some

days. The ptyalism was profuse, and much resembled that produced by mercury: it lasted more than a week. The patient denied ever having taken any mercurial preparation during her illness. The saliva possessed a strong odour of creosote. Its behaviour towards re-agents is shewn in column E.

CASE V.—*Saliva from Temporary Ptyalism excited by Chlorine.*

This was collected from a patient at the Dispensary who had been using a wash for the mouth, consisting of a dilute solution of chlorinated soda. The woman had been for some time labouring under derangement of the digestive organs, attended with a most disagreeable taste in the mouth, for the relief of which the wash was ordered. Whenever she used the chlorine, even if she took the precaution to wash the mouth out immediately afterwards, a peculiar pricking sensation was experienced in the parotids, and a copious

flow of limpid saliva occurred, and continued for about a quarter of an hour. The action of re-agents on this secretion is shewn in column F in the table.

CASE VI.—*Saliva from Ptyalism following the administration of Iodine.*

This was collected from a patient in Westminster Hospital, under the care of Dr. Bright, labouring under sore-throat, apparently connected with a previous syphilitic taint, for which no mercury had been taken. Iodide of potassium, in doses of five grains thrice a day, was prescribed. After taking this for some days, active ptyalism appeared, with tenderness about the salivary glands. This excessive flow of saliva was kept up by the swollen state of the posterior fauces, which rendered the act of swallowing the secretion almost impracticable. This specimen, which was loaded with iodine, behaved to re-agents in the manner shewn in column G.

Action of Re-agents on Saliva of Ptyalism contrasted with the Healthy Secretion.

Re-agent.	Healthy saliva.	Hysteric ptyalism.	Mercurial ptyalism.	Mercurial ptyalism.	Ptyalism from creosote.	Ptyalism from chlorine.	Ptyalism from iodine.
Ebullition	...	Opalescence	Opalescence	Milkiness	Opalescence	Milkiness	Opalescence
Chlorine	Copious deposit of albumen	Copious deposit of albumen	Copious deposit	Copious deposit	Copious deposit	Copious deposit	Dense deposit, and iodine evolved
Nitric acid	...	Slight troubling	Opalescence	Opacity	Slight troubling	Milkiness	Iodine evolved
Sol. potass	Evolves ammonia
Sol. alum	Troubling	Opalescence	...	Opacity	Opalescence	Opacity	...
Proto-chloride of tin	Dense precipitate	Copious precipitate	Precipitate	Precipitate	Copious precipitate	Copious precipitate	Copious precipitate
Sesqui-chloride of iron	Blood-red colour	Deep orange red colour	Very pale orange	Slight yellow	Deep orange	Scarcely yellow	Palest orange
Bi-chloride of mercury	Opacity	Opalescence	Slight opacity	Copious troubling	Opalescence	Slightest troubling	Opacity
Nitrate of lead	Copious precipitate	Copious precipitate	Precipitate	Ditto	Copious precipitate	Copious precipitate	Dense precipitate
Nitrate of silver	Dense precipitate	Ditto	Copious precipitate	Copious precipitate	Ditto	Ditto	Ditto
Sulphate of copper	Bluish precipitate	Bluish troubling	Greenish troubling	Copious troubling	Slight troubling
Tincture of galls	Troubling	Copious troubling	Faint troubling	Ditto	Troubling	Dense troubling	Troubling
Litmus paper	Faintly alkaline	...	Faintly alkaline	Faintly acid	...
Specific gravity	1.0091	1.0080	1.0043	1.00062	1.0084	1.0062	1.0073
Column	A	B	C	D	E	F	G

The specimen of healthy saliva, whose behaviour towards re-agents is shewn in column A, was procured about an hour after breakfast; and the secretion was excited by moving the lower jaw as in the act of mastication. All these seven specimens had certain physical characters in common. They were all colourless, with the exception of the slightest tendency towards a bluish or yellow tint. They all contained in suspension more or less ropy mucus, which, by repose, soon subsided, and formed a creamy deposit, from which the supernatant fluid could readily be decanted. In every case the opalescence produced by exposure before submitting the specimens to examination was removable by a drop of a solution of potass, in which menstruum the copious precipitate produced by chlorine was readily soluble—a fact which strongly favours the notion of its albuminous character, as mucous matter, or at least what is conventionally regarded as such, is not soluble in a dilute solution of potass. Every specimen passed through a previously wetted paper filter with tolerable facility. The consistence of the filtered fluid was never such as to give a ropy character, but still was enough to allow it, in most instances, to be drawn into short threads on pouring it from one vessel into another—a character denominated by the French *filante*.

Regarding the action of the individual re-agents employed, I may be permitted to remark that I made this selection from those used by Leopold Gmelin in his elaborate researches, so as to permit of comparison between the results obtained.

Ebullition, in every instance, excepting in the case of healthy saliva, produced an opalescence, which was removed by the addition of a solution of potass. This deposit by heat, which I may remark is also occasionally noticed in healthy saliva, I feel inclined to regard as albuminous in its nature; and, from analogies presented by other secretions, we should not feel surprised at this element of the blood escaping the transforming influence of the cæcal secreting tubes of the salivary glands, under circumstances of irritation, in the same manner that this element appears in the urine in the congested state of kidney consequent on scarlatina. The effect of chlorine I

have before alluded to. This agent, when added *per se*, or generated in the fluid by an electric current, I believe acts by developing albumen previously existing in a combined state in the secretion. The effect of nitric acid I am not quite prepared to explain. We might, with Mitscherlich, indeed, assume that the acid precipitates mucus which existed combined with soda; but the chemical characters of the deposit at once negative this hypothesis: and, on the other hand, we are not justified in considering the deposit produced by nitric acid to resemble that thrown down by chlorine, because it does not occur in every case in which the latter produces a copious precipitate. The action of potass is remarkable. In no specimen did it produce a deposit in the cold. On heating the mixture it turned yellow in every instance; and in the specimen of saliva collected from the patient who had taken thirty grains of calomel (column D), and where a very irritable state of the salivary apparatus existed, a copious deposit appeared at the boiling temperature, accompanied by an evolution of ammonia. In the other specimens there was no trustworthy evidence of the existence of this alkali in a free or combined state. The rationale of the action of the sesquichloride of iron has been already explained; and in the results observed on the addition of this re-agent to the different specimens of saliva, we find some interesting ground for speculation, assuming, for reasons already explained, that the blood, or orange-red hue, is produced by sulphocyanogen. Thus, in the cases here taken as examples, we find the red colour developed in considerable intensity in the saliva produced in hysteric ptyalism (column B), where no medicinal sialagogue had been used, and in the specimen resulting from the ptyalism produced by the creosote (column F); whilst in the cases of long-continued ptyalism (C, D, G,) we begin to lose our evidence of the presence of the sulphocyanogen. How, then, does it occur that a ptyalism of more than a year, which existed in the case of the hysteric patient, did not effect the removal of the ingredient, whatever that may be, which coloured the salts of iron red, when, after a similar action lasting only a few days, we lose nearly all traces of it in the secre-

tion submitted to examination? This question is difficult to solve. It may, however, be suggested, as possible, that when the salivary secretion is increased by artificial means, the law applying to other secretions obtains, viz. that the amount of water is increased, and the solid elements are diminished; whilst, when an increased flow is produced by some action originally excited in the body itself, the proportion of solid to the fluid constituents remains nearly unaffected. This view appears to be countenanced by the circumstance that nearly in the ratio of the diminution of the specific gravity we lose evidence of the presence of sulphocyanogen: the exception to this observed in the specimen of saliva obtained from the patient under the influence of iodine (col. C.) is more apparent than real, as the considerable proportion of iodide of potassium present in the secretion would tend to raise its specific gravity. Still a much more extended examination of the subject is necessary before the accuracy of this view can be regarded as beyond impeachment; and in suggesting this explanation I offer it with extreme diffidence, as the more elaborate investigation of future observers will, I have no doubt, replace it with a more satisfactory one. The action of the other re-agents is only interesting as affording means of comparison between the properties of saliva and of true gastric secretions; and to these we shall have occasion to recur when examining the chemical properties of the latter class of fluids.

[To be continued.]

OBSERVATIONS

ON THE

USE OF THE VECTIS, OR SINGLE-BLADE EXTRACTOR, IN DIFFICULT LABOURS.

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(For the Medical Gazette.)

I PROPOSE in this communication to consider the claims of the vectis as a safe and efficient mechanical power, in accomplishing difficult labours in a manner similar to nature, by supplying and increasing uterine contraction.

As the following observations are in-

tended to be altogether practical, I abstain from entering upon the early history and introduction of the vectis into midwifery practice, referring my readers to Dr. Bland's learned historical memoir of that instrument*, published in the second volume of the London Medical Communications, in 1790.

Dr. Bland, in that paper, while stating the comparative utility of the vectis and forceps, gives a striking instance of the successful application of the former, related to him by Dr. Garthshore, by which "Dr. Bromfield, whose prejudices against the vectis were as strong as even those of Dr. Osborne, was compelled to acknowledge he had been mistaken in his opinion." Strongly impressed with the superior utility of the vectis, Dr. Bland used it on the principle of a lever of the first kind. Still he makes mention of Mr. Dease's "Observations" published in 1783, to whom the profession is indebted for its appropriate appellation and use as an *extractor*: indeed, until the time of Mr. Dease, the vectis was made with so slight a curve in the blade as to render its abuse as a lever inevitable. Such an objection cannot be urged against Dr. Lowder's vectis, which by its construction is admirably adapted to aid as an extractor in lingering and impeded labours, as its introduction and application on the principle proposed by Mr. Dease, was a decided improvement in the state of instrumental delivery practised at that time.

Dr. Denman likewise expressed himself in terms so commendatory of the vectis, that Dr. Osborne, who lectured with him, not choosing to be considered as having adopted his opinion, and fearing "it would tend to establish the preference of the vectis," takes exception to the following paragraph in Dr. Denman's comparison of the vectis with the forceps. "That the vectis, prudently used, is, in every case, an equally safe and efficacious instrument with the forceps, and a better adapted instrument in many cases, which occur in practice."

In 1794, Dr. James Hamilton published an admirable essay on the use

* Observations on the Obstetric Extractor, by Dr. Breen. A valuable paper published in the Dublin Journal of Medical and Chemical Science, July 1, 1835. See also, Principles and Practice of Obstetric Medicine and Surgery, by F. H. Ramsbotham, M.D.

of Lowder's vectis, in which he has stated in a clear, correct, and convincing argument, its utility as a mechanical expedient in the second class of laborious labours.

I had not seen Dr. J. Hamilton's essay till after I had written the greater part of these remarks; and as I found we agreed in our views, I was not surprised at finding also a corresponding agreement in much of our language. Dr. J. Hamilton's opinion of the vectis, however, changed in the course of a few years; for we read in his "Practical Observations," the reasons which for many years have induced him to discountenance the use of that instrument, with some exceptions; "having found," as he states, "that by limiting the first stage of labour to twelve hours' duration, laborious labours very rarely occur in his own practice," and that when they do, he gives a preference to the forceps.

To those accoucheurs who do not approve of Dr. J. Hamilton's active interference in the first stage of labour, his reason for relinquishing the vectis does not apply; but admitting, that by venesection and the opiate enema*, by support and counter pressure, and by sliding up a portion of the cervix uteri compressed between the infant's head and the bones of the pelvis, the accoucheur renders the complete dilatation of the os uteri less tedious, and thereby the termination of the labour by the natural powers more frequent—admitting, according to many authorities, that the absolute necessity of applying either the forceps or vectis is very rare—nevertheless I am satisfied that the vectis can be used as an extractor in many cases with a certainty of diminishing the duration and severity of the labour, and therefore with advantage to the mother, and with a better chance of saving the life of her infant. Moreover, I am not disposed to concur in the exclusive preference given to the forceps by Dr. J. Hamilton, in those laborious labours which he describes as rarely occurring in his practice, any more than in the cases wherein accoucheurs commonly apply the forceps, because from my own experience I have found the vectis all-sufficient. Although the authority of Dr. J. Hamilton for the use of the

vectis be withdrawn in accordance with his own practical views, his argument in its favour holds good, and will have its due weight with the profession. To such high authorities as Drs. Denman and Hamilton, I may add those of Lowder, Haighton, Blundell, Breen, Gaitskill*, and the writer in the *Medico-Chirurgical Review*, July 1, 1821-22. Passing over the many eminent men who advocating the superiority of the vectis, used it altogether as a *lever*,—Dr. Blundell, in his lectures, says, "the lever is of more importance to the general practitioner than either the long or short forceps, and I more particularly recommend it to you."

I would here take leave to observe that Dr. Collins' doctrine of† "waiting till the patient's strength be much exhausted," or, "so long as the head advances ever so slowly," is not in my opinion the right practical rule for the guidance of the accoucheur in the use of either forceps or vectis. Such a doctrine is very like instituting an experimental observation as to the degree and duration of suffering a parturient woman can endure, with a bare chance of escaping injury both to herself and infant. Dr. Collins' imperfect estimate of the real value of the forceps is only, I conceive, to be explained by the faulty dimensions of the instruments he describes. Hamilton, Burns, Merriman, Naegele, Oslander, Siebold, advocate juster views of the advantages of the forceps. In my own practice, adopting in general Dr. James Hamilton's management of the first stage of labour, I use the vectis not only in those rare cases in which he applies the forceps, but also in various cases in which, if not absolutely required, it may yet be advantageously employed.

Much has been written on the comparative merits of the vectis and forceps, but until the time of Dease and Lowder, the main objection against the abuse of the former had not been completely met, nor the claims of the vectis or *single-blade extractor*, and of the forceps or *double-blade extractor*, by teachers and practitioners, fairly estimated. Since that time, the vectis has been used by experienced and success-

* See London Med. Repository, vol. 20, 1823.

† Consult the paragraph. Dr James Hamilton's Comments and Dr. Collins' rejoinder are before the profession.

* Of the opiate enema I am unable to speak from my own experience.

ful accoucheurs, not only in cases wherein the duration and difficulty of the labour justify its application by every right precept and rule of practice, but in those cases also in which the most procrastinating practitioner has a reluctant recourse to the forceps.

It is, indeed, singular, but not less true, that what ought to have been deemed a ground of preference of the vectis, namely, the greater facility of its application, has by the almost unanimous voice of lecturers been pronounced as a conclusive objection to its general introduction into practice. Such, indeed, might have been held to be a good reason against its *abuse* as a lever in the hands of coarse, rude, and bold pretenders, but in these days of advanced and diffused knowledge, and of higher qualifications required by licensing bodies, such an argument can fairly have no weight. But if there be those who still entertain fears of the recurrence of such grievous cases as have been related by Dr. Merriman and Mr. Gaitskill, I would suggest a coroner's inquest as a summary remedy for such instrumental "malapraxis."

On no ground can a meddlesome midwifery be defended, which uselessly interferes with, or injuriously interrupts natural labour. On the other hand, humanity calls for, and reason and experience support that line of practical duty, by which anxiety of mind and bodily suffering are abridged without the least risk to the parturient woman.

The advocates of the vectis have been twitted for using an instrument which has not the power of diminishing the child's head, nor indeed any great mechanical power at all, when simply applied on the principle of an extractor. I shall endeavour to show that this opinion, which I believe to have led teachers to discountenance the use of the vectis, is both erroneous and absurd.

It seems to me, that teachers, by a plausible exposition of the application of the forceps, however ingeniously correct, as applied to the dried foetal cranium upon the lecturer's table, yet of utter practical fallacy, when used to help an infant's head through the bony canal of the pelvis, have unfairly prejudiced the minds of rising practitioners in favour of that instrument.

When the lecturer exhibits to his class the dried cranium, and explains the advantage of one forceps over another by reason of its fenestræ admitting the parietal eminences, or by the inclination of one blade permitting a more exact adaptation of surfaces; when he insists, that the forceps, not projecting beyond the most salient points of the head, does not therefore encroach upon the pelvic cavity, he is surely inculcating a great practical absurdity, which observation and reflection should correct. According to my view, the vectis and forceps occupy space in the pelvis just in proportion to their bulk: and consequently the *forceps, with two blades*, adds to the bulk which must pass through the bony canal twice as much as the *vectis, with a single blade*.

By an admirable provision of nature the foetal head is so made up of component portions freely moving upon each other, that it is capable of assuming almost any form required to traverse the unyielding bony channell through which it is forced by the "vis a tergo" of the contracting and propelling uterus. What accoucheur has not felt the semi-membranous, semi-osseous, multipartite head squeezed through the irregular form of an abnormal outlet, moulded accurately to it, and presenting no resemblance to the symmetrical conformation of the dried cranium upon the lecturer's table?

To suppose that in a narrow and deformed pelvis the head preserves its natural form under the gripe and pressure of the forceps, as when its application was exhibited to us in the class, is clearly an untenable proposition, which our experience soon disproves.

Taking for example a case wherein the natural powers fail to expel the infant: the question, to be solved, is, what means can most easily bring the compressible head through the pelvis, to whose form it can be moulded? I answer, such a mechanical power as, occupying the least bulk, will accomplish its delivery. Now, as the single blade extractor or vectis occupies but half the space of the forceps or double-blade extractor, the vectis is certainly that mechanical expedient. The forceps, being an instrument of still great power, may have in certain cases *pro tanto* a superiority, but as the vectis is commonly all-sufficient,

almost superseding a necessity for the forceps, the vectis has assuredly the first claim upon the accoucheur. It has besides many advantages; it is easy of application, makes only a harmless interrupted pressure on the child's head, and does not act at all upon the structures of the mother. With these combined advantages, "it affords," to use Dr. Hamilton's words, "exactly the assistance in the second order of laborious labours which is required. For as the size of the child's head is in natural cases diminished as far as is necessary, by the contractions of the uterus forcing it forward through the bones of the pelvis, an increase of the vis à tergo will of course increase that diminution, if the shape of the passage require it. While Lowder's vectis, therefore, possesses the power of compressing the cranium in common with the forceps, it has a decided superiority over them in this, that it accomplishes that end by similar means with nature."

In commencing midwifery practice I accustomed myself to the use both of the forceps and vectis, but after a time became so strongly impressed with the applicability, safety, and efficiency of Lowder's vectis used as an *extractor*, and more especially satisfied with the rationale of its action, either supplying the place of the propelling powers or increasing their efficacy, that I have in a great measure relinquished the forceps. I do not, however, mean to advocate an exclusive use of either instrument.

To Lowder's vectis I give the preference, and likewise find it more convenient for the pocket*. I have often used the vectis without the knowledge of the patient or her attendants: sometimes (which is perhaps the better way) I have explained its use to the patient, who in a subsequent lingering labour has urged me to have recourse to it. The vectis is to be applied in the following manner:—

The woman lying on her left side at the edge of the bed, with the knees drawn up and the feet resting against the bed-post or back of an attendant—a round towel, in cases of difficulty, being fixed to the same bed-post, for a purchase during each labour throe—the

lower bowel and bladder being ascertained to be empty, the os uteri dilated, and the Liq. Amnii discharged, the instrument being warmed by putting it up the coat sleeve with the blade resting on the palm of the hand, and greased, if required by deficient vaginal moisture, two forefingers of the left hand are to be passed along the vagina towards the sacral promontory, or to the posterior lip of the uterus, if it can be felt, and the vectis guided by them is to be carried within the os uteri, being kept closely applied to the head by the curve of its blade: its direction is then to be changed by a sliding side-movement to the left or right sacro-iliac synchondrosis; it is next to be carried upwards, and by a combined sliding and slight semirotatory motion brought round to the hollow of the ilium, or ramus pubis, and fixed over the occiput; or the vectis may be made to describe a semicircular movement, the fingers of the left or right hand serving as a fulcrum, on which it turns by the help of the other hand. When it is felt that the instrument has a firm purchase, an extractive effort is to be made during a pain in the axis of the inlet, the practitioner resting when the pain goes off, and thus alternately acting and resting in imitation of natural pains. If the head is detained so high up that by passing the hand within the vagina its position cannot be ascertained, the vectis is to be directed to the hollow of the left ilium, as the first position is by far the most frequent. According to a statement in the "Journal de Médecine," given by Dr. Merri-man, the first position occurs in five or six cases where the second is once found: according to Baudelocque the first and second positions occur in the proportion of 7 or 8 to 1. Naegele, however, in his "Mechanism of Parturition," states the *third** position to be next in frequency after the first; the *third* occurring in proportion to the first as 1 to 2½.

If, for example, the head is detained at the inlet, by reason of the head and inlet not having a correlative proportion, if the head does not enter in its right axis, or, if only one-third part of it has passed the brim, that movement of flexion upon the chest, which traction upon the occiput operates, will

* This observation exposes me, I am aware, to the risk of being numbered amongst the proscribed who go to no labours without instruments in their pockets.

* The *third* position of the German schools corresponds with the *fourth* of Baudelocque.

give the direction in which Capuron describes the head entering the pelvis, namely, with the chin approximated to the chest; the vertex or smallest part of the head being in the direction of the largest diameter of the pelvis. I have known my father to bring down the head by the vectis, when it could only be touched by the hand passed *per vaginam*. In this case, the practitioner had been foiled in the application of the long forceps, and as the liq. amnii was discharged, turning could not without difficulty have been accomplished.

When the head has entered the cavity of the pelvis, and the accoucheur has ascertained its position by the anterior fontanelle sutures, or the ear, the vectis, passed as before along the sacrum, is to be carried round either below to the left or above to the right ramus pubis, according as the face is turned to the left or right side, and applied over the ear, face, and chin. We commonly find the head engaged transversely in the cavity, and have only to determine to which side the face looks.

If the head occupy the fourth or fifth positions of Baudelocque, with the anterior fontanelle to the left or right acetabulum, we shall expect to find that the uterine contractions carry the face* into the hollow of the ilium or to the sacro-iliac synchondrosis, and the occiput in a corresponding opposite direction. If the natural pains do not effect this movement, we may succeed in bringing with the vectis the occiput to the acetabulum where it is found in the first and second positions: should we fail to accomplish this, the head having descended too low down, the chin may be brought under the arch of the pubes. After allowing due time for the expansion of the perineum and moulding of the head, if the uterine action become feeble and inadequate to the delivery, we are to apply the vectis over the occiput laterally as a single blade of the forceps, or posteriorly along the sacrum, and directing the head towards the pubis to deliver by a gradual extractive force, while with the left hand we support and carry forward the perineum

and nates. By the vectis we can rectify the transverse malposition of the head, described by Dr. Montgomery, in which the labour throes follow quickly and powerfully without in the least degree advancing the head. After we have rectified a malposition of the head, or a forehead or face presentation, or brought the arrested head through a narrow inlet, we should withdraw the instrument, and leave the case to be terminated by the natural efforts.

The accoucheur will find that, in numerous cases, he can terminate by the vectis, with advantage to his patient, a lingering or laborious and exhausting labour, in which one practitioner would have recourse to ergot, another to laudanum, and a third, as Gooch quaintly expresses it, to *tincture of time*.

Two rules are to be constantly observed in the use of the vectis:—Firstly, to co-operate with each contraction of the uterus, resting always in the intervals of the pains; secondly, to apply our extracting power in the axis of the pelvis, according as the head occupies its inlet cavity or outlet.

In the fourth clinical report of difficult cases in Midwifery, by Dr. R. Lee*, he observes that “had a faithful report been given of all the cases of artificial delivery contained in this table” (exhibiting a comparative view of instrumental delivery by the forceps, and craniotomy, in the several British and Foreign Hospitals), “it is impossible that so great a discordance of opinions could so long have existed respecting the employment of instruments in the practice of midwifery.”

With a view to aid in rightly appreciating the advantages, difficulties, and ruinous consequences resulting from the use of instruments in obstetric practice, I proceed to relate the following twenty-two cases, for the accuracy and fidelity of which I can vouch, wherein I employed the vectis. They present to the accoucheur practical data, which may be advantageously contrasted with the forty forceps cases recorded in the valuable series of clinical reports above mentioned. I am strongly inclined to the opinion, that a preference is due to the vectis in such cases; and that prudently used (and I entertain no fears of its abuse, certainly none which can

* We are indebted to Solayres, who followed Ould and Smellie in investigating the mechanism of labour, for this observation, which has been confirmed by Naegele and Baudelocque, Jun.

* Vide LOND. MED. GAZ. New Series, vol. 2, session 1838—39.

be compared with the grievous injuries inflicted in numberless instances by the forceps), it will be found worthy of more general employment by the accoucheur, and of the sanction and recommendation of midwifery teachers.

The nine cases in which I applied the forceps, present, it is true, no circumstances demonstrative of the comparative difficulty and danger in the employment of that instrument; still, in not a single case should I have failed, I believe, to accomplish the delivery with the vectis, and the favourable results are doubtless to be attributed to an earlier employment of the forceps than in the instructive cases recorded in Dr. Lee's report.

[To be continued.]

SOME
PHYSIOLOGICAL REFLECTIONS
ON
THE NATURE AND TREATMENT OF
ANGINA PECTORIS,
AND OF ANALOGOUS STATES.

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[For the Medical Gazette.]

[Continued from p. 532.]

Dyspnœa—Slowly acquired derangements of respiration—Dyspnœa a muscular sense—Besoin de respirer—Suffocation—Causes of dyspnœa—The base of the lungs in reserve—Congestions—The effects of posture, repletion, exertion, cold, spasm, complicated influences.

HAVING concluded our endeavours to analyse the main essentials of angina, or rather having shown the mode in which we conceive the disorder ought to be studied, we proceed to treat in the same way the subjects of dyspnœa and palpitation, which are often found to complicate angina, and which we suppose to be in need of some explanations, both for their own sakes, and for that of contrast.

Previous to the consideration of the respiratory distresses, the inquirer should reflect on the physiological and the merely senile changes or deteriorations, as well as more morbid states of each part of the respiratory organs, which may gradually come on prior to severer manifestations; and there will

be still more reason to attach importance to the progress of the same, and perhaps of additional changes, in connection with, or consequent upon, successive anginal or other attacks. It is not requisite that we should delay to enter into an account of these gradually acquired defects.

An experimental familiarity with dyspnœa obliges us to fix the distress in the muscles of inspiration. It is a sense of weight and weariness, or feebleness, of which the impediment in the lungs or air-tubes is the insensible cause. In kind it resembles the dull pain which occurs in the legs of feeble persons on ascending many stairs. It belongs only to the inspiratory movement, though it may sometimes be difficult to detect its intervals. It may either rapidly or slowly become severe, even to intensity; and we need not say that patients are insensible to the slighter forms. The physician is often the first to discover the disorder, and the sufferer may long be unwilling to believe the assurance that his respiration is defective.

Perhaps it is not right to regard merely heavy breathing as dyspnœa, yet it cannot be well to allow the patient to decide when dyspnœa begins. We should be alive to the earliest indication. When the time is come that a cutting pain is hardly an erroneous expression of the difficulty, it is too late for much of useful caution and interference.

The patient need scarcely be deemed wrong for locating the uneasiness in the sternum or breast; for as in disorder of the muscles of the fore-arm, pain is referred to the hand (and the hands, not their muscles, are commonly thought to estimate weight), so, notwithstanding the impressions of an erroneous experience, a tightness of the chest, or a weight upon the breast, is appreciable only by the efforts of the elevator muscles.

We have known some persons attribute cases of dyspnœa to the impeded expiration in general emphysema. Our remark to such is, that we would have them be very cautious how they try the experiment of relieving the state of the patient by artificial compression of the chest. The truth is, the sufferer will be found, not instinctively, but by the force of experience, to do every thing to dilate the chest, and prevent any

afflux of blood from muscular exertion or gravitation. His efforts are all to expand the chest; and gradually the cavity is dilated to the utmost, and the slightest expirations are hardly allowed to take place.

Should it be asserted that the sufferer under old bronchitis and emphysema dies with his chest full of air, and that his last act is not in truth an expiration, we may safely deny this; and we needed not to mention the idea, but to include a remark which is more important. Such an individual, *in articulo mortis* (supposing the fatal conditions of other organs out of the question), labours under a tendency to syncope; the right heart is too full, and the left is too empty, even although the chest is expanded to the utmost, and the extreme stimulus to inspiration will not allow of any but the least expiratory movement.

As we have before shewn, it will be of great use to discriminate between the laborious and even painful inspiratory effort, and that peculiar sense, whatever it may be, which induces inspiration. In extreme cases this may not be possible to the unprepared observer; yet certainly we may find at one time simple and serious dyspnœa by itself; at another the suffocative sense combined with it, in a slight or an eminent degree; and, in a third, there shall be superadded a marked anxiety, or even collapse. A careful observer will, we think, mostly succeed in detecting, in the worst cases, first, laboured inspirations, or muscular distress; secondly, a suffocative sense, or pulmonary obstruction (?); and thirdly, mental anxiety, as well as perhaps vital depression. The remark is important also, that either of these three characters may far exceed the rest under particular circumstances.

We conceive that we have had some useful experimental knowledge of the causes of dyspnœa. Every one is aware that exertion calls for quicker and increased expansions of the chest. Repletion has the same effect, although but few experience the fact impressively, or they would know that from three to four hours after a solid meal, or very speedily after a fluid one, the labour of the chest almost precedes the glow of the face, and the activity of the left heart.

We shall not dwell on the well-

known influence of a full abdomen, nor shall we at present meddle with the obscure subject of pneumogastric and other sympathies.

It is plain enough that very much of dyspnœa depends on the increase of the mass of the circulating fluids. When the greater part of the lungs is disorganized by inflammation, abscess, or compressions, the respiration may continue easy, or be only liable to disturbance with exertion or repletion. The dyspnœa in the cases of wounded lung, or pneumothorax, is greatly controllable by the lancet. The last cases afford us an opportunity for commenting on the actual parts whence the sense of uneasiness originates. Pulmonary turgescence is the state relieved by the bleeding. Dyspnœa and suffocation are the distresses alleviated.

The base of the lung may be said to be, in a degree, an organ in reserve for occasions of unusual need. Perhaps this is most the case in the young. If, on reflection, this statement should seem to belong rather to the cavity of the chest, it is to be remembered that it is also, in a definite manner, applicable to the lung itself. The gravitation of blood in the lungs, and the weight of superincumbent lung substance (in man), except when the tissues are most fully distended, are not to be disregarded. The summit of the chest being expanded and fixed, the lower parts are then enabled to act fully. It is a palpable fact, that it is only in extreme inspiratory acts that some serous surface of the diaphragm is not opposed to the pleura costalis. The various morbid susceptibilities of different parts of the lung and pleura are connected with these facts.

Apart from the general influence of the muscles on the blood-vessels, &c. the posture of the body has much to do with the different kinds of pulmonary turgescence. It is true that, during healthful sleep, the respirations become fewer, though we are less surely informed that the proportion of air in the lungs (considering the blood circulated through them) is decreased. Yet we imagine it to be a fact too much lost sight of, that the dyspnœa which comes on gradually in bed is induced very mainly by the horizontal position. It is not often just to say that the disorder comes on suddenly. It may be more or less rapidly aggravated, or we

may see that when it has arrived at a certain height the distress is sudden. A certain degree of narrowing of the air-tubes, or the like, is easily compensated for by natural efforts, but the slightest additional turgescence constitutes an almost overwhelming obstruction.

These reflections bring us pretty near to the reason why, after some time passed in the sitting posture, the laborious respirations begin manifestly to decline. Under these circumstances a little attention will often discover that the depending limbs become tumid with blood, not with anasarca; and occasionally very trifling movements of the limbs (impeding the general arterial circulation, or driving blood faster to the right heart and lungs) aggravate the disorder. Again, it may happen that food yet in progress of digestion has still to keep up the thoracic fulness or congestion until the general eliminations have reduced the circulating materials to a quantity more consistent with an easy transmission.

We need not say that the above causes must be variously combined, and that we are ready to believe that others exist. While sometimes a little stimulus facilitates the general circulation, and relieves the local, at another a little cup of fluid may suddenly and fearfully aggravate the paroxysm of turgescence; and the former will only add to the evil, if in any degree inflammatory. We think it is often an error to expose the surface of the body for the sake of relief.

In order to become acquainted with perhaps the simplest form of dyspnœa, in the way of experience, the inquirer will only find it necessary, having closed his mouth, to use just so much compression of the nares as will render the passage of air a little difficult.

Dyspnœa sometimes seems to depend alone on obstructions in the left heart, and at times on accumulations in the pleuræ, or on any similar source of pulmonary compression. The lungs themselves may be the causes of dyspnœa, as in extensive pneumonia, disorganizations, or congestions. Bronchitis is also a specific cause of dyspnœa, and also limited laryngitis, as œdema glottidis. Again, distortions or contractions of the chest will produce some dyspnœa even alone. A tight waistband may become excessively distress-

ing, as under repletion, notwithstanding long habit and great resolution. The causes of dyspnœa are still more numerous, and their complications may be said to be innumerable, yet in their chief features they are by no means intricate or difficult to manage in practice. Undue venous accumulations, a bad safety-valve, or too powerful a right ventricle, amongst other important derangements, will well repay the attention of the medical man in relation to these points.

It might be expected that we should not pass over the influences of the nerves and of sympathy; but as, in truth, we must omit still many tangible physical things, besides those which are humoral, we may leave the nervous doctrines for the present, with the same hope that was formerly expressed, namely, that our labours may somewhat facilitate those of the nervous pathologists. We may, however, venture on an illustration or two which may assist to place the relative importance of physical, humoral, and nervous doctrines in a definite light as respects dyspnœa.

A gentleman or a lady complains of indigestion soon after dinner, and it is manifest that the respiration labours more and more; but it is not less evident that the whole vascular system indicates an over-copious digestion. Heated, and red, and full to the eyes, taking a high chair for the sake of combining the relief of muscular rest and sanguineous gravitation and a free diaphragm, the sufferer awaits the climax of digestion, and the gradual diminution of his dyspeptic troubles, by the activity of the general circulation and excretory actions. Others, perhaps a little less oppressed, find relief in hot drinks, or in exercise, or they *sleep it off* with a high pillow.

Mr. Hunter describes the case of an old gentleman, who suffered a good deal of inconvenience from irritation over the inner parts of the thighs, and had a kind of sympathetic dyspnœa whenever he scratched himself. Now if this individual was more or less exposed during the day, and grew warm and replenished towards evening, the irritation coming on, and some augmented thoracic impediment also, we may well suppose that the constrained posture, and irregular efforts in scratching, should disturb the circulation ad-

ditionally. Blood would be driven to the heart; the arterial system would be temporarily impeded (or at least partially); and so pulmonary or bronchial turgescence be set up without any particular sympathy.

If we paid a little attention to such cases, we should detect indications of very different things. Clearing the throat, as it is called, does not refer exclusively to mucus; the gentle half cough empties the capillaries of their excess, and gives a manifest relief for the time. The great difficulty to us lies in the question, why is it that even in the same asthmatic, exercise and stimulus, as well as even food, posture, and temperature, shall have one effect to-day, and the opposite to-morrow? We do not doubt, however, that the difference depends only on the recent supervention of inflammatory causes, on the one hand, and their absence on the other. But our own experience is not to stand for demonstration.

The periods at which we are to seek for the dyspnoea, after the applications of different causes, are worthy of great attention, and still more, perhaps, the times in which the decline of dyspnoea is to be anticipated in various cases. Without these considerations all etiology and therapeutics must be void of rationality.

The vascular fulness from liquid food should be perceived earlier than that derived from the gradual digestion of solid matters; and impediments from these causes, without material complications, should begin to decline within five or six hours (at the most) from the last meal.

There are disturbances of the heart which involve the respiration, but of these we do not now speak, except to set down the remark, that the heart must not be regarded as a single organ: for instance, we conceive that sleep, by itself, may give much more of ease to the left ventricle and its disorders than to the right; and, speaking generally, we would say that the disturbance of the heart in sleep is chiefly in its right side.

The congestion produced by the recumbent posture may be produced quickly or slowly; and other considerable influences being absent, we are assured that its decline will pretty accurately follow the pace of its accession; that is, without regard to the

patient's sudden sense on waking, but respecting the hours already passed in the horizontal posture, provided no previous taking cold, or repletion, or other disturbance, has to be taken into the account. Those who have patience to watch these events will soon perceive the importance of attending to all these kinds of agency, and the remedies will assist to rectify their judgments, when they do not immediately corroborate or justify their opinions.

That more determined and enduring inflammatory states may constitute an asthmatic attack, is not a refutation, but a corroboration of what we have advanced.

The period of the accession of disturbance, in consequence of cold (complications, as of diet, aside), is perhaps still a little less definable. The more delicate betray the effects the more quickly, and *vice versâ*. But even taking into consideration that the exposure may have been slight and protracted, or repeated or severe, or variously aggravated by other circumstances, we are not to expect that an asthmatic can get through the night, in a comfortable bed (favouring alike vascular reaction and local accumulation), without the development of his disorder*. Time and abstinence, posture and remedies, may combine to terminate the paroxysm; food, exposure, imprudences, and recumbency, may favour its return. This is, in a manner, the sum of the doctrine for which we now contend; not only in thoracic pathology, but with reference to the rise, progress, and termination of disease generally.

Those who maintain the spasmodic doctrine of asthma have regarded too little the solid indications of bronchitis, as well as almost all the other points to which we have alluded.

The existence of bronchial muscles is still a problem; and it is still more a problematic statement, that they are the seat of most forms of asthma.

The contractions of the *visible* transverse tracheal fibres we are disposed to date precisely and solely with the acts of expiration, and especially with those which are most violent. Thus, while the evacuation of secretions or dust is more certainly effected, the access of

* We do not mean that the disease cannot be cut short, or obviated; or that gout or diarrhoea may not take its place.

air (with diminished pressure and more tumid capillaries in the tubes?) is facilitated by widening of the passages.

As a general law, it is plain that inspiration is attended by dilatation of the air-passages, and that expiration is not, but rather often the reverse, or by tension of all the orifices, as in sneezing.

Spasms of the bowels, excepting what are inflammatory, spasms of the diaphragm, and stomach, and œsophagus, and bile ducts, and urethra, have been hitherto almost exclusively discovered by the literary and least observing; and although it is not necessary to deny the possible occurrence of these states, yet we have no idea of any observer who has rightly distinguished one of them; notwithstanding they are all unscrupulously employed with asthma to explain and support the doctrine of spasm, as if there were no such thing as congestion, or inflammatory tumefaction, or consequent contraction.

Bronchial affections being quickly discoverable, and claiming often urgent attention, and being so constantly attended with expectoration, and being moreover pretty readily connected with recent causes, and generally amenable quickly to remedies, became very naturally a firm basis of reasoning to the sagacious old humoralists — a basis which has been always held secure.

And it is just to say, that the case of pure muscular asthma (not laryngeal) remains still to be indicated. To be prepared to discriminate the various opposite or different states is the first secure step.

In the following Dr. Forbes seems almost to write against himself; but his testimony concerning cold we think very important; and it seems drawn from some of the best old English observers, who studied when they could attend to nothing but external circumstances. (See the article *Asthma*, Cyclop. Pract. Med.) In the outset the Doctor thus delivers himself:—

“The main phenomena of the asthmatic paroxysm are precisely such as would result from a morbid contraction of the bronchial muscles; and several of them seem inexplicable on any other principle.

“The extreme suddenness of the attack in many cases; its equally speedy removal in others, sometimes

naturally, sometimes through the effect of remedies; the character of the dyspnoea, and of many of the general symptoms; the state of the pulse, the urine, the countenance, the skin, the intellectual functions, &c. &c. all clearly demonstrate it to be of the kind usually termed nervous or spasmodic. The only questions that can be mooted relate to the exact part or parts of these organs in which the spasm is fixed.”

With reference to cold, and its more or less inflammatory consequence, we quote from the latter part of the essay:—

“Although it will appear from what is gone before, that we do not deny the existence of cases of asthma of a purely nervous kind, and altogether independent of any permanent local affection of the bronchial membrane, we are decidedly of opinion that they constitute an extremely small proportion of the cases met with in practice. And we are further of opinion, that out of the immense majority of cases of asthma from other causes, nine-tenths are complicated with some form of catarrh, or, at least, with a morbid susceptibility of the bronchial membrane to be affected by cold. In this very numerous class of cases, then, all those circumstances which induce catarrh, and which may generally be considered as some form or modification of cold applied to the whole body, or to a part of it, must be understood to be the exciting cause of the asthmatic paroxysm.”

We refer the greatest variety of incidental disturbances, in the form of asthmatic attacks, to the effects of cold. Though the preceding causes may exert more or less influence in a given case, we do not know that any other, singly (one only, perhaps, excepted, which we shall yet hint at), is adequate to the production of a paroxysm of dyspnoea. Common asthma is to be regarded as a bronchitis coming on a few hours after some exposure, often very inconsiderable (to one in perfect health), and terminating in secretion. It is not only subject to be complicated by all the foregoing influences, but it is also dependent on the state of the left ventricle, which may either facilitate or impede the flow of the aerated blood; and on that of the right, which may impel a moderate or excessive quantity through the lungs.

The causes which render the simpler cases of dyspnoea, or asthma, most in-

tricate to the medical inquirer, still remain for examination; but, for the present, we shall only point out one or two. We may safely conclude that air in certain states (or in certain places) is capable of action on the respiratory tubes or cells so as to induce dyspnoea, as it sometimes appears to derange the conjunctiva, the nares, the skin, or the lips, &c. We conceive that the state induced in the larynx, &c. is of ingestion, as it is of morbid secretion. This effect, however, is to be investigated mostly as a complication with common cold, or the like, and not a simple or unmixed state.

A second, and indeed the extreme complication, involves in a striking manner what we have formerly treated of under the expression variable disease. (See Guy's Hospital Reports, vol. v. p. 215.) We speak of the established disposition to dyspnoea in dependence on any or all external influences, and the defect of other organs, all combined.

[To be continued.]

ANALYSES AND NOTICES OF BOOKS.

“L'Auteur se tue à alonger ce que le lecteur se tue à abréger.”—D'ALEMBERT.

First Principles of Medicine. By ARCHIBALD BILLING, M.D. A.M. 4th Edition, 1841. (Highley.)

WE noticed, at some length, the third edition of this useful practical work; and the appearance of another edition shews the estimation in which it is held by the profession. The fourth edition has been carefully revised, and much additional matter added in a clear condensed form.

The author was originally led to publish this treatise by a recollection of the difficulties he had met with in the study of his profession, and by the hope that he might aid in removing them from the path of others. After detailing a few illustrations of these difficulties, he endeavoured to draw up a little code of general principles for his own use; concluding that, on investigation, some true general principles would be found to exist, by which the apparent inconsistencies of correct practice might be reconciled, and the contest between such systems as were essentially at variance be decided.

It is impossible in a cursory notice to analyse the numerous topics brought forward, most of them treated with great ingenuity by our author. We shall give a few extracts, in the hope that our readers may be induced to read the original treatise.

In noticing determination of blood, he states—“the word *determination*, in ordinary language, implies that blood is sent somewhere in particular; but the heart has no power to direct any blood to one part more than another, although if in any part there be unusual relaxation of the vessels, they will receive more than ordinary.” An interesting physiological illustration of this principle, in the “determination” of the blood alternately to the stomach and spleen, is adduced. “A given quantity of blood is constantly sent through one arterial channel, which branches off to the stomach and spleen. When the stomach is empty and collapsed, the blood passes into the spongy texture of the spleen, so constituted and situated as to be ready to receive it: on the contrary, when the stomach is distended with food, its elongated arteries admit the blood freely, and consequently the spleen, being the less forcibly injected, collapses and contains less blood. The spleen,” according to the language of Dr. Billing, “thus performs the office of a mill-pond, by receiving the surplus of the stream when not required for the mill—the stomach; and, by this contrivance, the quantity of venous blood sent to the liver from the two organs does not fluctuate.”

The following speculation on hydrophobia is given, viz.—“that it is a leaven which poisons the nervous system, takes a considerable time to ferment, and will eventually be remedied by some medicine which conquers neuritis—perhaps arsenic—perhaps some narcotic. My own inclination,” he adds, “would be to try as much arsenic as the constitution would bear, combined with plenty of opium, which always enables the patient to take more arsenic than he could without it.”

On the action of stimulants, Dr. Billing remarks, that “sedatives, as opposed to stimulants, diminish the injection of the brain, at the same time repressing the nervous influence; so that the proximate cause of delirium, stupor, or coma, from *sedatives*, is

inunition: whereas the proximate cause of delirium and coma from *stimulants*, is *congestion* or *plethora*.

The remarks on the administration of opium in inflammation are well worthy attentive perusal. Its value is pointed out when often morbid sensibility remains, which would wear out and destroy the patient, though the inflammation be past, or arrested by bleeding or other remedies. That it is antiphlogistic is proved by its power in controlling the action of the heart, by allaying morbid sensibility. Indeed, opium or the salts of morphia in large doses, combined with mercury, antimony, colchicum, &c. at the onset of inflammation, supersedes in many cases the lancet; especially in constitutions in which, after loss of blood, the convalescence is protracted.

The following observations on the nature and treatment of *coughs* will give a good illustration of the practical character of Dr. Billing's mind. "Independently of recent cases of inflammation in the chest, as cough is a symptom of consumption (tubercular case of the lungs, to which the term consumption is properly restricted,) it always produces, when long continued, an anxiety in the minds of friends; but cough may arise from a variety of causes which only medical men can ascertain, and which even some of them often fail to distinguish, from not having paid sufficient attention to auscultation; besides which, young practitioners are often not aware of the variety of circumstances which may produce a long-continued cough. I have been consulted for severe coughs of some duration in more than one instance, which I discovered to depend upon a cause at first sight insignificant—as a chronic inflammation, with hardened wax in the ear; and though most persons are acquainted with the fact, that irritating the internal part of the ear will produce coughing, these cases had passed through the hands of several medical men previously, without this cause being detected, although there was a degree of deafness which attracted my attention to it. Chronic enlargement of the tonsils, and the accumulation of a white curdy matter in their ducts, will produce cough; and a relaxed, elongated uvula, it is commonly known, induces a most distressing continued cough; and in all these coughs, when

long continued, the membrane of the larynx and trachea, inflamed by mechanical irritation, gives out extra mucus, thus producing expectoration, with occasionally even streaks of blood.

Such cases occur both in males and females, giving rise to suspicion of consumption. But the most common cause in females is hysteria, often so slight that scarcely any, if any, discoverable irregularity or derangement of the functions of the uterus is evident; and yet the morbid sensibility of the bronchia and larynx resulting thence, produce cough, with alarming symptoms; not merely expectoration, as in the cases just alluded to, and which is chiefly from the continued mechanical irritation of the cough, but also spitting of blood, the occurrence of which gives additional cause of apprehension of consumption.

The mode of treatment of the former cases is obvious to all medical men as soon as the cause is discovered, according to the old adage; and, as to the latter, I have found no difficulty in curing this imagined consumption by directly attacking the primary disease with such remedies as turpentine, iron, cubebs, aloes, quinine, assafoetida, &c. and anodynes, with squills, &c. to allay the morbid sensibility of the bronchia and larynx; at the same time supporting the strength by animal food and fermented liquors, which are too often forbidden, from the practitioner supposing the symptoms to depend on inflammation, instead of attributing them to the true cause—morbid sensibility; and, on that account, not only resorting to low diet and antiphlogistic medicine, but also to frequent bleeding by leeches or otherwise, which makes the patient more hysterical, and increases morbid sensibility.

On the other hand, to shew the value of auscultation where there was serious disease existing, I was consulted by a patient who had been ill about two months; he had all the nosological symptoms of advanced consumption—cough, expectoration yellowish-white with a little blood, night sweats, emaciation, some pain in the side on deep inspiration, &c. Thanks to Laennec, I was able to discover immediately that it was not consumption, but hectic fever from neglected peripneumony; and, notwithstanding the debility, I adopted pretty active treatment—free leeching

of the side, and saline antimonial medicine, with milk and vegetable diet, which soon cured him."

On the effect of climate as a remedy in pulmonary disease there are some excellent practical observations:—"We uniformly see that real consumption (tubercular) runs its course rapidly in Italy, or any warmer climate; such at least is the result of my observation. A deception has arisen, in consequence of persons not really consumptive, but affected with severe chronic catarrh, having been sent into warm climates, who, from the comparatively trifling nature of this disease, have returned cured, or at least not worse. In some of these cases, erroneously called phthisis, the progress of the disease is said to have been checked by the influence of the milder climate. This popular prejudice has still, however, a strong hold on the minds of men, and even auscultation has not yet corrected it. It is generally very unnecessary, and worse than useless, to send patients away from their friends, and often at an enormous inconvenience. If they are consumptive, they will thus die in exile; and if not, they may be cured at home. Of the first it is unnecessary to give examples—there are abundant marble records in the neighbourhood of Leghorn, in the West Indies, and Madeira. A case will explain more fully what I mean by the second. A young gentleman was condemned, by high medical authority, to banishment to Madeira, as 'nothing else could save him;' but to this some strong objections existed. The first was love—he was on the point of being married: the second, his engagements in a valuable business, which depended much on his personal superintendence. A reconsideration of his case was therefore moved for, and my opinion requested. I decided that it was mere chronic catarrh in a relaxed constitution; that some tonic, such as iron or bark, with animal food and fermented liquor, was alone necessary; but above all, exercise on horseback in the cool open air. Under this treatment he recovered within a month, and is now the father of a family." —p. 276.

From these extracts some idea may be formed of the value of this book, written in a simple unpretending style. There are few practical subjects on which Dr. Billing has not touched.

That he is a man of extensive observation, and an original thinker, every one who reads a few pages of the work before us must be convinced. We therefore strongly recommend it to the young as well as to the advanced student; and we feel assured that those whose engagements preclude them from keeping pace with the rapid advancement of medical science, will be amply repaid by carefully reading and pondering over the multifarious subjects brought before them in this volume.

MEDICAL GAZETTE.

Friday, July 16, 1841.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso." CICERO.

PROGRESS OF SELF-REFORM

IN

THE COLLEGE OF PHYSICIANS.

THERE are some people in this world whom it is impossible to please, do what you will. When, for instance, a body of men persevere in a course which has received the sanction of past ages, and under which that body has attained an eminence which renders it the object of envy, there are people who thereupon raise the cry of monopoly, illiberality, and exclusiveness, and cease not week after week to reiterate their charges, and pour the vials of their wrath on the heads of the devoted corporation. By degrees the scene changes. The signs of the times are perceived and felt. A *nova rerum ordo* begins to display itself. Profiting in part by the lessons which it has weekly received, in part by the changes which time has made in its own composition, the corporation enters upon a new course of procedure, evinces a more liberal feeling, shews every disposition to shake off the character of exclusiveness which, whether justly or unjustly,

had been charged against it;—and what now, think you, gentle reader, is the conduct of its early accuser? Doubtless, replies the said reader, he hails the incipient improvement, tenders the hand of good fellowship to the repentant corporation, and encourages it to persevere in the good work so laudably begun. Not a bit of it. Such may possibly have been the proceeding of some periodical journal in the capital of Utopia, in some former age; but, assuredly, such is not the conduct of the Lancet towards the Royal College of Physicians of London in the present day.

Let our readers judge for themselves. A correspondent well acquainted with the subject favoured this journal with a sketch of the measures of self-reform which the College of Physicians has recently meditated, and is prepared to carry out. Shortly afterwards we again drew attention to the subject, and recapitulated the principal points in our correspondent's letter. We freely admitted the necessity of reform. We did not then, nor have we from the earliest period of our labours, ever attempted to palliate the errors of the College. But we rejoiced to see an improved spirit diffusing itself through the corporation, and evincing itself in a series of measures well calculated, in our judgment, to benefit the medical profession, to heal party feuds and animosities, and uphold at the same time the character of the College itself. What is the reply of our cotemporary to these announcements? Why this. It broadly insinuates, on the authority of a correspondent "who evidently writes on good information," that the anxiety of the College for reform was pretended, and "now that Parliament is no longer likely this session to meddle with the College, the hope is *that the whole question may be staved off till another time.*" Not satisfied with this,

it again assails the *Licentiate-Fellows*, by whose co-operation the several measures were carried through, and contrasts their proceedings with those of certain of the Fellows, who are all of a sudden, and for this especial occasion, declared to be "*virtuous and high-principled*," they being the very same parties whom, for the last ten years, the Lancet has been designating as monopolists, exclusives, and bigots.

We must therefore again advert to the question of reform in the College of Physicians, and endeavour to dispel those calumnious misrepresentations of the proceedings both of the College and of certain of its junior members, which the Lancet would fain disseminate. Let us see if a plain tale cannot set this matter to rights. The Lancet complains that the reforms contemplated by the College have not been effected, but still remain a dead official secret. The secret, however, if such it be, is known to very many, and among others to the Marquis of Normanby, her Majesty's Secretary of State for the Home Department, a deputation from the College having (as the Court Circular might have instructed the Lancet) waited upon his Lordship to communicate the resolutions recently passed. So much for the insinuation that the hope of the College is "*to stave off the question.*" We further beg to inform the Lancet, that the opening of the Library and Museum of the College to the Licentiates is to take effect from the 1st of October next, and would have taken place earlier, had it not been necessary to make certain preliminary arrangements. The anxiety of the College to work out the full measure of its contemplated reform is, we can venture to assure the Lancet, much more sincere than that journal either believes or desires.

We must now hold out a helping hand to those especial objects of the

Lancet's vituperation, the *Licentiate-Fellows*, among whom we are glad now to see the name of Dr. Marshall Hall, the quondam friend of the Lancet, but from henceforth, we presume, the theme of its weekly abuse; to be hereafter characterized as the bribe-accepting Licentiate — the renegade — the pretended reformer — the betrayer of his brethren — the graceless victim of a Machiavelian policy. At all events Dr. Hall's best chance of escape is afforded by the unwillingness that the Lancet will have to verify this our prediction.

The Lancet inquires, in a tone of exultation bordering on defiance, whether the Licentiates did not, one and all, vote against a certain motion for admitting the Licentiates and Physicians of England to the Fellowship of the London College of Physicians? How they voted, or how those other Fellows voted, who, after ten years of the Lancet's abuse, were suddenly transformed into virtuous and high-principled men, we are unable to say, and for a reason which we think will be acknowledged as a valid one even by the Lancet, albeit such acknowledgments from such a quarter are rare. It is proper, however, that our cotemporary should know, that from the earliest periods the College of Physicians has adopted, in its proceedings, that palladium of English liberty—that safeguard to good order and virtue, so long and so ardently espoused by the Lancet, called THE BALLOT. Now this great blessing is attended with the great but unavoidable disadvantage, of rendering it extremely difficult, even if not absolutely impossible, to answer the Lancet's question. It may be that the virtuous and high-principled Fellows voted one way, according to the shewing of the Lancet; and the bribe-accepting graceless *Licentiate-Fellows* voted the other way, as the Lancet

alleges. But if this be so, and the Lancet cognizant of the facts, then, we ask, where is the benefit of the Ballot, so much, so long, so loudly vaunted and bepraised by that journal? We recommend, therefore, the Lancet in future either to abandon the defence of the Ballot, or to eschew the practice of questioning the correspondents of this journal.

We have been accused by our contemporary of blinking the several questions at issue: and we are gravely informed, that the Licentiates of the College are not to be easily misled. For once we agree with the Lancet. We believe most sincerely that the Licentiates of the College are not men to be misled by the evasions of one journal, or the misrepresentations of another. They will judge for themselves; and when they hear of certain of their professional brethren formerly associated with them, betraying those to whom they had been virtually indebted for promotion, they will have great misgivings as to the value of the authority on which such statements rest. When they come to sift the matter accurately, it will probably appear, that the question was, not whether the Licentiates and Physicians of England should be admitted to the Fellowship of the London College, but whether the Fellows of the London College should be amalgamated into the class of Licentiates, abolishing thus the very object for which the Licentiates had been petitioning. That a difference of opinion existed among the Fellows as to the propriety of such an amalgamation and fusion of orders is admitted, but the Licentiates of the College who did not hear the arguments *pro* and *con* stated and discussed, will hesitate ere they believe that those among their London associates, who took one view of the question, were therefore to be branded as

betrayers of their brethren, while their opponents were to be lauded to the skies as the "virtuous and high-principled" Fellows of the Corporation. Those among the Licentiates to whom the career of the *Lancet* is as well known as it is to us, will each exclaim, on hearing such things,

Timeo Danaos et dona ferentes.

There must be something more meant than meets the eye, when the *Lancet* characterizes some of the oldest and most respected members of the College of Physicians as "virtuous and high-principled Fellows." Virtuous and high-principled indeed they are, and whatever measures they may have brought forward and advocated, would merit the careful and candid consideration of the College, and, as we firmly believe, did receive all such due attention; but if we ever had any misgiving as to the policy of those measures, such doubts would not be removed from our minds by the consideration that the *Lancet* has taken them under its special protection, and proclaimed to its readers that "they were supported by every virtuous and high-principled Fellow in the Corporation."

MEDICAL REFORM.

ANSWER TO DR. HULL.

To the Editor of the Medical Gazette.

SIR,

WILL you permit me, through the medium of your columns, to make a few remarks on the paper of Dr. R. Hull, contained in your journal of the 2nd instant. I ask this, because Dr. Hull has chosen to attack the qualifications of the great body of general practitioners *as a body*; placing an immeasurable gulf between their acquirements and those of his own class; but, more particularly, because he has poured the vials of his wrath on those who, possessing a degree, act as general practitioners. He declares himself most strongly against Medical Reform, and charges those who advocate it with not

being "true Englishmen," with being "lovers of America," and "imitators of France." But, further on, he says, "The agitation is raised by the general practitioners." May we infer from this, that the general practitioners are, as a body, not "true Englishmen," that they are "lovers of America," and "imitators of France," which is the logical inference from his own premises? No, Dr. Hull; the general practitioners, as a body, are as respectable men, as much true Englishmen, as little lovers of America, or imitators of France, as yourself.

Only look back on the state of the profession, and the changes that have taken place in it during the last forty or fifty years; you will then see, that the apothecaries have been gradually rising in respectability, and in knowledge of medicine and its collateral sciences, until on those subjects they have, as a body, become equal to the physicians, also taken as a body. I would ask Dr. Hull how and where physicians gain their medical knowledge? In Oxford or Cambridge they cannot do it. In the Scotch universities they may: but does Dr. Hull mean to assert, that the lectures and mode of study, given and pursued in the Scotch universities, are superior to those of the medical schools of London; and are the "boys" sent to the one at all more talented than those who proceed to the other? Surely Dr. Hull would not make such an assertion. Besides, Dr. Hull, I would remind you that many of the most eminent of physicians have handled the pestle and mortar of which you speak with so much disdain, as if it were a disgrace or pollution to touch such an execrable instrument. I have only to turn over a few pages of the N^o. of the *MEDICAL GAZETTE* in which your paper is inserted, to find, among the newly-elected *Fellows of the College of Physicians*, one who has so disgraced himself, in your opinion. I allude to Dr. A. T. Thomson, who was many years a general practitioner. So, then, throwing away the pestle and mortar on the one hand, and the lancet and knife on the other, and obtaining a degree, with or without an examination, raises a man from an inferior to a superior class. But does Dr. Hull forget that, till within these few years, a degree could be got for £20 by almost any one, without examination, and without any proof of his competency? all that

was necessary being the recommendation of two other physicians: and how easily such recommendations may be obtained by the most ignorant of mankind Dr. Hull must know, if he has not lived with his eyes closed to what was daily passing around him. Dr. Hull must be also aware, that although "the upholders of medical reform in Parliament have all of them been radical reformers, and in the profession itself reforming radicals," yet, if he questions many, I think I may say most, of his medical friends among the general practitioners, he will find, (even among those who profess conservatism in general politics), an almost universal wish for medical reform.

Whatever epithets he may apply to the original promoters of medical reform, or whether these epithets be true or not, is a matter of little consequence, since the justice or injustice of a cause cannot depend on its supporters, but must rest on its own intrinsic merits or demerits. The very quarrel between the pure physician and pure surgeon concerning the boundaries of their respective professions, must show to any unprejudiced person that they are very indistinct and variable; that what at one time was thought to belong to the one, at another time is thought to belong to the other! If, then, the boundaries are so very variable and indistinct, it is manifest that the pure physician and pure surgeon must intermix their studies and practice to a very great extent, and become, in fact, *almost* general practitioners. The ironical banter about admirable Crichtons amused me exceedingly, and must amuse all general practitioners; but I would remind Dr. Hull, that however banter and invective may amuse a crowd of ignorant men, when put on paper, and left for the serious perusal of the well-educated, it can only excite bad feeling towards the person who would make use of so unworthy a weapon.

A little further on Dr. Hull gives some of his own advice to two imaginary personages—Mr. Dix of Modbury, and Mr. Hosegood of Piddletrenthide, an equally imaginary place, the name of which appears chosen, if we may take the two first syllables alone, to throw vulgar ridicule on the future residence of the young general practitioner. He tells them in this advice, that they are to be *rustic physicians* and

rural surgeons. It is rather curious, when we consider that Dr. Hull places so great a distinction between physicians and general practitioners, that he should call general practitioners physicians; for, if they are physicians at all, he places them on an equality with himself; unless, indeed, he would make a great distinction between "rustic" and city physicians, which would be a mere distinction of locality. If the value of life be equal to all, whether residing in the country or in a city, all who have the care of human life ought to have an equal education; but the value of life is equal in the country and in a city, therefore all who have the care of human life ought to be equally educated.

By this reasoning I must not be understood to say, that all who practise medicine in its various branches are equal in their knowledge of medicine. Such a result cannot follow any system of education; but that all should be subjected to an equal examination, as a test of their *capability to practise without endangering the lives of her Majesty's subjects through ignorance*; and then be left to chalk out their future course, whether in physic or surgery, or any particular department of either, and not bound down by legal enactments to follow any one course. I think with Celsus when he says, "Ego eundem quidem hominem posse omnia ista prætere concipio; atque, ubi se diviserunt, eum laudo, qui quamplurimum percipit"; which would put many of those *pure* physicians, who declare themselves ignorant of surgery, and glory in that ignorance, to the blush. Dr. Hull appears very angry that "one in authority has ventured to deny the inherent essential differences between surgery and physic, which require that they should be exercised by different persons, and taught by different teachers;" has cavilled at "a College of Physicians and a College of Surgeons." Yet he does not show that this authority is wrong; but, instead of attacking the opinion, shifts the burden of proof on the assertor of the above propositions. It is evident that the same processes, modified by the circumstances of the situation and structure of the organs, produce both external and internal diseases, and that similar processes for their reparation, similarly modified, produce a return to a healthy

state. Inflammation has its three terminations—resolution, suppuration, and sphacelus, as well in external as internal parts; cancer, which in one part belongs to the surgeon, in another belongs to the physician; hectic, when arising from abscess, belongs to the surgeon; hectic from phthisis to the physician. I might greatly multiply examples, were space allowed me, but the only inference I can draw from circumstances is, that inflammation, cancer, and hectic, should alike, in all their modifications, be studied by both the physician and surgeon, whether the causes from which they arise belong to the physician or surgeon, as it is certain that they illustrate one another. If, then, it be necessary for each to study all the more important diseases that belong to the other, the physician is, in point of fact, the surgeon (if we omit the use of the knife), and the surgeon the physician. For my own part, I cannot understand a man's being a pure surgeon or physician in knowledge, although he may be so in practice. Manual dexterity in operations can only be acquired by practice, but a *perfect* knowledge of disease, so far as knowledge of it extends, is equally to be acquired by the general practitioner and the physician; indeed, I think the former has the greater facilities, if he has the talent and will to perfect himself, so far as is possible, in the knowledge of medicine. Talented men will occupy a prominent situation in the profession, whether they be physicians, pure surgeons, or general practitioners: but as most men wish to rise as high in rank as possible, the talented general practitioner will, if he can, obtain a doctor's degree, in order not only to obtain rank, but also to secure a larger share of remuneration for his exertions; and hence many men who have advanced the profession by their writings as physicians have been originally general practitioners, have handled the contemned pestle and mortar. As the general practitioner has more *opportunities*, if he has the *will*, to examine disease, inasmuch as he attends more cases than the physician, and as he obtains the same education, so far as medicine is concerned, with the physician, it is evident, supposing the two to have equal talent for observation, that the general practi-

tioner will have the advantage over the physician. He has also this further advantage, that he has studied the principles and practice of surgery, which even Dr. Hull must admit has an intimate connection with medicine. Now considering the average talents of men, we cannot suppose that among the men destined by their friends to the medical profession, the physicians should monopolize the talent, but that it will be pretty equally distributed between the two classes, and, therefore, it is unjust to the general practitioner to place him below the physician. Hence I infer that one test of capability should be applied to all, leaving them afterwards to follow what branch they please, and to take what honorary titles (earned, mind, by more stringent examinations) they may desire. This would be placing medical men, like horses on the race-course—even at first, but gaining pre-eminence by their strenuous after-exertions.

As the minds of men are so differently constituted, and their tastes so various—some will take up some part of science, others another—there will be still division of labour. “Abercrombie will still be consulted on the brain, Hope on the heart, and Johnson on the bowels.” Some will take up Anatomy, some Physiology, others Chemistry; but the public in general will be satisfied as to the capability of all to treat diseases, although some may be more clever in treating some particular diseases than practitioners in general. “*Palmarum qui meruit ferat.*” Medicine is now, as it was in the time of Celsus, divided in three parts. “*Iisdem temporibus in tres partes medicina deducta est ut una esset, quæ victu, altera, quæ medicamentis, tertia quæ manu mederetur. Primam, διατητικὴν; secundam φαρμακευτικὴν; tertiam, χειρουργικὴν; Græci nominarunt.*” I find no hint in Celsus that physicians did not in his time both compound and furnish their remedies to their patients, but whether they (the patients) paid for their medicine or advice, he does not state; it seems, however, pretty certain to me, that they did compound and furnish their medicines, and in that they did what Dr. Hull so strongly condemns, and what he upbraids those who

possess a degree and assume its title, while practising as general practitioners, for doing. Alas, poor Hippocrates! alas, poor Celsus! and ye host of great Grecians, ye fall under the lash of Dr. Robert Hull! ye are degraded to apothecaries, and ye violate the gentlemanly practice of dropping your degree (physicianship), when you send out your medicine, apply dressings to a wound, or take scalpel and saw in hand! Vile occupations! only fit for the low-bred surgeons and apothecaries. But—stop—I had forgotten that I was getting into the bantering style I condemned in Dr. Hull, but really in reading his paper for the purpose of commenting on it, I had actually imbibed some of his spirit, on the principle that “like begets like.”—But, Mr. Editor, I am one of Dr. Hull’s proscribed class, one who should be cut by all others; in short, one who possesses the degree of M.B. and practises as a general practitioner. However, I have no objection to being in such good company as that of Hippocrates and Celsus, notwithstanding Dr. Hull’s previous denunciations. I am glad I do not stand alone, but not only have my confrères of the present day, with whom Dr. Hull seems so angry, (perhaps for poaching on *his own manor*?) but the first and greatest of all physicians, Hippocrates, who, I suppose, compounded his own medicines, as I am not aware that there were any such classes as apothecary and chemist and druggist in those days, when the ARTIFICIAL distinctions in medicine had not been instituted. Perhaps, however, I am in error on this point, and Dr. Hull, who is evidently a better *Grecian* than framer of axioms and syllogisms, will be kind enough to correct me. With this I conclude, wishing that Dr. Hull may not be incommoded by the too great proximity of one of these physician-general-practitioners, and subscribing myself

Your obedient servant,

PHILIP B. AYRES,
M.B. Lond. M.R.C.S.L.

Thame, Oxon, July 9th, 1841.

ROYAL MEDICAL & CHIRURGICAL SOCIETY.

Tuesday, July 6, 1841.

DR. CLENDINNING IN THE CHAIR.

Case of Aneurism of the Right Subclavian Artery, in which that vessel was tied internal to the scalæmus muscle. By RICHARD PARTRIDGE, F.R.S. Professor of Anatomy in King’s College, and Surgeon to King’s College Hospital.

THE patient, David Hickman, was a married man, thirty-eight years of age, muscular, of a sallow complexion, and had lived rather an intemperate life. His occupation was that of a grinder and polisher of heavy iron plates, an employment requiring great muscular exertion of the arms. He had been the subject of several slight rheumatic attacks; and once, about a year before his admission into King’s College Hospital, he had, what he called, a fever, which laid him up for six weeks, and was attended with cough and pains about the upper part of the chest. After this illness he was always subject to pains below the clavicles, which were brought on by any unusual muscular exertion. In August, 1840, about five months before admission into the hospital, he began to experience pains along the right side of the neck, and over the corresponding shoulder, with numbness of the arm, and an increasing want of muscular power in the whole limb. About a month after the appearance of these symptoms, a pulsating tumor shewed itself above the right clavicle, and, as this enlarged, the numbness of the arm, and the pains in the neck and shoulders, became worse, so that finally the man was obliged to abandon his employment.

Feb. 1, 1841, Hickman was admitted into King’s College Hospital. He had then an aneurism of the right subclavian artery, extending from the outer border of the anterior scalæmus, which it appeared to overlap, downwards behind the clavicle into the axilla as far as the lesser pectoral muscle. The tumor was slightly compressible, and appeared to contain fluid blood. Upon a careful examination, the right common carotid, and the subclavian internal to the scalæmus, seemed healthy; the arteria innominata was thought to be enlarged as well as the arch of the aorta. It could not be ascertained that they were otherwise diseased. No disease could be detected in the heart or lungs, though there was pain on percussion beneath the right clavicle. The pulse at both wrists was about 80, full, soft, and regular. The right upper extremity was somewhat wasted comparatively with the opposite

limb. The patient's rest at night was much disturbed by the pains and numbness of the arm.

The patient was placed in bed at perfect rest, with low diet and occasional aperients.

The pains in the limb were relieved by wrapping it in flannel; and this, with an occasional opiate at night, procured him comfortable sleep. The patient was bled from the left arm, after admission, four times, at intervals of three or four days. After these venesections the pulse always became lower, and they appeared to have the effect of diminishing the pains in the arm and shoulder.

20th.—Mr. Partridge put a ligature on the subclavian artery internal to the scalæmus. An incision was made, between three and four inches in length, along the clavicle to the centre of the upper border of the sternum, dividing the skin and platysma. The sternal origin and a part of the clavicular fibres of the sterno-mastoideus were next exposed and divided, and then a small anterior jugular vein which crossed the incision. Afterwards the sterno-hyoid and sterno-thyroid muscles were cut across, and, after some careful dissection, the artery was exposed, lying very deeply at the bottom of the wound. The vessel was large, but otherwise healthy: the internal jugular vein and the nervus vagus were drawn to the outer side of the wound, and a strong thread ligature was passed around the artery by means of a common aneurism needle; care being taken to avoid wounding the pleura which lay immediately underneath the vessel. Upon securing the ligature, pulsation in the tumor and at the wrist ceased. Soon after being put in bed the man felt sick, and vomited. In the evening about four ounces of blood were lost from the small external jugular vein, which had been divided during the operation: it was secured with a ligature. At night the patient complained of pain at the pit of the stomach: this was relieved by an opiate draught, and he dosed quietly until morning, when the pain at the stomach returned severely, accompanied with thirst: it yielded to venæsection; but after a few hours recurred, and was not as before relieved by bleeding. The thirst also returned and pain was felt along the right side of the sternum, and the pulse and respiration were quickened. A very faint pulsation was felt at the right wrist, and continued until the patient's death. The symptoms increasing day by day, the patient soon became exhausted; his face was pale and covered with a clammy sweat; his look became anxious; the pulse small, and very rapid, and the respirations quicker. The thirst was excessive, but all attempts to relieve it by fluids taken into the stomach produced in-

tense pain at the scrobiculus cordis. Temporary relief was afforded by leeches and mustard poultices applied to the chest and pit of the stomach, but the pains soon returned. On the 24th February (at 11 A.M.) the fourth day after the operation, the man died. On the 26th the body was examined as carefully as could be done at the patient's house, where it had been removed by the friends. The aneurism was found to extend from the outer edge of the scalæmus to the lesser pectoral muscle; internally the sac turned over the outer border of the scalæmus, and overlapped it. It contained coagulated blood, but no fibrinous deposits, and its coats in some places were exceedingly attenuated. It would have been impossible to have placed a ligature on the artery, either on the outer side, or behind the scalæmus. The ligature was found on the artery midway between its origin and the inner border of the scalæmus anticus. The nervus vagus, the recurrent nerve, and the pleura, were uninjured.

In the chest, on both sides, there were found old adhesions between the pleura costalis and pulmonalis, with serum in the cavities of these membranes, but especially on the right side of the chest, where the pulmonary portion of the pleura was slightly coated with recent lymph. There were tubercles, in a quiet state, however, scattered through the upper lobes of both lungs.

A small quantity of serum was found in the pericardium, and its inner surface was coated with a thin layer of recent lymph. The heart, arch of the aorta, arteria innominata, and both common carotids, though rather larger than natural, were healthy. Three small deposits of pus were found in the cellular tissue near the wound, and along the anterior mediastinum: they were unconnected with each other. The wound itself looked healthy. No clot existed either in the subclavian artery or in the vessels springing from it. None of the veins were inflamed. The stomach appeared quite healthy.

The author, after detailing the case, states his reasons for putting a ligature on the subclavian artery internal to the scalæmus in preference to any other operation. He next proceeds to take a review of the symptoms, (with the probable cause of death), and compares them with those which were observed in the other cases in which this operation had been performed.

An account of two cases of Aneurism of the Trunk of the Superior Mesenteric Artery, in one of which Jaundice was induced by Pressure of the Sac. By JAMES ARTHUR WILSON, M.D. Physician to St. George's Hospital.

The symptoms, which had most attracted

attention during life in the first of these cases, had been very severe pain between the shoulders along the track of the sixth or eighth lower dorsal vertebræ. The patient died, after illness of about six months, in a state of great exhaustion, much aggravated by mercurial salivation.

On examination of the body a large globular tumor was seen extending from behind the head of the pancreas upwards, forwards, and outwards to the right side. The ductus communis was in close contact with this sac, but was, however, pervious to a probe. The poribiliarii of the liver were universally much enlarged.

The heart was small: the membrane lining its cavities uniformly yellow. Tubercle of a consistence like mortar, and of a yellow colour, were observed in the lungs. In the head the dura mater was universally yellow; but both tunica arachnoidea and pia mater were free from that colour. The substance of the brain was also normal in colour; but a thin yellow fluid could be pressed from the divided surfaces of many of the vessels. The synovial fluid contained in cavities of joints was yellow: their cartilages were of the normal colour. The stomach contained thick yellow mucus.

The author observes that this case may lead us, under similar circumstances, to apply the ear to the upper part of the abdomen as a means of inquiry; it may also prevent our being taken by surprise in the event of sudden death: he also remarks on the inefficiency of the mercurial treatment adopted. In the other case noticed by Dr. Wilson there was a tumor pulsating in the epigastric region, about the size of a small orange, which, when the patient lay flat, projected to the left of the scrobiculus cordis. When the patient turned to the left side, the tumor ceased to be perceptible. On his turning to the right it might again be observed. Between Feb. the 11th, when he was admitted, and July the 12th, when he died, he was attacked with frequent hæmoptysis; and towards the last symptoms of phthisis presented themselves. In the course of this illness there was severe and increasing pain down the dorsal vertebræ, and cramps in the legs; and the tumor became more and more tender to the touch.

The aneurism in this case was in the trunk of the superior mesenteric; it was large, and kidney shaped, raising up with it the pancreas, which lay at the upper extremity of the tumor.

The author notices, as distinguishing points in these two cases of aneurism of the superior mesenteric artery, that jaundice was, during life, a symptom of the one,—hæmoptysis of the other. In the latter case, the lungs, he observes, were extensively diseased by tubercles of the common kind.

Case of fatal Encephalitis, with hemiplegia, immediately excited by Cantharides, in consequence of intense predisposition from basilar and internal carotid aneurisms. By P. N. KINGSTON, M.D. Physician to the St. George's and St. James's Dispensary.

James Hullah, between fourteen and fifteen years of age, shoemaker, admitted Dr. Kingston's patient at the Dispensary, October 17, 1836.

He has for three years and a half had a throbbing tumor at the right side of the neck, which has gradually reached the size of two-thirds of an orange, but has otherwise been considered strong and free from ailment till this morning, when he was suddenly seized with vomiting and with numbness and almost total loss of power of the trunk, the right arm and leg, the right side of the mouth, and the tongue. Pulse 80. Bowels confined for four days.

He died in thirty-eight hours and a half. During this period the urine was almost entirely suppressed, the vomiting continued frequent, there was occasional suspension of respiration, followed by spasm of the larynx; he lost, after a time, the power of deglutition, and became by degrees completely comatose.

It was ascertained that the night before the seizure he had taken some Spanish fly in an apple.

Sectio Cadaveris, fifteen hours after death.—Patches of the mucous membrane of the stomach presented a degree of redness, softening, and elevation, which contrasted strongly with the appearance of the adjacent parts. The spleen was softened; the kidneys were much congested, and their cortical portions softened; the pelvis of the right was filled with nearly opaque, white, flaky fluid, of creamy consistence. The mucous membrane of nearly half the bladder was deeply reddened and much softened.

There was a large aneurismal dilatation of the right internal carotid artery, and one of the basilar artery, of the size of a moderate walnut, which appeared to have formed gradually, and without rupture of its tunics. The pons varolii had become completely flattened by the pressure of the basilar aneurism, and was much softened for the depth of a quarter of an inch. The right lateral ventricle contained an ounce of limpid serum: the left, none.

In his evidence, given at the Coroner's Inquest, on this case, the author commented upon the fact, that the fatal termination immediately occasioned by cantharides would not have resulted from so small a dose, but for the predisposing causes here noticed; and he points out the application, of which this kind of reasoning is capable, in many trials for murder.

ON THE SPONTANEOUS EVOLUTION
OF
SULPHURETTED HYDROGEN IN THE
WATERS OF THE WESTERN
COAST OF AFRICA,
AND OF OTHER LOCALITIES.

By J. FREDERIC DANIELL,
or. Sec. R.S., Prof. Chem. in King's College,
London, &c.*

My attention was first directed to the subject, which I shall have the honour of submitting to your notice this evening, by the Lords of the Admiralty, in April 1840, who sent me eight specimens, and afterwards two additional specimens, of water from the mouths of the rivers on the western coast of Africa, with directions to analyze them, for the purpose of discovering, if possible, the cause of the rapid decay of the copper-sheeting of ships employed under those stations.

Of the comparative duration of the metal in the vessels of the Royal Navy, I have not been informed; but the evil complained of is well known also in the merchant service; and upon inquiry of one of the largest copper-smelters in South Wales, he assures me, that "the experience of between thirty and forty years has led his mind to the conclusion, that sheathing copper will be as much or more injured in a nine-months' voyage to and along the coast of Africa, as by the wear of from three or four years on any other trade."

The first water which I examined was from the river at Sierra Leone, taken at three miles from the mouth. Upon drawing the cork of the bottle it was found to smell very strongly of sulphuretted hydrogen.

The first idea which occurred to me was that which seems generally to have prevailed upon similar occasions, viz. that this gas was generated from some change which had taken place in the water after it had been bottled, from the decomposition of some animal or vegetable substance; but a little consideration showed that this explanation was quite inadmissible, inasmuch as the sediment from the whole bottle did not exceed half a grain, the water being perfectly bright, and the salts upon evaporation *snow-white*; and the water became perfectly sweet a very short time after it had been exposed to the air.

Indeed, the common prejudice regarding the unlimited quantity of sulphuretted hydrogen generated by putrescence is perfectly untenable, and is founded solely upon its disagreeable odour. The fact is, that the quantity of sulphur in animal matter is very

small, and the nauseous smell is by no means an infallible criterion of the existence of the gas.

As a natural product, sulphuretted hydrogen has hitherto been known chiefly as an ingredient in certain mineral waters, such as those of Harrowgate and Aix-la-Chapelle; the former of which contains, per gallon, 18.4 cub. in., the latter 44.0. The comparatively small springs which yield these waters are most carefully preserved by their proprietors, on account of their medicinal virtues, and the profits which are derived from their use.

The generation of the gas with which such waters are impregnated, has been usually attributed to some unknown action upon pyrites and other sulphurets of the metals in the interior of the earth, and it is pretty generally ascribed to volcanic action. It has only been generally known that sulphuretted hydrogen is produced by processes of decay or fermentation, in which large quantities of animal matters are concerned.

To the natural sources of this gas must now be added the estuaries of many large rivers and immense tracts of the ocean in their immediate vicinity.

The results of the analysis of the African waters, sent to me from the Admiralty, embracing an extent of fifteen or sixteen degrees of latitude, are as follow:—(See next page.)

Thus there can be no doubt of the important fact of the impregnation of the waters, upon the western coast of Africa, with sulphuretted hydrogen, to an amount, in some places, exceeding that of some of the most celebrated sulphur springs in the world; and of the injurious effect of such impregnation upon the copper sheathing of ships, you will be convinced by the experiments upon the table.

Were any further evidence wanting, it would be found in the state of the copper of the *Bonetta*, which lately returned from the coast of Africa, and three sheets of which were sent to me from the Admiralty for examination.

Nos. 1 and 2 were pretty uniformly covered on the outside with a green crust; and on the inside, as evenly, with a black crust of equal thickness. They were very thin in parts, and here and there eaten into holes.

No. 3 was in a much worse state, very thin, and eaten into large holes. In most parts it was easily broken by the fingers; one of the holes, of an irregular shape, measured eighteen inches in length by four inches and a half in width. This sheet was covered with green crust chiefly, on both sides; but there were evident traces of the black crust on the inner side.

Upon analysis the black crust was found to consist of sulphuret of copper, and the green of oxy-chloride of copper.

* From the London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science, for July.

The following are Professor Daniell's results: the chemical details we omit:—

	River at Sierra Leone, 3 miles from the mouth.	River Volta, 28 miles from the mouth, at Sea.	River Bonny, off the town.	River Mooney, 1 mile inside the mouth.	River Gaboon, 8 miles up the river.	Cape Lopez Bay.	River Congo, entrance of the river.	River Congo, 35 miles up.	River Bongo, 40 miles from mouth.
Sulphuretted Hydrogen .	6.18 cub. in.	6.99	1.21	11.69	0.67	...	4.35
Chlorine .	943.14 grs.	1411.68	970.92	1184.11	1130.75	1467.37	106.11	Chlorides of Sodium, Magnesium, and Sulphate of Soda, 8 grs.	1513
Sulphuric Acid .	82.70	92.47	92.10	109.80	120.08	115.20	2.30		128
Lime .	19.14	14.75	17.36	14.17	23.05	23.21	...		1095
Magnesia .	27.68	35.70	33.65	44.78	43.58	41.02	...		
Magnesium .	32.71	12.46	47.11	28.54	35.41	28.44	...		
Sodium .	553.33	916.20	553.06	732.32	683.00	921.60	70.00		
Potassium	A trace	A trace	A trace	A trace	A trace	...		
Iodine	A trace	A trace	...		
Total in an Imperial gallon	1668.70	2483.26	1714.20	2113.72	2035.87	2596.84	178.41	8.0	2736

There can be no doubt that the injury of the copper arose primarily from the sulphuretted hydrogen. The gas appears to have penetrated to the inner side of the copper, where in Nos. 1 and 2 it has been protected from the further action of the sea water; by which, on the outside, the sulphuret was converted into chloride of copper. This conversion appears to have taken place on both sides in No. 3, from the water having penetrated to the under side in consequence of its greater corrosion.

That the establishment of this fact is of some importance in a mercantile point of view, I think I shall be able to convince you by two anecdotes which I will now narrate.

Not many years ago a new Copper Company set up a smelting establishment and brought their copper to market: some merchants purchased sheathing of them, coppered their ship, and sent her to the coast of Africa. Not many months after she returned to this country with the copper in the same state as that of the *Bonetta*. The merchants said—the copper-smelters were inexperienced hands—they did not know their business—the sheathing was improperly made; and they brought an action against the Company, who defended it.

Upon the trial some of the most eminent scientific men of the day gave evidence that there was nothing in sea water which could produce such rapid decay of the copper, and the jury, in consequence, brought in a verdict for the plaintiffs.

Now contrast this with what has happened to me within the last two months. An eminent copper manufacturer of South Wales, who had heard nothing of the investigations in which I had been engaged, came to me with two samples of copper which he wished me to analyze. The one was of new metal, and the other part of the sheathing of a ship which had just returned from Africa, after a voyage of a few months, the copper being in a state of utter decay. He stated that the merchants to whom the vessel belonged had brought an action against him on the plea that the copper was imperfect, and he wished for my evidence upon the subject, as he well knew that the copper was perfectly good. Instead of entering upon the analysis I gave him a copy of my report upon the waters of the western coast of Africa, which he sent to the merchants, and nothing further has been heard of the action.

But it may perhaps be said that little good will arise from pointing out the evil, unless we are prepared to propose some remedy for it; not that I agree to this, for the existence of the sulphuretted hydrogen is so readily tested, even by the roughest hands, that nothing can be easier than to ascertain and avoid the localities in which it prevails; motives for which course I shall presently

mention, of greater weight even than the preservation of the copper.

But I think that the remedy is certainly within our command. The principle of protection proposed by Sir H. Davy is quite applicable to it, with some additional precautions suggested by this newly-discovered destructive agent, which had escaped his notice.

It is well known that his experiments were conducted chiefly with zinc and iron, as the active elements of protection; and he was led ultimately to the adoption of cast iron, "as the substance which is cheapest, most easily procured, and likewise most fitted for the protection of the copper*."

But this is not the case with regard to sulphuretted hydrogen; for as you will see by reference to the experiments upon the table, copper is much more acted upon by this substance than iron, the latter being protected by the former; and the fact is, that a piece of iron attached to copper increases the corrosion of the latter.

Zinc, on the contrary, protects the copper not only from the action of the chlorides in sea water, but also from the sulphuretted hydrogen. This I have ascertained by the experiments before you, and you will find that the results are in perfect accordance with the electric order of these two metals in solutions of the hydrosulphurets, as given by Dr. Faraday in his last beautiful number of *Experimental Researches in Electricity*.

In the table which he gives, iron stands far above copper in electro-negative order, and zinc below it; lead is also above zinc; while in the usual acid solutions both zinc and iron stand below the metal†.

Now I have long been of opinion that the experiment of voltaic protection in the Navy was much too lightly abandoned upon the first appearance of an unforeseen difficulty, and that under circumstances otherwise the most encouraging.

This abandonment, you are aware, arose from what might be called over-protection, by which the attachment of weeds and zoophytes to the ships' bottoms was found to be encouraged. Earthy deposits were formed, and to these the weeds and shell-fish attached themselves.

The remedy for this appears to me to be obvious: instead of keeping the protectors always in contact with the copper, let them be insulated, and let them only be brought into metallic contact when occasion may require. This might readily be done by means of a bolt or bar, forming in one position a continuous conductor between the two metals and in another breaking the connexion: this might always be at command of the proper officer of the ship. Nothing could

then be easier than to throw off the protection when the ship is in harbour, or in situations particularly liable to deposits; or to restore it upon going to sea, or arriving in latitudes where sulphuretted hydrogen might be found to exist.

But the protectors should invariably be of zinc, which would preserve the copper not only from the effects of sea water generally, but from the more destructive agency of sulphuretted hydrogen, which I shall presently give you my reasons for concluding not only prevails upon the western coast of Africa, but in other situations where it has never yet been suspected. Indeed I incline to believe that it would only be found necessary to use protection in sulphuretted waters, and that the action of the chlorides alone might not be more than sufficient to preserve the copper from deposits.

Another motive for this change may be found in an observation of Sir H. Davy, viz. that a "common cause of the adhesion of weeds or shell-fish is the oxide of iron formed and deposited round the protector. In the only experiment in which zinc has been employed for this purpose in actual service, the ship returned after two voyages to the West Indies and one to Quebec perfectly clean. The rudder, which was not protected, was corroded in the usual way*."

But it is impossible not to speculate upon the origin of the deleterious gas which has thus been found to contaminate the sea upon the western coast of Africa, in such enormous quantities, through an extent of more than sixteen degrees of latitude, and reaching in places forty miles seawards, making altogether an area of 40,000 square miles in extent.

Volcanic action seems naturally to suggest itself, but is negatived by the absence of any other indications of such actions along this line of coast; and I think that I shall be able to convince you, by the evidence of experiment, that the real cause may be found in the mutual reaction of the immense quantities of vegetable matters, which must be brought down by the intertropical rivers, and the sulphates of the sea water.

The idea was suggested to me by a memoir of my friend Dr. Malcolmson, in the *Geological Transactions*, who speculates upon the origin of sulphuretted hydrogen in the saline lakes of different parts of the world, being "the decomposition of the sulphates in the water by the carbonaceous matter of vegetables." I tested this hypothesis by experiments in the following way:—

On the 2nd of November, 1840, I placed a quantity of newly-fallen leaves in three glass jars capable of holding about a gallon and a half of water.

* *Phil. Trans.*, June 1824, p. 243.

† *Ibid.* 1840, p. 113.

* *Phil. Trans.* 1826, p. 420.

No. 1. Upon the first I poured about a gallon of New River water.

No. 2. Upon the second I poured about the same quantity of the same water, in which three ounces of common salt had been dissolved.

No. 3. Upon the third, the same quantity of water, in which three ounces of crystallized sulphate of soda had been dissolved.

The three jars were then placed in a warm chamber, the temperature of which varied from about 70° to 110°, and the water was filled up from time to time, as it evaporated, and the mixture well stirred.

Upon examining them on the 5th of February, 1841 (three months), the following was found to be the state of the jars :—

No. 1 had a very disagreeable odour, but produced no change whatever upon paper soaked in acetate of lead.

No. 2 was perfectly sweet, and possessed, indeed, a rather agreeable odour. It produced no effect, of course, upon the test paper.

No. 3 had a most insupportable sickening odour, much worse than that of pure sulphuretted hydrogen, and instantly blackened paper soaked in acetate of lead, throwing down sulphuret of lead with a metallic lustre.

You will have an opportunity of observing, by the specimen upon the table, that the evolution of gas is at this moment proceeding with increased energy.

[To be continued.]

MEDICAL REFORM MEETINGS IN MARYLEBONE.

To the Editor of the Medical Gazette.

SIR,

Two meetings have been called of the medical practitioners of the borough of Marylebone, for the purpose of ascertaining the views and intentions of the candidates for the representation of the borough respecting medical reform. I attended the first meeting for curiosity merely, and, although I waited for half an hour beyond the time that I expected the chair would be taken, I came away before the business commenced. I was not the only practitioner who did so. Now as these meetings have been reported in the metropolitan and provincial journals, I beg that the chairman or chairmen of these great meetings will have the candour to inform the profession how many practitioners they believe attended at each of these meetings, that we may have a fair and just idea of the strength of the medical reformers of the vast borough of Marylebone.

I am, sir,
Your obedient servant,
JOSEPH HOULTON.

Lisson Grove,
14th July, 1841.

ROYAL COLLEGE OF SURGEONS.

At a Meeting of the Council on Thursday the 8th inst., George James Guthrie, Esq. F.R.S. was elected President; and Anthony White and John G. Andrews, Esquires, were elected Vice-Presidents of the College for the year ensuing.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 3d July, 1841.

Small Pox	22
Measles	13
Scarlatina	16
Whooping Cough	36
Croup	9
Thrush	1
Diarrhoea	2
Dysentery	1
Cholera	0
Influenza	0
Typhus	19
Erysipelas	1
Syphilis	1
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	131
Diseases of the Lungs, and other Organs of Respiration	233
Diseases of the Heart and Blood-vessels	17
Diseases of the Stomach, Liver, and other Organs of Digestion	60
Diseases of the Kidneys, &c.....	0
Childbed	3
Ovarian Dropsy	0
Diseases of Uterus, &c.	3
Rheumatism	2
Diseases of Joints, &c.	2
Ulcer	1
Fistula	1
Diseases of Skin, &c	0
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Old Age or Natural Decay.....	49
Deaths by Violence, Privation, or Intemperance	32
Causes not specified	2
Deaths from all Causes	751

METEOROLOGICAL JOURNAL.

*Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.*

July.		THERMOMETER.	BAROMETER.
Wednesday	7	from 51 to 67	29.82 to 29.62
Thursday	8	52 67	29.70 29.85
Friday	9	45 70	29.86 29.91
Saturday	10	45 68	29.87 29.53
Sunday	11	47 61	29.20 29.47
Monday	12	48 63	29.53 29.57
Tuesday	13	45 64	29.57 Stat.

Winds, W. and S.W.

On the 7th, morning clear, otherwise overcast; raining very heavily during the afternoon and evening. The 8th clear. The 9th, evening cloudy, with heavy thunder and rain in the N.W.; otherwise clear. The 10th, morning clear, afternoon cloudy, evening overcast, with rain. The 11th, general overcast; rain at times. The 12th, generally clear; a shower of rain about half-past one p.m. The 13th alternately clear and cloudy; frequent showers of rain during the day.

Rain fallen, .56 of an inch.

CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

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FRIDAY, JULY 23, 1841.

LECTURES

ON THE

PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XLIII.

*Epistaxis. Bronchocele: Cretinism: their
phenomena and probable causes.*

I YESTERDAY finished what I had to say respecting intermittent fever: its symptoms; its cause; and its cure. The subtle poison which produces it is thickly distributed over the fairest portions of the habitable globe: blighting human health, and shortening human life, more often, and to a far greater numerical amount, than any other single cause whatever. Known only by its noxious effects, holding out no signal of its presence, this unseen and treacherous enemy of our race has yet been tracked to its haunts and lurking-places, and detected in some of its habits. It was necessary, therefore, that I should enter somewhat fully into the history of the malaria, and show how it may sometimes be shunned, sometimes be averted; how also, in this climate at least, the effects it has already produced upon the human body may be successfully combated. But I shall not pursue, in further detail, the ravages committed by this invisible agent, and the remedies they require, in hotter and less favoured regions than our own. Of these, personally, I know nothing; and I must refer you, for information on such diseases, to authors who have seen and treated them: particularly to Drs. Lind, Jackson, Bancroft, Johnson, and Sir William Burnett.

It is customary, with writers and lecturers,
712.—XXVIII.

to pass from the consideration of ague to that of continued fever. A paroxysm of ague has been regarded as exhibiting a paradigm or sample of fever in general. But this has always appeared to me rather an ingenious refinement, than a useful matter of fact. Practically, I see nothing to be gained by the association. Intermittent fever, it is true, does often run, in hot climates, into the remittent, and the remittent into the continued form. But these are very different disorders from the continued fever with which, in these climates, and in this country, we have to do. Intermittent fever, and continued fever, as we see them, differ in their phenomena, in their cause, and in their treatment. They are alike inasmuch as they both are called fever, and both are attended, in some part or other of their course, with *pyrexia*: but, in essential symptoms, I have known many a compound fracture more like continued fever than any ague that we are likely to witness. I shall take up the subject of continued fever, then, in connection with the eruptive febrile diseases, with which it has many strong links of analogy; and I resume the consideration of the disorders that come within the province of the physician, according to their anatomical seat.

Epistaxis.—After what was stated of hæmorrhage in general, in an earlier part of the course, I hardly know whether *epistaxis* needs or deserves any formal notice. There are, however, some points relating to this simple, and commonly harmless hæmorrhage, which it may be worth while very briefly to touch upon. Sometimes it is a remedy; sometimes a warning; sometimes really in itself a disease. The readiness with which the mucous lining of the nasal passages pours forth blood is familiar to the experience of every school-boy; who “often wipes a bloody nose.” A slight blow, brisk exercise, a strong bodily effort, a fit of sneezing,

or the summer heat, is sufficient, in many boys, to make the nose bleed; and this facility of hæmorrhage furnishes, often, an index of some unnatural state of the circulation; and especially of undue fulness of the vessels of the head. But the import of this symptom is not always the same. Epistaxis may indeed be taken as affording an epitome of the various forms of hæmorrhage by exhalation. In childhood and early youth it is idiopathic, dependant upon active congestion, and probably arterial. It is nature's favourite mode of blood-letting at that period of life. In old age it is symptomatic, the result of passive or mechanical congestion, and probably venous. In some adult persons it happens periodically, and is habitual: and its *suspension*, rather than its *occurrence*, becomes a token of disease or of danger. In young women it is not seldom vicarious of suspended menstruation: in men it is apt to take the place of hæmorrhoids. Lastly, it may proceed from disease in the nares themselves; or form a part of a more general hæmorrhagic disorder.

It is unnecessary to go at length into the phenomena of epistaxis. The main phenomenon becomes obvious at once both to the patient and to those around him: and the accessory and incidental circumstances are easily discoverable when the attention is aroused to them by the sight of the blood. Usually the blood flows *guttatim*; in a succession of drops: but these may follow each other so fast as to constitute a little stream. Sometimes a few drops only fall; sometimes several pints are lost. A moderate hæmorrhage of this kind is generally succeeded by a sense of relief and refreshment. A large efflux of blood may cause pallor, faintness, debility, exhaustion, even death.

Active idiopathic epistaxis, as it occurs in children, is almost always salutary, and may be left to work its own cure. When it runs into excess, or is too often repeated, it may be checked by applying cold water to the forehead and bridge of the nose. The sudden contact of some cold substance with a distant part of the surface of the body will often have the effect of restraining the hæmorrhage: apparently by producing a general and sympathetic constriction of the superficial blood-vessels. This is doubtless a reflex phenomenon. The nursery remedy is to slip a cold key down the child's neck, between its back and its clothes. The aspersion of cold water is still better. Besides these external appliances, cooling laxatives should be given: and if the bleeding proves obstinate, some astringent internal remedy may be thought proper. I have, myself, hitherto found none so efficacious as the acetate of lead. But I have been recently informed by Dr. Latham that his experience has led him to trust much to *mercury* in the management

of epistaxis; and that the same indications have governed him, in adapting its use to this form of hæmorrhage, as serve to guide him in cases of inflammation. Thus, when the hæmorrhage has been profuse and frequent, and moderate depletion by blood-letting, purgatives, &c. has not arrested it, he has brought the constitution rapidly under the influence of mercury; and as soon as the mouth was sore, the hæmorrhage has ceased, not a drop more of blood has been lost. Again, when the epistaxis has been, not in large quantity, but habitual or frequently recurring, without any excess of vascular action, or any other apparent ailment in the constitution at large, Dr. Latham has often cured his patient by a moderate salivation, gradually induced, and continued for a few weeks.

In conversing with Dr. Southey on the same subject, I find that he also has been taught by experience to rely upon mercury as almost a specific remedy for obstinate hæmorrhage, occurring under similar conditions, from whatever organ of the body it may proceed.

This plan of treatment it is therefore my purpose to prove, as future opportunity may permit.

When epistaxis *begins* to show itself in advanced life, it is a symptom which cannot safely be neglected: for it indicates that the veins of the head are loaded. It implies a morbid condition that requires to be redressed. You will look for disease of the heart—or for threatenings of apoplexy—and take your measures accordingly. The blood-vessels which ramify upon and beneath the pituitary membrane, communicate by indirect inosculation with the veins and sinuses of the skull, as well as with the jugular veins. You see, therefore, how it is that hæmorrhage from this membrane may perform the office of a safety-valve, and protect the important organ within the cranium from impending mischief.

On the other hand, when epistaxis, which is known to have been habitual, fails to recur at or about the usual periods, you will look, with a jealous care, into your patient's state, and watch for and obviate any tendency to plethora capitis.

When epistaxis forms a part of more general hæmorrhagic disease—as when it occurs among other symptoms of purpura—its treatment merges in that of the whole malady.

In any case, if the flow of blood be excessive, and cannot be restrained by the ordinary remedies, but is exhausting the patient's strength, it becomes an absolute disease: and it will be requisite to staunch the blood by manual expedients.

These consist in stopping the bleeding orifices mechanically; which is most effec-

tually to be done by plugging the cavity. A dossil of lint must be carefully inserted into the bleeding nostril. Its mechanical effect, which is pressure, may be chemically aided by first wetting the lint with a saturated solution of alum. The mode of introducing these plugs it is the business of the surgeon—and not mine—to teach. The operation is not a very comfortable one either to bear or to perform.

Bronchocele.—Before we trace this mucous membrane downwards, through the mouth, to the inside of the throat, let me turn your attention to a singular disorder which may be deemed external, for it is scarcely more than skin deep: I mean that enlarged state of the thyreoid gland to which the name of *bronchocele* has been given. This word is not merely derived from the Greek, but was used by the Greek writers in the same sense in which we now employ it. In Switzerland, where it is very common, and in France, the complaint is called *goitre*; a corruption, it is believed, of the Latin “guttur,” the throat. It is known in England as the *Derbyshire neck*; from its frequent occurrence in that county.

The term bronchocele has been sometimes applied indiscriminately to all protuberances or swellings in front of the throat; or, at any rate, to all enlargements of the thyreoid gland; whereas it should be restricted to *hypertrophy* of that part: an exaggeration of its natural structure, with augmentation of its volume. The texture of the gland becomes coarser; its blood-vessels grow larger and more numerous; its cells are magnified, and filled with a thick, viscid secretion. It usually presents a soft, smooth, elastic tumor, which is neither painful, nor tender, nor discoloured. The lobes of the gland become more obvious. Sometimes the whole tumor is irregularly lobulated: sometimes the exact form and relative proportions of the gland are preserved, each lobe and portion being equally increased in size. Occasionally there is a soft uniform or irregular swelling, without much distinction of parts. Alibert states that the right lobe is more frequently enlarged than the left. Mr. Rickwood found it so in every instance of bronchocele that came under his notice in the neighbourhood of Horsham.

Unless the tumor be very large, it follows all the motions of the larynx: and this is a point of considerable importance whenever the diagnosis is at all doubtful. It is just possible that an enlarged lymphatic gland, or an encysted tumor in the neighbourhood of the larynx, or even a collection of pus thereabouts, might, in some degree, embarrass the diagnosis. But swellings of this accidental kind may be ascertained, in general, if the head and neck be placed in

different successive positions, to be unconnected with the larynx: and they do not follow its up and down movements when the act of deglutition is performed.

It is of importance to know, also, that the gland itself is subject to different kinds of enlargement. It may swell from inflammation, chronic or acute: and then it will be hard, and tender, and painful. But it does not seem very prone to inflame; and probably Dr. Copland is right in his opinion that inflammation occurs spontaneously in this organ in scrofulous persons only. Baillie and Alibert speak of it as being occasionally the seat of cancer; but that must be very rare. Sometimes cartilaginous or ossific deposits take place in the gland. It is necessary, I say, to be aware of these circumstances, and to distinguish one kind of thyreoid tumor from another: for some of the morbid changes just referred to are clearly beyond the power of any *medicine* to remove; and if all forms of enlargement incidental to this part are lumped together under one common name of bronchocele, we shall be liable to arrive at false conclusions concerning the power of remedies over that disease.

Bronchocele is not, *in itself*, a painful disorder; nor does it taint the system, or affect the constitution in any way. It has no character of malignancy about it. It is always, however, a deformity; and, by its mechanical effects, that is, by its weight when large, and by the pressure it exercises on contiguous parts, it may occasion great distress, and suffering, and even death itself. The size, and the effects, of the tumor both vary much in different cases; but its *injurious* effects are not always, though they are generally, in proportion to its bulk. Sometimes there is no more than a slight fulness of the throat, which some persons, I believe, think rather graceful than otherwise. Now and then, the swelling, after its first commencement, develops itself with great rapidity; but its ordinary progress is slow. It often continues for months, or years, without reaching any extreme or very troublesome magnitude. Sometimes it remains stationary for a considerable time, and then *suddenly* increases, without any apparent cause. The worst effects of bronchocele are its interference with the circulation, and with respiration. By its pressure it may obstruct the free descent of the blood through the veins of the neck, and give rise to headache, giddiness, noise in the ears, confusion of thought, and a turgid condition of the head and face. Or, by pressing upon the windpipe, it may cause hoarseness, wheezing, and dyspnoea. It may even impede deglutition. But these effects, I say, do not depend altogether on the actual size of the tumor. A very large *goître* may produce no

other inconvenience than what results from its weight and its unseemly appearance. It may surround all the front and sides of the neck like a thick collar, and rise as high as the ears; or it may hang down, in a pendulous lump, and be supported upon the chest. Nay, the tumor is said to descend, in some rare instances, so low as to be in contact with the abdomen: and Alibert mentions one case in which the swelling was of a tapering cylindrical shape, and reached to the middle of the thigh. On the other hand, a small tumor, not bigger than one's fist, especially if it happens to occupy the central portion, or what is called the isthmus, of the gland, may so press inwards upon the trachea as materially to hinder the breathing, and even to threaten suffocation. A pupil now attending the hospital has informed me of a case, which he himself saw, of death produced by the encroachment of a bronchocele; not so much, however, from suffocation as from starvation: for the swelling encircled the trachea, and came at last to press so much upon the woman's oesophagus, that she could not get food into her stomach. I suppose that the reason of these differences may be sometimes found in the manner in which the tumor grows, and in its relative situations. When it is bound down by the muscles of the neck, it presses, as it continues to enlarge, upon the parts behind it. When it is not so confined, the skin readily yields, and the entire growth of the tumor takes place anteriorly.

This disease is much more common in women than in men. Indeed we seldom see it, in this country, except in females. Yet I happen to have an example of it now (December 1837) in a male among my patients in the hospital. Dr. Andrew Crawford states that forty-nine cases were admitted into the Hampshire County Hospital, in ten years, and forty-eight of these were in women. Of seventy patients treated in the Chichester Infirmary in nine years, two only were males, and they were boys of a very feeble and feminine habit, and backward for their years. Among one hundred and sixteen patients of Dr. Manson, fifteen were men. Taking an average from these three lists, we have one male for twelve females. It is well to bear in mind that our fashion of dress renders a small bronchocele much more noticeable, much less easily concealed, in women than in men. In the former the swelling has been known to come on, or at any rate to increase rapidly, during their confinement in child-bed: and it is frequently observed to undergo a temporary enlargement at the menstrual period. Dr. Copland has seldom met with an instance in the female, unconnected with some kind of irregularity in the catamenial discharge, or disorder of the uterine functions; and he

never saw a case in which the disease made its appearance before the period of commencing puberty. In Switzerland, however, and in some parts of India, where the complaint is much more prevalent than here, the proportion of males affected is greater; and it begins, often, prior to the age of puberty, in both sexes. It seldom shows itself earlier than the age of eight or ten. Dr. Elliotson states, indeed, that he himself, when in Switzerland, saw goitre in a little boy only four years old; and the natives told him that it rarely made its appearance before the age of six. But children have been *born* goitrous. M. Godelle, physician to the hospital at Soissons, had a preparation of the body of an infant, which only lived a few hours, and which came into the world with a goitre; the mother being affected with the same disease. A case is mentioned in the *London Medical Repository* of a child born in Derbyshire with bronchocele of considerable size. The disease, therefore, undoubtedly may be *congenital*: and one of the facts I have just mentioned points to the question of its being *hereditary*. It is said to be so; and there is much probability in favour of that opinion. Children born of goitrous parents often have goitre. But that, you will say, may depend upon their being in the same place, and exposed to the same causes, which produced bronchocele in the mother or father. Dr. Crawford states, however, that he knew a woman, with goitre, whose grand-mother, father, paternal aunt, and cousins, also had it; although they did *not* all live in the same place, and no other person in their respective neighbourhoods was affected by the disease.

However, if the disease be, as it probably is, sometimes hereditary, in the sense in which I formerly explained that term, there can be no doubt that it is often *acquired*.

In the first place, bronchocele is *endemic*—prevalent in certain localities, and scarcely occurring elsewhere. And persons who, being previously well, go to live in those localities, often become affected with the complaint: and persons who migrate *from* those localities, having the complaint upon them, sometimes get rid of it by the mere change of residence. The physical circumstances of the places thus selected by the disease have been studied with the natural hope of discovering what the cause may be of an effect so singular. Some morbid quality of the *air* was long suspected. The habitats of the unknown cause of bronchocele appeared at first sight to be very much like those of the malaria. Goitre abounds in the hollows of many mountainous districts; among the Alps, for example, and in the Pyrenees. This was notorious to the ancients. Juvenal asks—

Quis tumidum guttur miratur in Alpibus?

And it is in the deep, close, and humid valleys of Switzerland, which lie at the feet of, and between, high mountains, that bronchocele is most common. Several writers, who have personally investigated this subject in places where goitre is rife, concur in the belief that it depends upon insalubrity of the air, arising from the peculiarities of the situation. They affirm that it is most frequent in low, damp, confined situations, where the stagnant atmosphere is seldom stirred by wholesome breezes; and where the sun, in summer, has great power. Dr. James Johnson remarks, "We find in the Valais (one of the Swiss Cantons) and in the lower gorges or ravines that open on its sides, both cretinism and bronchocele in the most intense degrees. As we ascend the neighbouring mountains, cretinism disappears, and goitre only is observed. And when we get to a certain altitude, both maladies vanish." Dr. Reeve, again, states that "all the cretins he saw were in adjoining houses in the little village called La Batia, situated in a narrow corner of the valley, the houses being built up under ledges of the rocks, and all of them very filthy, very close, very hot and miserable habitations. In villages situated higher up the mountains, no cretins are to be seen."

Cretinism.—The *cretinism* mentioned in these quotations is a strange and melancholy disease: a sort of idiocy, accompanied by (and doubtless dependent upon) deformity and imperfection of the bodily organs. The mental affection exists in all degrees, from mere obtuseness of thought and purpose, to the complete obliteration of intelligence. Many of the cretins are incapable of articulate speech; some are blind, some deaf, and others labour under all these privations. They are mostly dwarfish in stature; with large heads, wide vacant features, and goggle eyes, short crooked limbs, flabby muscles, and tumid bellies. The worst of them are insensible to the decencies of nature, and obey, without shame or restraint, every animal impulse. In no other class of mortals is the impress of humanity so pitifully defaced.

More recent and extensive observation of the localities infested by goitre have rendered it improbable that the disease derives its origin from any deleterious properties of the air. Certainly it is not owing to any thing that is common to *all* mountainous countries. Some parts of Switzerland are free from it. So are the Highlands of Scotland. It is met with also in flat situations—as in Norfolk. I have seen several cases of it in Cambridge-shire, which is a *very* flat country. In one village in particular, about five miles from Cambridge, it is extremely common. There are some striking facts collected by the celebrated and philosophic Humboldt, which go

to shew that the prevalence of bronchocele does not depend on any particular configuration of the surface of the earth, nor on any peculiar condition of the atmosphere. He tells us that in South America bronchocele is met with, both in the upper and in the lower course of the Magdalen river; and in the flat high country of Bogota, 6000 feet above the bed of the river. The first of these regions is a thick forest; while the second and third present a soil destitute of vegetation. The first and third are exceedingly damp: the second peculiarly dry. In the first the air is stagnant; in the second and third the winds are impetuous. In the first two the thermometer keeps up all the year at 22 or 23 degrees of the centigrade scale: in the third it ranges between 4 degrees and 17.

The researches of Mr. M'Clelland, in India, lead to the same conclusion. He found goitre extremely frequent in one portion of the district which he surveyed, while the other portion was almost exempt from the complaint, "although an equality of moral as well as physical circumstances appeared to affect the whole. The external alpine characters of the province are the same in every part, the inhabitants all belong to the same tribes of Hindoos, and are subject to fewer irregularities in their mode of life than any other people in the world."

The different localities of the villages, in the portion where goitre was *not* prevalent, he describes as being as diverse as can well be imagined. "Some are erected on narrow ridges, others in deep vallies, surrounded by abrupt and lofty mountains; others on rugged declivities between lofty peaks on one side, and dark ravines on the other, into some of which the sun can scarcely penetrate. The different altitudes of these villages vary from 2000 to 6000 feet."

Facts of this kind have turned the attention of scientific inquirers towards the only other obvious source to which the disorder could, with probability, be attributed: viz. the quality of the *water* used for drinking. Wherever goitre prevails, the popular belief assigns it to the water, as a cause: and the more accurately the search is prosecuted, the more strength and likelihood does this supposition acquire. Its very universality is a presumption in its favour. The disease was formerly ascribed to the use of *snow* water: a notion which originated, I imagine, in its frequent occurrence in alpine regions. But the people in almost *all* the valleys of Switzerland drink the water that comes from the Glaciers; while bronchocele is known in *some* of the valleys only. It prevails also in certain spots where pump water is used, and *there* the people accuse the *pump* water of producing it. Besides, goitre occurs in other countries, where the snow never lies, as in Derbyshire; and even in Sumatra,

where there is *no* snow. Dr. Bally, a native of a goitrous district in Switzerland, believes that bronchocele is caused by certain *waters*, which issue from the hollows of rocks, trickle along crevices of the mountains, or rise from the bowels of the earth. And in support of that opinion he refers to some fountains in his own neighbourhood, the drinking of the waters of which will produce, or augment goitrous swellings, in eight or ten days. Such of the inhabitants as avoid these waters are free, he says, from goitre or cretinism. In Captain Franklin's narrative of his expedition to the shores of the Polar sea, there is the following statement, made by his fellow traveller Dr. Richardson:—"Bronchocele or goitre is a common disorder at Edmonton. I examined several of the inhabitants afflicted with it, and endeavoured to obtain every information on the subject from the most authentic sources. The following facts may be depended upon:—The disorder attacks those only who drink from the *water* of the (Saskatchanan) *river*. It is indeed, in its worst state, confined almost entirely to the half-breed women and children who reside constantly at the fort, and make use of river water, drawn, in winter, through a hole made in the ice. The men, from being often from home on their journeys through the plain, where their drink is *melted snow*, are less affected: and if any of them exhibit during the winter some incipient symptoms of the complaint, the annual summer voyage to the sea-coast generally effects a cure. The natives, who confine themselves to *snow water* in the winter, and drink of the small rivulets which flow through the plains in the summer, are exempt from attacks of this disease. A residence of a single year at Edmonton is sufficient to render a family bronchocelous. Many of the goitres acquire great size. Burnt sponge has been tried, and found to remove the disease; but an exposure to the same cause immediately reproduces it. A great proportion of the children of the women who have goitres are born idiots, with large heads, and the other distinguishing marks of cretins. I could not learn whether it was necessary that both parents should have goitres to produce cretin children."

We are able to go even a step farther, and to announce a probable conjecture as to the specific quality of the suspected water. Bronchocele is very prevalent in Nottingham and its neighbourhood; and the vulgar there ascribe it (so Dr. Manson informs us) to the *hardness* of the water. You know that the rough practical distinction between soft and hard water is that the former *dissolves* soap, while the latter *decomposes* it. The hardness is generally occasioned by the presence either of *sulphate of lime*, or of *carbonate of lime*. In the one case the remedy

is to mix the carbonate of an alkali with the water; in the other you simply boil it. Now the well water in and about Nottingham is more or less hard, and unfit for the purpose of washing. Dr. Coindet, of Geneva, declares that the use of hard or pump water in the lower streets of that town brings on the goitre very speedily. At Cluses, on the Arve, numerous cretins and goitrous persons are seen in the streets: lofty cliffs of limestone tower over the town, and through its caverns copious streams of water find a passage. The soil in the neighbourhood of Edmonton was found by Dr. Richardson to be calcareous, and to contain numerous fragments of magnesian limestone. In a *Treatise on English Bronchocele*, very recently published, Dr. Inglis states his belief that the presence of magnesian limestone always implies the co-existence of the disease. "Take (he says) that ridge of magnesian limestone running from north to south through the centre of Yorkshire, and margining the shires of Derby and Nottingham. All along that line we have goitre to a very great extent; whereas, on our diverging to either side, the disease is found to diminish."

These scattered indications that the hurtful quality of the water is somehow derived from its contact with limestone rocks, receive a powerful corroboration from the result of Mr. M'Clelland's minute and valuable inquiries, which were carried on in the province of Kemaon, south of the Himalayan mountains. I have not been able to obtain his book; what I am about to tell you I take from a full and instructive notice of it in the fifteenth number of the *British and Foreign Medical Review*. Mr. M'Clelland finding goitre very abundant (as I mentioned before) in one great section of a district, and almost entirely absent from another section, set himself to find out in what other particulars these sections were distinguished from each other. And he ascertained that they agreed perfectly in external aspect, altitude, and climatology, but differed remarkably in their geognostic relations: "and this distinction was even traced down to the very villages in which the disease is found, with such perfect nicety, as to enable one almost to predict *à priori*, on examining the rocks of a neighbourhood, whether the inhabitants are affected with goitre or not."

It would be impossible for me to give you even an abstract of Mr. M'Clelland's numerous observations; but I select one or two striking instances in favour of his opinion that the endemic prevalence of goitre is connected with the use of water impregnated with calcareous salts.

One extremity of the long village Deota, which occupies half a mile of the foot of Durge mountain, is inhabited by Brahmins; the other by Rajpoots and Domes. Of the

first caste there are about twenty persons, all of whom are free from goitre. There are forty of the second, and two-thirds are affected, more or less. Of the third caste, forty-six in number, nearly the whole are goitrous. "To what cause can we ascribe the immunity of one caste of the inhabitants of this village, and the almost universal affection of the other two castes? They are all alike well fed, and have little toil; their land producing the requisites of life almost without labour. Difference of caste does not here imply a difference of pecuniary circumstances, and consequently of the comforts of life. In these respects the three castes in this village are on perfect equality. Nor will hereditary predisposition acquired by intermarriages be sufficient to explain the interesting fact: for the affected parties are confined to the Rajpoots and Domes, who cannot intermarry, while the Brahmins and Rajpoots may. The village is raised about 100 feet above the level of the valley; and the mountain at the foot of which it is situated, rises with a gentle slope, and is not, in this vicinity, at all rugged. It is chiefly composed of transition limestone, and the village is erected on a conglomerated rock, composed of calcareous tuff, inclosing fragments of other rocks. There is a spring in the valley, about 100 yards from the village, bearing on its first appearance the character of a mineral spring. The water bursts forth with strong ebullition, in the quantity of at least forty gallons in a minute, and agglutinates the sand and gravel by which it is surrounded, by the deposition of calcareous tuff. The temperature and quantity of the water is the same at all seasons. The former inhabitants of this village, aware perhaps of the noxious effects of the spring, had an aqueduct formed, by which water is conveyed into the Brahmin portion of the village from a distant source. The aqueduct having been suffered to get out of repair, the quantity of water it transmits is reserved exclusively for the Brahmins; except during the rainy season, when, the water being plentiful, the Rajpoots also use that of the aqueduct; but the Domes have no alternative at any season but to use the water from the spring."

The valley of Baribice is elevated 4000 feet above the sea. Its eastern extremity is composed of *clayslate*, and in five villages, containing 152 inhabitants, there is not one goitre. The other extremity of the valley is partly composed of limestone; and of 192 inhabitants, distributed in six villages, 70 are affected with goitre: but Ducyong, one of these villages, supplied with water from clayslate, has not a single case of the disease; while Ager, only half a mile distant, and containing 50 inhabitants, has no less than 40; and of that number 20 are cretins. They use the water that issues from an old

copper mine in limestone, and which contains carbonate of lime, and of soda, but no sulphate.

Mr. M'Clelland affirms that in the course of his personal inquiries, which extended over 1000 square miles, and which were prosecuted without regard to any theory, no instance occurred in which goitre prevailed to any extent where the villages were not situated on or close to limestone rocks.

ON
SOME POINTS IN THE PATHOLOGY
OF THE CIRCULATION.

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*Being the substance of the Gulstonian
Lectures for 1841.*

[Continued from p. 642.]

INFLAMMATION is the most important of morbid processes, because the most frequent of those which are quite peculiar to disease. It is also more complicated than either congestion or determination of blood, and its ends and results are more varied. It is at once the process of destruction and construction, of decay and reparation, of separation and union, of exaltation and abolition of the vital properties. It constitutes, as it were, the artillery, the war apparatus, of the body; capable of acting on the offensive or defensive; attacking parts, and protecting them; and pressing into its service all the various functions and properties in the work of disturbance, demolition, or restitution to which it tends.

Now with such diversified and opposite results, who can wonder that inflammation should be a very complex process, or that its pathology should doubly partake of the obscurities hanging about the physiology of the parts concerned? To give even an abstract of the observations and opinions of writers on this subject would far exceed the limits allotted to these lectures. I shall therefore only state those leading points which are established by the concurrent testimony of observers, or which I have confirmed by my own examinations. We shall then proceed to consider how far these facts may be explained by reference to known physical and vital properties.

The simplest causes of inflammation are irritants, which have been divided

into the chemical, mechanical, and vital. It is better to exclude the chemical irritants, as in their operation it is difficult to distinguish between their mere chemical effects on the tissues and fluids, and their effects on the vital properties of these parts. If we examine the first effect of a mechanical irritant, as a thorn; or of a vital irritant, as a grain of capsicum or cantharis, on a living tissue, we shall find that it is on the sensitive nerves, causing itching, uneasiness, or pain. Is this a preliminary essential to inflammation? Certainly not; for we find that inflammation may be induced in parts that have lost all sensation, whether by disease, or in animals by dividing the nerves. I exclude the consideration of the organic or ganglionic nerves (supposed by some to be mainly concerned in all vital processes), not because I deny their influence, but because this influence has not been positively established; and if it exist, it is not separate from the properties of the tissues themselves. So too we know that many inflammations are latent, without pain or other sensation at their commencement; these commonly arise from defective secretion, or other causes of congestion, and not from direct irritation: and where pain is present, it bears little proportion to the degree of the inflammation.

We are thus led to place the first seat, as well as the process of inflammation, in the vessels themselves. What is the condition of these vessels? That they are greatly enlarged is obvious from the redness. Wherein then does it differ from congestion, in which also the vessels are enlarged? Not only in the accompanying and resulting phenomena, the increased heat, sensibility, exalted and altered functions of the part, &c. but also in the ascertained state of the vessels themselves. The flow of blood to and from an inflamed part is positively increased. That there is increased flow to the part is obvious from the enlargement and increased throbbing of the arteries leading to it; a fact familiar in inflammations of limbs, and established by measurement in the experiments of Dr. Alison. That the flow from an inflamed part is increased is proved by the well-known experiment of Mr. Lawrence, in which venesection being performed at the same time and in the

same manner in both arms of a patient, much the greatest quantity of blood flowed from the arm the hand of which was inflamed.

What, then, is inflammation more than determination of blood, in which there is an increased flow as well as quantity? Inflammation includes determination of blood. It has been observed by Gendrin, and well insisted on by my colleague, Professor Cooper, the vessels in the neighbourhood of an inflamed part are the channels of an increased flow; and I would add, there is also an increased flow towards the whole inflamed part; determination of blood around it and to it. But this is not all. The tension and swelling indicate accumulation in the part; and altered products shew something more than merely increased action.

The microscope has supplied much of the desired information. The researches of Thomson, Hastings, Kaltenbrunner, Gendrin, and Marshall Hall, clearly establish the fact that there is more or less *obstruction* to the passage of the blood through the vessels most inflamed. Hence in these vessels the blood is seen to move more slowly, and gradually accumulating, to make the vessels larger and more tortuous. As the motion diminishes, it becomes oscillatory rather than progressive (Kaltenbrunner); the blood becomes darker, and, at last, stagnates, the particles losing their shape, the serum and some of the colourless globules being effused beyond the vessels (Gendrin).

The chief point, then, in which inflammation differs from determination of blood, is in the obstruction of some of the vessels. This answers to our definition, *too much blood in a part; the motion of that blood being in some parts increased, in others diminished.*

But what is the cause of the obstruction? Certainly not spasm, or contraction of any kind, for the vessels are dilated. The difficulty of accounting for obstruction through dilated vessels has been a leading argument used by those who maintain that capillary circulation and its varieties depend mainly on certain vital attractions and repulsions between the vessels and the blood. But, as in the healthy circulation, so in diseased, we should be slow to assume the existence of any new powers, until we are assured that those

already known are insufficient. The only adequate explanation of the cause of obstruction in inflamed capillaries, is that of Dr. Marshall Hall. He supposes that it is caused by the adhesion of the blood globules to the sides of the vessels; and this adhesion he ascribes to the direct influence of the irritant or inflaming cause on the interior of the vessels. That something like this is a chief cause of the obstruction, and particularly of its continuance, will appear in the sequel; but there is another cause, more purely physical, which also co-operates.

In speaking of determination of the blood, I had occasion to mention that the force of a palpitating heart is sometimes so expended on an aorta temporarily dilated, that the pulse at the wrist is scarcely to be felt. These and analogous facts prove that the propulsive power of the heart is not proportioned to the violence of its contractions, even when these distend a considerable portion of the aorta; but rather to a due relation between these contractions, and the size and elasticity of the arterial tubes. During the palpitation, much of the force of the heart is lost, and much is engrossed by the branches nearest to the organ, whilst little reaches distant arteries. A changed condition of other parts of the vascular system may cause a like unequal distribution of force. The natural proportion and elasticity of terminal arteries and capillaries best adapts them for transmitting the moving blood. If they become dilated, tortuous, inelastic, they obstruct the passage of the blood, which therefore moves but slowly, or stagnates, whilst the main current passes by other anastomosing channels.

As the physical principle, of which this is an exemplification, is not sufficiently recognized, I will relate some experiments which show how much more readily fluids are propelled through small rigid tubes than through those which are large and inelastic.

To one of Read's enema syringes was adapted a tube with two arms; to one arm was attached a brass tube two feet long, having several right angles in its course; to the other arm was tied a portion of rabbits' intestine, four feet long, of caliber, when distended with water, double that of the brass tube. The intestine was laid in curves and

coils, avoiding angles and crossings, which might obliterate the canal. The discharging end of both tubes was raised to the same height of three or four inches; that of the intestine being kept open by a short tube of metal. The tubes were then both filled by successive strokes of the piston; and when they both began to discharge, the quantity received from each in a given number of strokes was ascertained. Without entering into details, it may be stated that from two to five times the quantity of water was discharged by the small metal tube more than by the longer and larger membranous tube; the difference being greatest when the strokes of the piston were most forcible and sudden, although the syringe end of the intestine was much swelled at each stroke. The difference was also much increased by raising the discharging ends higher; and when both were raised eight or ten inches the gut ceased to discharge; each stroke only raising the column of water in it; but this subsiding again by discharging itself backwards through the metal tube without rising high enough to overflow. On increasing the force of the stroke, the part of intestine nearest to the syringe burst.

The experiment was repeated with a metal tube two feet eight inches long, with a bore three-eighths of an inch, and ten feet six inches of dogs' intestine, the diameter of which when distended was about three-fourths of an inch. The quantity of water passing by the intestine varied from one-tenth to one-fifth of that passed by the smaller metal tube.

The experiment was repeated with equal lengths (two feet eight inches) of tube and intestine. This reduced the difference to as three to one in favour of the narrow metal tubes.

Although the differences between the tubes used in these experiments are much greater than those between inflamed and healthy vessels, yet they are the same in kind, and prove the general fact that the propulsion of liquids through vessels is proportioned not to the propelling force nor to the size of the vessel, but to a due relation between these, and especially to a due tone or resistance of the walls of the vessels.

Let us review the modifications of the capillary circulation in relation

to this principle. In determination of blood the arteries are enlarged, and so are the capillaries in due proportion: the circulation is therefore equally increased. In congestion the capillaries are enlarged without any increase in the arteries: the motion is therefore impaired; but still, being gentle, it diffuses itself through the mass, which moves slowly. But if to congested vessels you let in the force of enlarged arteries, or if to determination of blood (enlarged arteries) an atonic distension of the capillaries be added, then the propulsive power will be impaired; the blood will pulsate or oscillate in the distended vessels rather than pass through them; and the main current will pass by collateral anastomosing channels, which become the seat of simple determination or increased flow. This is just the state of things in the incipient stage of inflammation, and if either the capillaries do not speedily recover their tone, or the arteries do not contract, the blood in parts becomes stagnant, and coagulates; first in the venous portion of the capillaries, and the obstruction is confirmed. The arterial portions of the obstructed capillaries still remain exposed to a pulsative force from the enlarged artery, which continues to strain their coats and cause an oscillatory motion of their particles, but no passage. This is precisely what we see under the microscope.

But we have yet to inquire how it is that the blood coagulates in the most inflamed capillaries? Mere stagnation is insufficient, for we do not find coagulation to take place in the blood congested by venous obstruction. I think my observations supply an answer to this inquiry.

I have before mentioned that Poiseuille had observed the blood particles to get into the motionless layer of serum which intervenes between the moving blood and the walls of the blood-vessels; in fact, it was chiefly from the stagnation or slow motion of the globules that he inferred the existence of this layer. From his description we might be led to suppose that their presence there was a usual result of the particles being thrown out of the current, and that this commonly happens to every description of blood particle, both with and without colouring envelope.

I first examined the particles of the

motionless layer through the microscope of my friend Mr. Toynbee, and he then remarked that they were all round, and not elliptical. This remark I have subsequently verified in numerous examinations. The particles fixed in the motionless layer, and those that roll slowly along it, are spheroidal or lenticular, varying in size, but all considerably smaller than the elliptical blood disks which move in the axis of the vessels. I have never seen a solitary elliptical disk adhering to the sides of the vessel; and whenever one is arrested in its course, it is from its becoming hitched by one or more of the round globules. The round globules, on the other hand, are often seen either moving slowly on the circumference of the vessel, or fixed to its walls*.

But what appeared to me most remarkable, with regard to these smaller globules, was the great difference in their number under different circumstances. In young frogs, and in those much subjected to experiment, they are always present: but in healthy adult frogs, placed under the microscope with as little handling of the web as possible, there were few or none to be seen. I have watched for ten minutes at a time without seeing one. The motionless layer was very thin, but clear, and all the blood particles in the larger vessels seemed to move at the same rate of speed. By pressure of the finger on the web, partial stagnation was produced in many of the vessels; and when this yielded to the returning current, the walls of the vessels were seen studded with the small globules; whilst many others of the same kind rolled over them slowly in the direction of the current. I have before mentioned that a similar result ensued after the web had been stimulated by capsicum or an essential oil. Even during the rapid flow following these applications, minute globules could be seen creeping slowly along the transparent outline of the larger vessels; and as the arteries contracted, and the flow through the other vessels became less rapid, the number of these globules increased, their motion became slower, and many seemed to stick on the sides of the vessels. If the stimulus used was rather strong, or long applied, the number of sticking globules

* The microscopic observations have been made since the lectures were delivered.

was so great as to prevent the red particles from passing; and these or their fragments becoming impacted in the interstices, gave the obstructed vessels a uniform red colour. When the stimulation was equally applied to the web, the stagnation usually took place first in some of those anastomosing veins in which the current is naturally slow and varying in direction: but when an essential oil was used, the stagnation speedily ensued at the point of its application: in fact, unless very minute quantities were used, the stagnation was almost immediate and extensive.

These observations in part confirm the opinion of Dr. Marshall Hall, that the obstructed state of inflamed capillaries is due to the adhesion of globules of the blood to the vessels: and they seem to shew further, that the globules which thus adhere are either formed in the blood, or detached from the red corpuscles, under the influence of irritants or other disturbing agents.

The complete obstruction of the capillaries in this way by coagulation takes place to a greater or less extent in all cases of severe inflammation of the frog's web; but there are slighter kinds of increased vascularity in which there is no obstruction, but continued enlargement of the capillaries and veins, as well as of the arteries: this might be called simple determination of blood; but it differs from that recently produced, in the motion in the capillaries and veins being slower, and in the vast number of colourless globules seen moving slowly in them. I suspect that this corresponds with the low or subacute variety of inflammation; but the whole subject deserves a fuller investigation.

The effect of all the varieties of hyperæmia which we have now been considering, is to exhaust much of the circulating force conveyed by the arteries on the walls of the dilated capillaries, particularly their arterial portions; and their enlargement and tortuosity, the production of globules which adhere to their sides, and their entire obstruction by the same means, seem to be so many progressive expedients used by nature, to direct the force of the circulation to that part of the vessels by which the process of reparation is carried on.

OBSERVATIONS

ON THE

USE OF THE VECTIS, OR SINGLE-BLADE EXTRACTOR, IN DIFFICULT LABOURS.

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[Concluded from page 653.]

IN the preceding cases all the women* did well; not an untoward symptom occurred with the exception of the case of J. D., in which flooding supervened. Of the 22 vectis cases (in which are two cases of puerperal convulsions) two children were still born; in one only did the death appear clearly to depend on the duration and difficulty of the labour. In the nine forceps cases three children were still-born, two being cases of puerperal convulsions.—(See Table, next page.)

CASE I.— Taylor, sixth labour. Her midwife had sent for a surgeon in consequence of flooding, who, before my arrival, had left the woman.

I found her blanched, with a very feeble pulse, and wholly without pains. The os uteri was dilated, the liq. amnii discharged, and the head resting at the brim; the cord prolapsed through the os externum.

As there was no flooding, I tried the experiment of including the mass of cord in a muslin bag, and having carried it above the head, I brought with the vectis the head into the cavity, and in a quarter of an hour she was delivered of a dead child. The loss had been great, and her recovery was protracted.

CASE II.— Knowles was attended by a midwife in her seventh labour, October 25th, 1835. The pains had been throughout regular, and during the last three hours frequent and strong, but did not advance the head. I applied the vectis, and, co-operating with uterine contraction, the child was born with the third pain. Its weight was $9\frac{1}{2}$ lbs. avoirdupois.

CASE III.— Mary Bunn, æt. 33: first labour, October 1st, 1831. She sent for the midwife at two P.M. on Wednesday, who did not then remain with

* I may here observe, that no case of midwifery occurring in my own practice has required more than the ordinary management of women after delivery.

Table of Difficult Labours terminated by the Vectis from 1830 to 1840 inclusive.

No.	Name.	Age.	Labour.	Remarks.
1	Taylor	..	6th	Accidental flooding—prolapse of cord.
2	Knowles	...	7th	Weight of child (male) 9½ lbs. avoirdupois.
3	Bunn	43	1st	
4	Brunton	22	1st	
5	Mott	19	1st	
6	Howard	33	8th	Narrow inlet—conjugate diameter.
7		36	10th	
8	Marshall	26	2d	
9	Buttifiant	31	1st	Child (female), still-born.
10	Buttifiant	32	1st	
11	Hutchin	21	1st	Weight of child (male), 10lbs.
12	Spooner	35	7th	Previous labours, five instrumental; in another premature at the 7th month; in one child still-born.
13	Betts	...	6th	Weight of child, 10 lbs.; weight of placenta, 3½ lbs.
14	Cobb	33	1st	Child died next day; vomited meconium.
15	Wright	32	4th	Two previous labours instrumental; one by natural efforts.
16	E. F.	32	1st	Small pelvis, deformed at outlet.
17	Parker	37	12th	Previous labours, 8 tedious, 3 instrumental; narrow conjugate diameter.
18	Rushmere	24	1st	
19	Mansfield	34	6th	Previous labours, 4 tedious; children all small; one at 7th month; weight of child, 7¾ lbs.
20	Bunn	35	8th	Former labours, 2 instrumental; one at 6th month.
21	Wright	39	1st	Puerperal convulsions.
22	Mountain	24	1st	Puerperal convulsions.

her. On Friday, at four P.M., she was again called, and was with her through the night. At seven the next morning she left her, and on her return in the forenoon found the os uteri dilated, the liq. amnii discharged, and the pains strong. These continued with regularity, when I was requested to attend at six in the evening. I found that the head came down on the perineum during the pains; the vagina was dry, its orificium unyielding, and the perineum very rigid. During the last seven hours there had been no progress in the labour. I applied the vectis, and in less than half an hour the child was born.

CASE IV.—On December 30th, 1831, I was requested to visit Sophia Brunton, æt. 22, in her first labour. A midwife had been with her through the night. The os uteri had been fully dilated for eight hours, and the pains regular and of good strength. The head lay in the cavity of the pelvis, with the face to the right side; the right ear was felt behind the symphysis pubis; there was swelling of the soft parts in the pelvis, and of the labia. I applied the vectis, and after repeated powerful co-operation during

each uterine contraction, the head was brought through the os externum much elongated, and with a considerable scalp tumor.

CASE V.—Mott, æt. 19, first confinement. Symptoms of labour commenced on Sunday evening. At 3 A.M. she sent for the midwife, who found the os uteri dilated to 2½ inches, and the liq. amnii escaped. At 8 A.M. she sent for me. I learned that the head had scarcely stirred for the last five hours; that the pains, which had been good during the last hour, were less frequent, and broke off short. The vagina was hot, dry, and tender. Having used lard very freely, and awaited a few inefficient pains, I delivered by the vectis: the child still-born*.

CASE VI.—Elizabeth Howard, æt. 33, of short stature, but healthy and strong, was attended by a midwife in her eighth confinement, September 6th, 1833. None of her former labours had been good, lasting 24 hours and upwards; the child always resting high up. Although the pains had been vigorous

* I did not think the infant died in the birth. From the treatment of her husband, and the appearance of the infant, it is probable it died *in utero*.

my assistance. I applied the vectis, and regular for many hours, the labour did not advance. On examination, I found the head thrown forwards on the os pubis, the promontory of the sacrum projecting more than is found in a well-formed pelvis; the teguments of the belly were pendulous. The midwife had not, by position or by the binder, endeavoured to obviate the malposition of the head. I passed the vectis, and brought the head to the brim of the pelvis, and aided by steady traction during a few pains it cleared the inlet.

CASE VII.—In her next labour, she was attended by a midwife only. At her 10th labour I was again summoned to her assistance. Having brought down the head into the cavity of the pelvis I withdrew the vectis, and left the termination of the case to the natural efforts. The next was a foot presentation, and in the 12th and last the child was born after thirty hours' hard labour.

CASE VIII.—Marshall, æt. 26, second labour: she became uneasy at three o'clock in the morning of November 8, 1834. The midwife was called to her at 5 A.M. The liq. amnii came away at eleven A.M.: the uterine orifice was dilated to the diameter of $2\frac{1}{2}$ inches. At six P.M. I was requested to visit her: the os uteri was fully dilated: the head had not cleared the brim of the pelvis: the pains had been regular but short and inefficient: she complained of being weary, and in want of sleep. I gave her \mathfrak{mxx} . of laudanum, and left her. At ten o'clock I found she had slept, and that the pains had been renewed, but without effect. I delivered by the vectis.

CASE IX. — Buttifant, residing in Heigham, æt. 38, first labour, May 28, 1837. I was summoned on Saturday at 3 A.M. by the midwife, who had been with her since four in the afternoon of the day preceding. My patient began to be uneasy on the Tuesday, and passed a restless night, and had little or no sleep on Wednesday and Thursday. Slept in the intervals of pain through the Friday. At 7 P.M. the os uteri was fully dilated, the liq. amnii discharged, the vagina swollen and tender, the pains short and wholly inefficient, the presentation and position natural. I could just reach an ear at the symphysis pubis; the parietal bones were overlapping, the scalp tumor was considerable: she com-

plained much of cramp of the right thigh, and said she was worn out*. I passed the vectis, and for some time assisted, co-operating with the feeble uterine action, but the head, impacted by the swelling of the soft parts, made no advance. After a brief interval I re-applied the vectis, and at length the head was brought lower down: still its advance was trifling. I again withdrew the instrument, purposing to employ the forceps. The midwife now sat down at the bed-side: pains followed of rather more strength, but at longer intervals, during which she slept heavily. On awaking up she complained of thirst; the tongue was dry, pulse slow: the powers of her system were much exhausted, and could not much longer have continued the struggle. I proceeded to apply the forceps, but finding the head much lower I awaited two or three pains; but as these seemed unequal to accomplish the expulsion of the head, I helped it through the os externum with the vectis: the child still-born.

CASE X.—Buttifant, living at Trowse, æt. 32, first labour. The midwife was called to her on Sunday, but left, there being no symptoms of labour. She got but little rest, however, that night; the pains increased through the next day, and in the afternoon she summoned her midwife again: the labour proceeded slowly through the night. At six A.M. the os uteri was fully dilated, and the liq. amnii discharged. My assistance was requested by the midwife at 11 A.M.: the pains had been feeble since 6 o'clock. She expressed herself quite worn out: the foetal pulse was heard: the presentation and position were natural, passages relaxed and dilated, and only efficient pains required to terminate the labour. I watched them for half an hour; they were powerless, scarcely perceptible. Two proceedings were open to me: to secure by an opiate refreshing sleep and recruited powers, or to deliver at once. I preferred the latter course, and shortly delivered her with the vectis.

CASE XI.—Mary Hutchin, æt. 21: first labour, July 30, 1833. The pains had been regular and strong during six hours before the midwife requested

* There was very considerable swelling of the whole of the left arm, which followed the removal of a tumor from the breast and axillary margin.

and in half an hour she gave birth to a male child weighing 10lbs.

CASE XII.—Sarah Spooner, æt. 35, seventh labour, September 12, 1833. She had been delivered with the forceps by surgeons, in five previous labours: in one, she gave birth to a still-born child without assistance; in another, to an infant at seven months. The midwife had been with her six hours. I found the os uteri dilated to the diameter of two inches, the head resting at the brim. I could not ascertain its position by the hand passed per vaginam: the conjugate diameter was diminished. During the next two hours she vomited often; the pains were frequent and of good strength, but the head did not descend. An hour after the os uteri was dilated; one third part of the head had entered, and was fixed in the inlet. Having waited during several powerful pains, which failed to force the head into the cavity, I delivered her by the vectis, co-operating with each labour pain. The child (female), was born with a considerable depression on the left parietal bone.

CASE XIII.—Betts, 6th labour, June 15th, 1835. Had been in strong labour five hours, when I was summoned to her. The os uteri was fully dilated; the bladder and lower bowel empty. I delivered her quickly by the vectis of a child (male), weighing 10lbs.; weight of secundines, 3½lbs.

CASE XIV.—Mrs. C., æt. 38, a strong, healthy person, in her first labour, sent for me, May 9th, 1835. At four A.M. the os uteri, thin and soft, was dilated to two inches, and the liq. amnii discharged; the pains were quick and strong. At eight A.M. the head was coming down on the perinæum, which was firm, dense, and resisting. Warm moist cloths were applied, and lard freely used; the bowels had been relieved by castor oil. The perinæum not yielding in the least degree during the pains, I bled my patient at two P.M. During the preceding three hours the struggle had been severe, with but a brief pause between the labour throes. I now passed the vectis, and brought the head to bear forcibly during each contraction of the uterus upon the perinæum. At the fourth extractive effort the head suddenly cleared the perinæum, which did not spread out, but rather yielding

like a piece of caoutchouc, instantly resumed its dense resistance. The fourchette had not even suffered.

CASE XV.—Eleanor Wright, æt. 32, fourth labour, November 19th, 1835. Her two first labours were instrumental. In the third a midwife was with her. Her midwife went to her at eight P.M.: the liq. amnii had flowed some hours. At two A.M. (20th), she quitted, the pains having been through the night slow and feeble. At seven A.M. the midwife returned, and at ten my assistance was requested. After waiting three feeble actions of the uterus, I applied the vectis, and brought the head through the os externum. A considerable interval elapsed before the shoulders followed.

CASE XVI.—E. F., æt. 32, of small stature and delicate form: first labour. The midwife went to her on Friday at nine P.M. At ten A.M. on the following day I was summoned by her. The head had not come into the cavity; I could with difficulty tip the ear; the arch of the pubes admitted only the two fore-fingers. There was an unusual curvature of the sacrum, and the pelvis was small. There was swelling of the soft parts in the pelvis; scalp tumor considerable; the pains were good, but they failed to carry down the head. I delivered by the vectis without much difficulty.

CASE XVII.—Eliz. Parker, æt. 37, a tall, strong, and apparently well-formed woman, twelfth labour, October 6th, 1838. She told me all her labours had been slow; that a midwife was with her in the first six, who was detained two and three days together. In her 7th, 8th, and 9th, she was delivered by surgeons with instruments. The midwife who had requested my attendance had been with her twice before. She was called to her at nine A.M., and at five P.M. sent for me. Her report was, the os uteri had been dilated and the pains strong, with scarcely an interval of rest during the last four hours, but the labour was not more forward.

On examining, I found the conjugate diameter reduced by the projecting promontorium of the sacrum, and the parietal bones riding. The pains were violent, following each other rapidly, but, as she said, of no use to her. I applied the vectis, and co-operating with four uterine contractions the head cleared the brim, and passed during

the next pain through the os externum with the vectis applied, but without any further extractive effort being necessary. The child did not give signs of active life till early the next morning. I found the patient sitting up at the end of a fortnight.

CASE XVIII.—M. Rushmere, æt. 24, first labour, February 10th, 1838. At 4 A.M. symptoms of labour commenced, and pains continued through the day. The midwife went to her at 6 P.M. The pains increased in frequency and force as the night advanced, and at 3 A.M. (11th) became very strong. The os uteri had been dilated since midnight. As the labour had made but little progress I was requested to visit her at seven A.M. There was some heat and want of natural secretion: lard was used very freely. I remained with her two hours: the bladder and bowels having acted freely, I left her. On my return at eleven A.M. I found the increase of tumor to proceed from swelling of the scalp: the head was impacted. I immediately delivered her with the vectis.

She suffered from inflammation and discharge from the vagina for ten days after, in consequence of the pressure of the child's head.

CASE XIX.—Sarah Mansfield, æt. 34, sixth labour, July 26th, 1839. In her first confinement the surgeon was at her bed-side for eight hours; the next was premature: her other three children were small, and the labours tedious; she had miscarried once since. The midwife was called to her at four A.M.: the liq. amnii flowed at seven: the os uteri was dilated to two and a half inches, and the pains good, but the head did not descend into the cavity. At noon the uterine action was less powerful, but more prolonged, and without a perfect intermission, the uterus remaining high up in a state of tonic contraction. I was sent for at six P.M. I found the forehead presenting: the eye was distinctly felt: the os uteri not being fully dilated was compressed between the head and the bones of the pelvis. The conjugate diameter at the brim was narrow. The head was so wedged, that although the parietal bones were riding so as to afford a firm purchase, I could not stir it. There was much swelling of the scalp. She complained of great pain of body, said "she was worn out, and

was sure she should die." Having first carried up and secured from pressure the os uteri, I succeeded in bringing down the occiput with the vectis, elevating at the same time the forehead with my left hand, till the anterior fontanelle was to be felt opposite the left sacro-iliac synchondrosis. I now watched the effect of several pains, but there was no advance. She had not felt the child since eleven A.M., and said "it was dead." I had not a stethoscope with me. I now again applied the vectis over the face and chin, and after several pains accomplished the delivery at nine P.M. The child (male), remained in a state of suspended animation for some time. The weight was 7½ lbs. It was much larger than her former children.

CASE XX.—Bunn, æt. 35, eighth labour. Two of the former were instrumental, one premature.

When I was called to her by the midwife, the os uteri had been dilated, and the liq. amnii discharged five hours. The pains, which had been regular, had become frequent and feeble. I applied the vectis, and after three times co-operating with the uterine contraction, the child was born.

CASE XXI.—Wright, æt. 39; first labour: had miscarried at six months when in India some years before. Her midwife called in the afternoon, and advised her, as she complained of headache, to take a little spiced porter in the evening. She took it, and said she felt better. At 11 o'clock P.M. while preparing to go to bed, she had a convulsive fit. At 1 A.M. I was called in great haste to her; found her husband and six persons endeavouring to hold her forcibly down during a severe fit. There was no symptom of labour. At 8 A.M. I could with difficulty tip an ear above pubes, the head not having cleared the brim.

I delivered by the vectis: the child still-born. It gave no signs of active life till two hours and a half after its birth. I employed for half an hour the usual means, of bringing about respiration, and requested to have the child warmly covered, except its mouth, and laid carefully aside: two hours after I had left the room the child cried aloud.

CASE XXII.—At 11 o'clock P.M. February 29th, 1836, Mountain, æt. 24, in her first labour, became violently

convulsed : the midwife had been with her half an hour. When she got to her the os uteri was dilated, soft, and the vaginal passage relaxed. I delivered her of a living child at 2 A.M.

I have briefly sketched the two preceding cases, as it is my intention in my purposed continuation of midwifery cases to give them more circumstantially.

Table of Difficult Labours terminated by Forceps from 1831 to 1840 inclusive.

No.	Name.	Age.	Labour.	Sex.	Remarks.
1	Comar	37	8th	Female Still-born Male Female	Three previous instrumental labours.
2	Peak	20	1st		Labours all slow ; children large.
3	Fish	19	1st		
4	Browne	42	8th		
5	Davy	23	1st		
6	Cupper	39	13th	Male	Delivery five hours after the first fit ; puerp. convulsions. Weight of child nine pounds. Delivered seven hours after first fit ; puerp. convulsions.
7	Rice	41	1st	Female	
8	Day	23	1st	Male	
9	Lynes	20	1st	Still-born	
				Female	

CASE I.— — Comar, æt. 37, was attended by a midwife, in her eighth labour, Dec. 8, 1832. She had been delivered three times with instruments by different surgeons. At eight P.M. the midwife went to her, and at two in the morning following my assistance was required ; the os uteri was dilated, the liquor amnii discharged ; the head had not entered the cavity of pelvis ; the pains were regular, but not strong ; the head was impeded by a narrow outlet. I returned at nine A.M. There had been no pains during the last two hours. I delivered with the forceps.

Some interesting particulars of adhesion of the placenta over its entire uterine superficies, with contraction of the os uteri, which was overcome with extreme difficulty, I shall take another opportunity of recording.

CASE II.—Peak, æt. 20, first labour, October 1st, 1831. She was attended by a midwife, who reported the orifice of the womb had been open, and the waters off for ten hours : the vagina was dry, swollen, and tender. I delivered by the forceps. Considerable inflammation of the vagina followed.

CASE III.—Fish, æt. 19, was attended by a midwife in her first labour ; the head had made no advance for eight hours. I applied the forceps ; she was delivered of an infant, male, still-born.

CASE IV.—Browne, æt, 42, eighth labour, September 13th, 1834. The

midwife was called to her at three A.M. Her previous labours had been slow ; the children large. I was requested to visit her at five P.M. She had been in strong labour five hours. I delivered with the forceps : child (male) large.

CASE V.—J. D. æt. 23, first labour, September 18th, 1836, of delicate frame and of excitable temperament ; had suffered much mental anxiety and indisposition of body, with very flatulent and acid stomach and costive bowels. I was sent for at ten A.M. The three previous days she had felt pains, but there was no appearance of labour ; she had passed a restless night. The os uteri was thin, soft, and dilated to an inch ; the pelvis small, and the arch of the pubes narrow ; vaginal secretion scanty. At two P.M. the os uteri was dilated to two inches and a half. At half-past six o'clock in the evening the first stage of labour was completed, the os uteri being fully dilated, and the liquor amnii discharged : the head rested high up. During the next four hours the pains were regular and vigorous, but the head made no sensible advance. Acting with the vectis on the occiput I failed to bring down the head, but having carried it round to the pubes, and fixed it over face, ear, and chin, I employed considerable extractive force, and the head descended and was engaged in the cavity of the pelvis. I withdrew the vectis, and awaited the effect of the pains, which

followed each other quickly and powerfully, but did not bring the head lower; there was heat and great dryness of vagina; lard was freely used during each pain. I endeavoured, by pressure in front of the ear, to carry the face toward the hollow of the sacrum; the head descended, but seemed as if it would pass towards the anus, and not in the axis of the outlet, the occiput not turning under the pubes. Not being able to accomplish the delivery by manual assistance I applied the forceps, and at a quarter to twelve delivered the head during the second pain; more than a quarter of an hour elapsed before another pain expelled the shoulders. The child did not cry for some minutes; its face and lips were of deep purple colour. I allowed blood to escape before tying the cord.

A dangerous flooding came on before the exclusion of the placenta, the uterus being emptied and filling again and again, so that I could not leave her bed-side for four hours. It was met by external pressure and cold applications, passing the hand into the uterus, and counter-pressure combined, plugging with sponge passed up to os uteri, brandy and laudanum.

CASE VI.—Cupper, æt. 39, thirteenth labour. She had been poorly through the day, and sent for the midwife at eight in the evening: the labour proceeded, and at midnight the os uteri was fully dilated. At six o'clock A.M. I was called to assist her; I applied the forceps, and delivered her of a large child.

CASE VII.—Rice, æt. 41, first labour. She had pains through the day, June 28th, 1837: at eight in the evening sent to her midwife, who, on examination, found the os uteri dilated to an inch, and the liquor amnii escaped; the vagina dry: the pains increased in force and rapidity, so that there was not an interval of five minutes between them. I went to her at eight the next morning; the head lay in the cavity of the pelvis, with the face to the hollow of the right ilium. I touched with difficulty the right ear above the symphysis pubis; the vagina was dry and unyielding, and the os coccygis did not recede on pressure backward. There was much swelling of scalp. During two hours there was no advance of the head, the apparent descent arising from increase of the scalp tumor. I passed

with difficulty a blade of the forceps between the head and left ramus pubis; the instrument locked within the vagina, and I succeeded in bringing the head down a little, and then, carrying higher up the blades, which had slipped, I delivered during the fourth pain: child (female) large.

CASE VIII.—On December 16th, 1832, at seven in the evening, I was requested by the midwife to visit Day, æt. 23, attacked with convulsions in her first labour; the os uteri was dilated to two inches and a half. At nine o'clock P.M. she had a second fit: at eleven P.M. I delivered with forceps: child still-born; weight 9lbs.; weight of secundines 1½lb.; there was a narrow conjugate diameter.

I learned that this poor woman died in her next confinement: she had the assistance of surgeons.

CASE IX.—M. Lynes, October 20th, first labour: was taken with a convulsive fit between two and three of the morning of Thursday, December 4th, 1834. At seven A.M. her midwife went to her, and in half an hour I attended at her urgent request. The os uteri was dilated to two inches. At ten A.M. I again saw her; the forceps was applied, and after considerable difficulty she was delivered of a child still-born.

Norwich, June 29th, 1841.

SOME
PHYSIOLOGICAL REFLECTIONS
ON
THE NATURE AND TREATMENT OF
ANGINA PECTORIS,

AND OF ANALOGOUS STATES.

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[For the Medical Gazette.]

[Continued from p. 658.]

Dyspnœa, with malformation of the heart, &c. — Bronchial states — Cases — Remarks.

WE now propose to take up a new branch of the subject, which for its own sake deserves attention, independently of the fact that it will assist to corroborate our general views of dyspnœa.

We have already hinted that the difficulty of breathing which depends more particularly on the larynx or first

air tubes, is regulated by the arterial circulation from the aorta, unless it is connected with the formation of tumors or the like. The congestive state, again, is most especially dependent on gravitations of fluid for its relief or aggravation; and it is hardly less than any cardiac disorder subject to the influence of motion, whether it be that the muscles, when in action, impede the general arterial current, or hurry on the venous currents, to the effect of accumulation about the right heart.

It is in order to exclude the case of pulmonary congestion, and enforce the subject of bronchial turgescence, that we have thought it available for illustration, and desirable also, for practical purposes, to introduce the consideration of dyspnœa in cases of malformation of the heart. This topic was passed over in a former paper*; but it will appear to be a necessary part of our present inquiry, and not, we hope, a mere digression.

In the most common of the serious malformations about the heart, as in some other cases, it is customary, we think, erroneously to refer the dyspnœa to that organ. When the commencement of the pulmonary artery is narrow, and the lungs receive but little blood, why should dyspnœa exist? or why should we seek for a greater cause than is so manifest in another part? If the larynx, and perhaps all that the bronchial arteries supply, be only slightly affected, in the manner in which we observe the face and tongue, &c. to be, is not here enough at least for the chief cause of dyspnœa, as well as of the catarrhal states?

The earliest and strongest evidences of cyanosis are doubtless in the face and mouth. Hæmoptysis, which is repeated, or perhaps suddenly fatal; epistaxis, which affords relief, and is sometimes difficult to arrest, and bleeding from tumid gums, are alike occasional indications of the nature of the disease: especially are they the occasional concomitants of the dyspnœa, and come and go, or increase and decline, with that affection; and there will not be much room to doubt that very often the difficult inspiration is pretty exclusively owing to the turgid narrowing of the air tubes nourished from branches of the common aorta.

We do not mean to exclude the notions of obstruction in the right heart and venous congestion; but this is not all; for the tumid aspect is likewise an indication of defective action in all the capillaries—those for nutrition and secretion, as well as the excretory and depurative. These views are not merely theoretical. The remedies, the exercise, and habits, and above all the warmth which liberates and exalts all the functions, afford the strongest indications of the truth of these explanations.

Once more, with reference to this state of dyspnœa, it is not unimportant to consider certain correlative states:—The left auricle is generally small, and devoid of any trace of impediment to which we might impute pulmonary obstruction. The mitral valve must at least act as truly, if not more so, than the tricuspid.

We notice a disposition to turgescence in every part but between the right ventricle and left auricle; as if the aortic capillaries, from various causes, were ill nourished and over-dilated; and we remark that the most delicate tissues seem to suffer most in this respect; for the vessels of the head are thinner than those of other parts generally, and some are still more prone to yield, or exposed to external influences, as those of the cheek and larynx. Some parts, as the hands and feet, seem more affected by the force of gravitation; but the head suffers in spite of gravitation.

It is by no means uncommon to hear the first signs or material characters of cyanosis referred, at least in point of time, to the occurrence of some one of the complaints of childhood, as to an exanthem, or to whooping-cough, or even to an accident. This, we think; serves to prove that the congenital malformations are by themselves less oppressive, and that some superadded states are often required before the actual and severe characters can become manifested.

In the paper before referred to, we shewed that serious malformations might exist long without any grave signs; and we indicated the manner in which bronchitis would bring on the prejudicial operation of communications between the two sides of the heart.

The heart being imperfectly nou-

* Vide the last volume of MED. GAZ. p. 685.

rished, as well as ill formed and too full, may fairly be considered as in a state more or less analogous to that of the capillaries themselves, *i. e.* feeble, incapable of due contraction, and liable to become dilated with comparatively slight disturbance. All the functions must be deemed defective, failing mutually to assist each other. There is no reason why the disordered bronchial membrane should suffer less than most other vascular tissues; but we may see that its tumefaction is more obstructive than that of the gums and tongue for instance; and there may be a reason why the lining of the air-tube, incessantly exposed to the motions and currents of respiration, should be least able to retain even a tolerably healthy state.

Is it necessary to shew that the state in question is a form of bronchitis? We shall hastily observe that the difficulty depends much on the weather, for the sufferer is indeed a delicate thermometer, and his disorder rises and declines with all the signs of bronchial catarrh, and at the same time it comes and goes with (in a pretty constant proportion to) the livor and fulness of the face and mouth; and this is true, whether we regard the succession of seasons, or hours or meals, or mere changes of posture or occupation. Should it be advanced that the general congestion precedes the dyspnœa, we venture to say that the assertion is not well founded for the common case; and it is important to remember that signs of catarrh are discoverable before dyspnœa is very manifest.

Before proceeding farther we must digress to remark, that in various records, and even of severe cases of morbus cæruleus, dyspnœa will be found unmentioned, or but very slightly noticed; and this we have found very curiously in coincidence with a *wasted* frame—with the absence of turgescence.

Dr. Hunter's case, already related, shews that the narrow pulmonary artery and communicating ventricles (the most common cærulean states) do not necessarily induce dyspnœa; and his case is a notable example of livor with emaciation. We shall presently introduce another.

It should be well remarked how much the cærulean dyspnœa is regulated by repletion and by gravitation of fluids, as well as by warmth, which,

while it liberates and excites the functions of the skin, does hardly less in such cases for almost every organ of the body.

And this also is the effect of exercise, when it can be readily endured. The capillaries, arteries, heart, and veins, and all the nutrient and secretory functions, are happily freed, and the balance of health is comparatively restored.

The following is a singular illustration of our present subject. Here it would be difficult indeed to entertain the idea of accumulation between the pulmonary artery and the left side of the heart; for the author almost thought that the pulmonary artery was impervious. It is a case of obstructed pulmonary artery, without communication of the ventricles.

Tacconi related the case, and it has been before employed in England; but we shall venture to agree with him in regard to it, rather than with Dr. Farre, from whom we quote.

A young woman, who lived by begging, attributed the commencement of her disorders to a fall, when five years of age. She was excessively livid and enfeebled—had a quick weak pulse, and *no perceptible respiratory movements*. Her *voice* was low and faltering, and her deglutition very difficult. She suffered most in winter, and grew much worse from her fifteenth to her eighteenth year, when grumous hæmorrhage from the mouth came on in severe weather, and she died in about three weeks after.

The chest was found extremely small, and the lungs dry, hard, and contracted. The left lobe was livid and adherent. About three ounces of grumous blood were found in the trachea and on the surface of the diaphragm.

The foramen ovale was more wide than in the fœtus. The pulmonary sigmoid valves were united so as to leave the smallest possible opening. The ductus arteriosus was closed. The heart was somewhat cubical, and the two ventricles in capacity and substance seemed as if transmuted, and did not communicate.

Now this is doubtless rather a wonderful case, but it was evidently well observed, and is well narrated (in a southern clime it should be a little less marvellous); and we do not hesitate

to reflect upon it, seeing it is not without parallels in most of its features.

We have formerly shewn that a certain amount of obstruction of the pulmonary artery*, under favourable circumstances, may be long in manifesting itself; but we cannot admit the positive conclusion of Dr. Farre, that accident could not give rise to the same obstruction in childhood.

The circulation from the left ventricle was mingled with dark blood, always weak and never difficult, and that from the right ventricle seems to have been most scanty and laborious, *i. e.* judging from the development of the parts. But there was no dyspnœa. The breathing was adequate to a weak voice, and to the limited quantity of blood conveyed by the pulmonary arteries.

These cases are amongst the rarest; but one, still more uncommon, of the same kind, had in addition the tricuspid valve imperfect, and still less propulsive, the foramen ovale being wide as the little finger, and serving instead of a communication between the ventricles. The patient was a female, who had "always lain sick," livid, feeble, and *short-breathed*. She died in her sixteenth year†.

The state of the lungs, which have not been freely employed, seems sufficiently peculiar. They are small, not devoid of air, too fleshy and firm and inelastic, and decidedly more solid and tough than simply collapsed airless lungs.

The absence of all signs of distension in the pulmonary arteries is too evident to be mistaken; and here we find a striking hint with respect to the seat of the suffocative sense.

It is certainly true that varying dyspnœa is an attendant on most kinds of malformation of the heart, and we shall yet seek to inquire into the common circumstances; but already we conceive it is made clear that malformation has no necessary effect in disturbing the respiration, since at birth, and even for years after, no dyspnœa is often manifested. Again, it does not appear that dyspnœa bears

any direct relation to the cardiac malformation, though it may belong more particularly to certain kinds of deformity, and yet the latter is permanently the same, while the dyspnœa is only occasional or variable.

We must admit that the morbid circulation of the misshapen heart is aggravated by bronchitis, as we have seen that the malformation induces indirectly the bronchial congestion; and looking towards the conclusion, we may understand how it happens that, with a weak and impure circulation, as the body grows and demands more, the combination of evils is constantly increasing:—oppressed functions oppress each other still more; all suffer increasingly, and thus development fails irretrievably and fatally; we do not say fatally, provided attention be paid in good time. It is little to say that too much importance cannot be attached to the respiration, when the fluid to be acted on is reduced to its minimum, the rest being venous in the extreme, and all the body is suffering proportionately: the malformation is irremediable, but the dyspnœa is not so.

It is not undeserving of remark that the first arterial blood being given to the heart, the next in point of time is devoted to the tubes, the muscles, and the brain of respiration. These parts are of the most vital consequence. It is defective respiration, next to a narrow pulmonary artery (and even more than a narrow mitral valve), which constitutes a fatal obstruction to the circulation, causing a communication from the right auricle into the left, or still more aggravating concomitant disorders—venous delays on the one side, and arterial deficiencies on the other, with all their consequences.

The following account of a heart, in a case of cyanosis, will assist to illustrate the state of the circulation:—

A young lady had in infancy some general symptoms of malformed heart, but these were not oppressive, and but very slowly increased. Her hands and face were blue, and she was rather inactive, and very sensible of the cold, and liable to be affected by it with catarrh; but her parents did not observe any alarming signs until she attained her eleventh year, when all the symptoms, especially the dyspnœa, became aggravated, and this last seemed to have considerable share in her death, which took

* MED. GAZ. *loc. cit.*—If the pulmonary artery be obstructed and the septum of the ventricles be perfect, it seems reasonable to infer that the obstruction has arisen (or at least in part) subsequent to the completion of the septum.

† Vide Morgagni's 17th letter, art. 12, 13; also Dr. Farre's Essay, p. 16.

place soon after she had attained her twelfth year. The livor of the face had long been extreme. This patient had grown tall, but was very thin. The following account and remarks were penned with the heart before me. The specimen is still in the possession of Mr. Iliff, of Newington, in whose practice the case occurred.

The heart is rather large, and its surface apparently healthy, but the substance of the ventricles may have been too dense and dark.

The right auricle is the most capacious cavity, being very wide, and its wall is strong. The foramen ovale is equal to the largest goose quill, but now perfectly valvular to close by pressure on its right side.

The valve of Eustachius is well formed and extensive, but somewhat perforated or reticular. It probably brought the stream from the inferior cava very close to the fossa ovalis.

The two ventricles are of good capacity and power, and about equal; a communication exists between them very close to the tricuspid and aortic valves, which would admit the little finger. The muscular columns in the right ventricle are very large. The tricuspid has considerable resemblance to a bicuspid, and it most likely acted very accurately and firmly, having a very short but strong moderator band, even double, and surfaces of contact on its curtains wide and studded with flat masses of minute firm vegetations; the result of attrition and increased nourishment.

The pulmonary artery is small and thin, with a conical valve truncated above to form an opening which may have admitted a very small goose-quill, but being thickly set round with vegetations (like the former) will now scarcely admit more than a crow-quill.

The cone has three bands or ridges of connection, with the lining of the artery extending nearly to its summit; these are evidently the vestiges of the natural attachments of the crescents.

The cord of the ductus arteriosus is complete.

The left side of the heart is less remarkable. The auricle is small and thin, and especially its apex. The surfaces of contact on the mitral curtains have vegetations, as the tricuspid, but in rather smaller numbers.

The aortic valves are quite healthy,

and the lining of the vessel is but a very little spotted with atheroma.

REMARKS. — The different vegetations may very well have originated within about twelve months; and we may infer that the granules around the narrow pulmonary orifice arose from unequal pressure or tension, as likewise the rest. Doubtless the main obstruction of the pulmonary artery has existed from the earliest period, and has become aggravated by the formation of vegetations.

It is evident that the communications between the two sides of the heart must have relieved the right in proportion as the latter was subject to accumulation.

The right ventricle remains, as in the fœtus, equal to the left, because it has continued to have equal labour.

The right auricle is large because of delay and accumulation, while the left shews that it has been able to dispose readily of all it received from the lungs and the right side. Although it is possible that reflux may have had some share in causing the greater dilatation of the right auricle, yet the labour of the right ventricle, and its nutrition retaining the fœtal proportions, will explain the fact that the safety-valve has not become developed (the right wall of the ventricle never having become thin and yielding); that the tricuspid has closed too well, and its curtains present impressions of mutual contact which irritation has rendered granular.

The pulmonary artery has had but little to do, and is little developed.

Now it might seem only tedious to repeat our opinions in reference to this case, and the reader who has accompanied us thus far can scarcely need such a review. We have still, however, a few general comments to add on the subject of dyspnœa.

There is as little reason to suppose impediment in the pulmonary veins as accumulation in the pulmonary artery. It is true that when the two ventricles communicate freely, and we can only infer that they act with equal or rather with one force (though not precisely equal in capacity, for the larger will, we think, be found proportionably the thicker), when but little blood is aerated, and all the functions are consequently and commensurately imperfect, the nutrition of the ventricles is

deficient, and they become susceptible of dilatation and palpitation, and thus the return of the blood from the lungs may be somewhat obstructed; yet here, and even when the foramen ovale is wide, the left auricle almost invariably retains less indications of distensions than the other cavities. The evident vestiges of delay in the left auricle are often little, always less than elsewhere, and sometimes altogether undiscoverable. We do not mean that impeded respiration will not leave the pulmonary artery still overburthened, however its orifice may be contracted.

It may be thought that the proof of bronchial obstruction is deficient in such cases, but this demonstration must be wanting, since the appearances after death cannot be conclusive.

Even in the instance of œdema glottidis, when fatal, the tumefaction subsides in part as the redness disappears; so that we have a right often to infer that such appearances in death are the indications of more serious obstructions in the last hours.

Pneumonia and bronchitis do not so much impose the erect posture as œdema glottidis and asthma, or even some diseases of the heart. With respect to hydrothorax we find room for a distinction in the facility with which the fluid gravitates and depresses the diaphragm as the patient sits, instead of oppressing the lungs when he is recumbent; yet we believe this is not to be relied on.

In taking leave of the dyspnœa of malformed hearts, we venture to conclude that the disorder does not so much consist in pulmonary congestion, as in aortic or bronchial, and that it is still less spasmodic. The case will commonly be explained by reference to the more or less simple effects of exertion, catarrh, repletion, or gravitation; and we may presume to ask with respect even to asthma, who is fit to contend for the spasmodic doctrine who does not first fairly determine and fully allow for all the manifest influences to which we have adverted?

Finally, we may observe that if our views of dyspnœa are just, they should doubtless be available in the treatment of the disorder considered by itself. We would, however, beg the observer to keep in mind the ceaseless complications which demand a modified treatment.

Our sketch has only regarded the chief facts of the case, but we feel assured that so much must not be lost sight of, unless empiricism is to be the guide in therapeutics, and no guide whatever is to be allowed in the study of thoracic disorders.

The topic of Palpitation must next engage our attention, and in conclusion we shall endeavour briefly to sum up the most positive part of our reflections, and their general concern with the laws of therapeutics.

[To be continued.]

MEDICAL GAZETTE.

Friday, July 23, 1841.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”

CICERO.

THE REPRESENTATION OF THE PROFESSION IN PARLIAMENT.

A MEETING of the members of the profession was held in Dublin about a month ago to consider the best means of ensuring the return of a member to Parliament for the purpose of advocating the interests of the profession. The result of the meeting has not, we believe, exceeded the publication of the speeches; but it will be desirable to consider, by the aid of their light, the subject of which they treat, although the time for acting on any conclusion is now past.

One is naturally led to ask, first, what are the *interests* of the profession which the holding of the meeting, and the general tenour of the speeches, imply are not represented in Parliament? Surely, when we remember that in the short session just brought to a close no less than three distinct measures of medical reform were brought into the House of Commons, it can hardly be said that the interests of the profession are not attended to, though, perhaps, they are in truth little cared

for. Is not Mr. Hawes, Mr. Warburton, or Mr. Wakley (the authors of these several bills, each of which assumed that they expressed the opinion of the profession) is none of these our representative? We have often doubted it; but the gentlemen of Dublin have no doubt. We think we catch in one of their speeches an allusion to the first as the representative of another much less respectable class; the second is spoken of as having introduced one good measure, the Anatomy Bill, but no more; and as for the third, hear what Dr. John Jacob, who seems to have had the feelings of the meeting with him, says:—"If there were no other reason" [in favour of having a medical member], "except that there is at present a medical man in Parliament who is generally looked up to as the organ of the profession, I think the necessity of having some proper representative for our body becomes most imperative [loud cheers]. We are, in fact, in a most deplorable state, when we recollect that that individual is looked on as our organ in Parliament. I allude to the coroner for Middlesex; and I ask does he represent the feelings of this meeting? [cries of no, no.] If we can effect our object in getting a proper representative, instead of that individual, I say that no sacrifice of time, of exertion, or of money, would be misapplied [hear, hear]."

As far as the assertion is concerned that the member for Finsbury is not the representative of the whole profession, or of more than a small section of it, every one, not a member of that section, will of course agree with Dr. Jacob; but we are surprised that, having stated this, he should still seem to think it possible to have one representative of the profession. Mr. Wakley represents a certain portion of our body, and we are bound to say that he does so with vigour, though it be with a wrong object, and

to little purpose. Mr. Warburton and Mr. Hawes, again, each represent another party with less energy, and with no better success; and the representative of the Dublin meeting would be the organ of their opinions, as far as they can agree upon them, but of no others. It would be as easy to find one representative of the whole commonalty of England, as one of the whole medical profession; for the diversities of medical politics are scarcely less numerous than those of general political opinions, and the sections of its members are at least as manifold as their nineteen governing bodies. And who of them all had best be represented? The maintainers of established institutions and customs, or the levellers of the one-faculty system? The aristocratic fellows and members of court and council, or the democratic licentiates and members of the mass?

Some of the speakers at the Dublin meeting, however, appear to have limited their views of what should be the duty of the representative within more reasonable bounds. The excellent chairman, Dr. Colles, for example, remarked rightly that there were certain points upon which all the parties in the profession were thoroughly agreed; and these it should be the office of the medical representative to pay regard to. Such are, for example, the just scales of remuneration for public services in courts of law, on inquests, in attendance on the poor, and so on; in all of which the interests of medical practitioners have, in late enactments, been grossly neglected. And indeed if for the remedy of these and many other things which we have never lost an opportunity of pointing out, as the evils under which the whole profession labours, and for which it would be much wiser to seek redress, rather than to make vain complaints that the shares of honour or emolument do not fall

equally on all of all classes without regard to merit—if, we say, for the remedy of these and other such grievances an efficient advocate were placed in the House of Commons, benefit would, without doubt, result.

We agree therefore with the general statement of the resolutions passed at this meeting, that the profession is not, but ought to be, represented; and that the advocacy of those things only which are obviously for the interest of the whole body should be the member's task. But further than this we cannot go with them. The plan of having a paid representative is one which we believe would speedily mar all the benefit that might otherwise be obtained. Such a man would not be listened to; his statements would be regarded with all the suspicion that attaches to those of a deeply interested party; and his opinions would be considered with no more respect than those of one forced to maintain a private interest, though it were at the expense of the public good. Besides, since he could not avoid adopting one or other course in relation to the politics of medicine, to the modes of government, the exclusive power of granting diplomas, and all questions of medical discipline, by whom should he be paid? Which party would support a man who advocated views opposed to their own, or even materially differing from them, on these points? The certain result would soon be that each party would have a representative of its own, and the House of Commons would become the theatre of discussions that had far better be kept from the public. Besides, the whole system of paid representatives is mischievous and un-English; and we should deeply regret to see the medical profession among the first to encourage it.

What is really wanted for the benefit of the profession is a member of the

House of Commons of independent property, and, to whichever party in general politics he might be attached, independent of any medical section. And surely it speaks little for the love of the profession—so much vaunted by many—that in these times no one of our own body has been found ready to sacrifice some of his time and money for the common good of all his brethren. Why are not some of the princely fortunes that are made in practice, thus, at least in part, devoted to so good a purpose as the defence of those through whose assistance they have been in great measure accumulated? How far better, for example, would the money of Sir Astley Cooper have been spent, if he had placed himself, instead of his brother, in Parliament! With his influence, and with the thorough independence which he in the last years of his life enjoyed, his word would have been amply sufficient to prevent the infliction of every one of those injuries which are now most severely felt in various recent enactments. We say this not to disparage him, but to endeavour to point out to those who now occupy his place in the profession the course which they should have deemed it their bounden duty to adopt.

It is by men of this kind, and not by paid agents, that the interests of all other professions and trades are protected in the House of Commons. The rich barristers deem it their highest honour to be members, and readily give up much of their time, after labours of no ordinary severity, to the public service, in which they, of course, never fail to protect their own and their brethren's interests. (And well enough, it must be confessed, they do provide for their own class; for there are certainly no two bodies of men who, being placed in other respects in nearly equal ranks, share more differently the advantages of place, power,

and honour, than do lawyers and medical practitioners.)

It is true that many of the legal members of the House have remote personal objects in view, such as peerages, and judgeships, and so on; while none of the same kind are open to medical men. But this is far from being the case with all the barristers who are in Parliament; and still less is it true of the members of other professions and trades—merchants, bankers, and the like. All these find their honour in protecting the interests of their several classes, and it is a thing almost inexplicable that no members of our profession are found anxious to follow the same course. We could mention several now in London who have scarcely any other honours left for them to earn, except those which they would have obtained by becoming representatives of their profession's interests, and they are all gentlemen with fortunes amply sufficient for contested elections, and with practices such as, if they sacrificed the half, would still leave them noble incomes. Neither would they act without precedent; they have the good example of Dr. Radcliffe before them, a man who probably enjoyed as extensive a practice as any of them, and who yet sat in several parliaments, and then devoted a large fortune to the public service. "Dr. Clements also," as Dr. Maunsell tells us, "for many years represented the University of Dublin; and the truest-hearted Irishman, perhaps, that ever sat in the House of Commons, was Dr. Lucas."

It would be a happy thing if the interests of the profession could be thus represented; but we still think that we had far better remain as we are—unrepresented, or even misrepresented—than resort to the expedient of paying any man, medical or not, to be our advocate. Such a scheme would be

useless, or worse than useless. Instead of attempting it, let medical practitioners make the best interest they can with the more intelligent lay members of the house; and above all let them take care not to make the most prominent objects of their anxiety the relief from supposed disadvantages of inequality of rank or privileges. Let them look steadily to the obtaining a due regard for their obvious interest in matters of immediate importance to them; such as remuneration for public services—protection from the incursions of the unlicensed—a fair scale of payment for medical assistance to the poor—a relief from the necessity of undertaking contracts, and all practices by which they are degraded to the level of tradesmen. These are things which all are alike interested in obtaining; and if these, and such as these, are secured, we suspect that all the other grievances, now so much complained of, will appear marvellously easy to be borne. Moreover, these are things the right of which is so obvious that any senator with common sense may be made to see the propriety of supporting them, though he might find much difficulty in appreciating the value of a modern system of medical reform.

UNIVERSITY OF LONDON.

M.B. FIRST EXAMINATION.—1841.—EXAMINATION FOR HONOURS.

Thursday, July 15th.—Morning, 10 to 1.

Anatomy and Physiology.

Examiners,

Mr. KIERNAN and Professor SHARPEY.

Candidates may illustrate their answers by sketching the parts they describe.

1. The muscles of the back of the neck, the sterno-cleido-mastoidei and the vertebral column being removed from the head, describe the posterior surface of the pharynx, and the parts which lie between the pharynx and the inner surface of the ramus of the jaw, as far as the last molar tooth. The pharynx being opened at its posterior part and in its whole length, describe the parts

brought into view; the description to include that of the posterior nares, the soft palate, its arches, muscles, vessels, and nerves, the tonsils, the isthmus faucium, the dorsum of the tongue, its glands and papillæ, the epiglottis and its folds, the superior aperture of the larynx and its posterior surface.

2. Give an account of the several structures which enter into the formation of the skin. The answer to include a description of the glands of the skin, but not the hairs or nails.

Afternoon, 3 to 6.

1. Commencing the dissection at the integuments, and continuing it to the anterior surface of the vertebral column, describe the parts successively met with in a portion of the neck bounded below by the upper part of the sternum and the first pair of ribs, above by a line drawn across the lower part of the cricoid cartilage, and on the sides by lines corresponding to the external margins of the *scaleni antici* muscles.

2. Describe the intimate structure of secreting glands in general, and the varieties in the intimate structure observed in different glands of the human body, giving the evidence on which any opinions you may state are founded.

Friday, July 16.—Morning, 10 to 1.

Chemistry.

Examiner, Prof. DANIELL.

1. According to the atomic doctrine, is there any, and what, connection between the atoms of bodies, and the heat and electricity which they contain?

2. Describe and explain the construction and action of Harris's unit-jar.

3. Describe and explain the construction and action of Saxton's magneto-electric machine.

4. Supposing a portion of fused chloride of lead, and an aqueous solution of sulphate of soda, to be exposed to the electrolytic force of the same voltaic current, what would be the products of their decomposition, and what proportions would they bear to each other?

5. Taking the composition of cane-sugar to be $C_{12}H_{22}O_{11}$, how must it become modified before it can be susceptible of the vinous fermentation? and what will be the products of such fermentation?

6. I have weighed a tube, containing an organic substance without nitrogen, to be analysed, and found its weight to be 51.610 grs.: having emptied it, I again weighed it, and found the weight of the tube to be 48.400 grs.

The substance thus transferred was mixed with oxide of copper, exposed to heat, and the products collected in the usual way.

Before the experiment, Liebig's potass-apparatus weighed 453.284 grs.; afterwards, 458.223 grs.

Before the experiment, the chloride of calcium tube weighed 174.000 grs.; afterwards, 175.870 grs.

What were the proportions of the elementary constituents of the substance analysed? What the number of the equivalents of each? What was the substance analysed?

7. State and illustrate Berzelius's doctrine of ammonium.

8. What is benzule? What is its connection with the volatile oil of bitter almonds and benzoic acid?

9. Explain the formation of hydrocyanic acid in the bitter almond.

Friday, July 16.—Afternoon, 3 to 6.

Examiner, Dr. Pereira.

1. What is the per-centage strength of the *acidum hydrocyanicum dilutum*, Ph. L.? What quantity of cyanide of silver should 100 grains of this acid yield on the addition of a solution of nitrate of silver? State the characteristics of cyanide of silver, and how you would distinguish it from chloride of silver. If a sample of diluted hydrocyanic acid yield a red precipitate on the addition of the iodo-cyanide of potassium and mercury, what inference would you draw therefrom? State the composition of this red precipitate, and explain the chemical changes which occur during its formation under the above circumstances.

2. The bottles marked respectively A and B contain adulterated powder of scammony: you are required to apply the appropriate tests [*which will be furnished*] in order to discover the nature of the adulterating matter in each.

3. Describe the mode of preparing the *spiritus ætheris nitrici*, Ph. L.; and explain the nature of the principal changes which occur during the process. What is the composition of this spirit? To what contaminating matter or matters do you ascribe the property usually possessed by the spirit of nitric ether of the shops, of becoming olive-coloured on the addition of protosulphate of iron, and blue with tincture of guaiacum?

4. State what are the symptoms caused by a poisonous dose of hydrocyanic acid, and what remedial means you would have immediate recourse to in order to avert death. Describe the *post-mortem* appearances when this poison proves fatal. How would you proceed to detect its presence in the contents of the alimentary canal? Explain the nature of the chemical changes which occur on the addition of the several tests.

5. By what chemical test is the flour of

white mustard seed distinguished from that of black mustard seed?

6. Describe the effects, uses, modes of administering, and doses of *digitalis*.

7. You are required to name the four barks labelled respectively C, D, E. and F.

8. Describe, in botanical language, *conium maculatum*, pointing out those characters by which it is distinguished from neighbouring species.

FIRST EXAMINATION FOR THE DEGREE OF BACHELOR OF MEDICINE.

July, 1841.

EIGHTY candidates presented themselves at this examination. The following is a list, alphabetically arranged in two divisions, of those who passed:—

First Division.

Medical Schools.

Ballard, Edward	University College.
Beaumont, Henry.....	University College.
Davies, John Jones	London Hospital.
Davis, John Hall	University College.
Fearnside, Henry.....	University College.
Fox, Joseph John	University College.
Francis, Dayrell Joseph T.	Guy's Hospital.
Garrod, Alfred Baring	University College.
Gull, William Withey.....	Guy's Hospital.
Hadwen, Arthur	University College.
Hudson, John	Leeds, & Univ. Coll.
Humble, William Edward..	University College.
Jacob, Henry Long	University College.
Jenner, William	University College.
Inman, Thomas.....	King's College.
Johnson, George	King's College.
Leonard, Thomas.....	University College.
Letheby, Henry.....	Aldersgate.
Matthew, Thomas Patrick..	University College.
Parker, Nicholas	London Hospital.
Raper, William Augustus ..	University College.
Savage, Henry	University College.
Stedman, Silas Stilwell	University College.
Swayne, Joseph Griffiths ..	Bristol Med. School.
Tapson, Alfred Joseph	University College.
Topham, John	University College.

Second Division.

Blake, James.....	University College.
Blomfield, Thomas Alfred..	University College.
Bunce, Richard.....	Bristol Med. School.
Evans, John	University College.
Francis, Charles Richard ..	Middlesex Hospital.
Graham, Thomas	Middlesex Hospital.
Hearne, Edwin	University College.
Heath, George Yeoman	University College.
Jones, Evan Burnell	Webb Street.
Jones, Thomas Lloyd	University College.
Langmore, John Charles ..	London Hospital.
Martin, Joseph Telford ..	Royal Coll. of Surgeons in Ireland.
Maynard, Thomas B. E....	Bristol Med. School.
Meryon, Edward	University College.
Mott, Charles.....	Webb Street.
Noyes, Henry George.....	Guy's Hospital.
Padley, George	Guy's Hospital.
Paley, William	London Hospital.
Randall, John	Aldersgate.
Smith, Henry	St. Bartholomew's Hospital.
Staples, Charles Theodore	Milby Hosp. at Ceylon, and Univ. College.
Stiff, William Phillimore ..	University College.
Wayte, Charles Matthew ..	Bristol Med. School.
Williams, William Henry ..	University College.

EXAMINATION FOR HONOURS.

(The names are arranged in the order of proficiency.)

ANATOMY AND PHYSIOLOGY.

Ballard, Edward, University College.

Exhibition and Gold Medal.—Matthew, Thomas Patrick, University College.

Gold Medal.—Tapson, Alfred Joseph, University College.—Stedman, Silas Stilwell, University College.—Inman, Thomas, King's College.—Davies, John Jones, London Hospital.—Fox, Joseph John, University College.—Swayne, Joseph Griffiths, Bristol Medical School.

CHEMISTRY.

Ballard, Edward, University College.

Exhibition and Gold Medal.—Fox, Joseph John, University College.

Gold Medal.—Inman, Thomas, King's College.—Humble, William Edward, University College.

MATERIA MEDICA AND PHARMACEUTICAL CHEMISTRY.

Inman, Thomas, King's College.

Exhibition and Gold Medal.—Davis, John Jones, London Hospital.

Gold Medal.—Tapson, Alfred John, University College.—Humble, William Edward, University College.

Examiners { J. F. DANIELL.
F. KIERNAN.
J. S. HENSLOW.
JON. PEREIRA.
W. SHARPEY.

At the late B.A. examinations for honours, Samuel Newth, B.A. obtained a Certificate in Mathematics and Natural Philosophy, and Samuel Coulter Davison, B.A. in Classics. Both these gentlemen are of University College.

INVENTION OF THE FLEXIBLE STETHOSCOPE.

To the Editor of the Medical Gazette.

SIR,

I VENTURE to occupy a space in your valuable journal, not to take notice of Dr. Burne's very unjustifiable cross-examination, but to do justice to the talent and ingenuity of an excellent and much-esteemed physician, to whom I have through a private source lately learned that we are indebted for the first application of a flexible tube for the purpose of auscultation. I refer to Dr. Stroud. This gentlemen, about twelve years ago, first suspected the possibility of hearing the sounds of the chest by means of a flexible tube, and on submitting this idea to positive experiment (which, by the way, was done in the shop of Mr. Weedon, the surgical instrument maker), he was gratified by discovering that the respiratory murmurs were distinctly audible; he was led to modify the length of the tube and to adapt a metallic ear-piece, and has constantly

used this instrument since. Dr. Stroud gave an account of his contrivance, and presented instruments similarly constructed to several physicians, and among others to Dr. Clendinning (who for eight years has always employed the flexible tube), Dr. Roge^t, and to Dr. Burne, whose claims to originality, at least on this point, must therefore be consigned to the tomb of the Capulets. It is to be regretted that Dr. Stroud did not publish an account of this instrument. I first heard of the employment of the flexible tube from a pupil of Dr. Clendinning; and it was not until the last week that I became acquainted with the real history of the contrivance; and was not aware that Dr. Burne "had ever dreamt of using a flexible tube" for auscultation until some time after the publication of my paper, in December, when I was first informed of it by my brother, Dr. Frederick Bird, to whom Dr. Burne has so unjustifiably alluded, as the medium through which I had attempted to rob him of a contrivance to which it now appears he has not the slightest pretension. I only trust that, for the future, before he prefers the serious and important charge of plagiarism against any one, he will take care to ascertain whether it be well grounded. As the matter now stands, all the remarks levelled at me by Dr. Burne apply *mutatis mutandis* to himself.—I remain, sir,

Your obedient servant,
GOLDING BIRD.

Wilmington Square, July 16, 1841.

AN INDIGENOUS REMEDY FOR GONORRHOEA.

To the Editor of the Medical Gazette.

SIR,

As I consider it would be a very important addition to our materia medica to obtain a remedy for gonorrhœa in an indigenous plant, I hasten to request you will make known through your journal that it is believed such a remedy is to be found in the *Senecis Jacobæa*, or ragwort. It does not fall to my lot to have sufficient cases of gonorrhœa to furnish very conclusive evidence upon the value of a new remedy; and as it happens that if a delay of a few weeks should occur before it is tried in a sufficient number of cases, the plant will be out of season, and not obtainable till next year, I have deemed it best, therefore, though somewhat irregular, to request through your pages that some practitioners who have more practice in the disease would be good enough to try it. To facilitate this I have desired Mr. Bullock, chemist, of 22, Conduit Street, to prepare

carefully a few pounds of the extract for distribution, as from its nauseous taste it must be administered in pills. He will, therefore, furnish any gentleman who will call or send his card with a sufficient quantity for two or three cases. May I add the request that the result of the experiments may be forwarded to me, to be, with my own, arranged for publication in the *MEDICAL GAZETTE*?

I am, sir,
Your obedient servant,
JOHN GARDNER.

49, Great Portland Street,
July 18, 1841.

ON THE SPONTANEOUS EVOLUTION OF SULPHURETTED HYDROGEN IN THE WATERS OF THE WESTERN COAST OF AFRICA, AND OF OTHER LOCALITIES.

BY J. FREDERIC DANIELL,
For. Sec. R.S., Prof. Chem. in King's College,
London, &c.

[Concluded from page 672.]

THE analysis of sea waters generally, and these analyses in particular, show that a large proportion of sulphates is always present in them, and there is no doubt that extensive mud-banks must be formed at the mouths of the African rivers, within the tropics, consisting chiefly of vegetable detritus, in the exact state which is most favourable for this action.

Since my report to the Admiralty upon this subject, I have seen a paper in the *Annales de Chimie* for July 1840, by Dr. Amédée Fontan, upon the Mineral Waters of Germany, Belgium, Switzerland, and Savoy, in which he suggests that the presence of sulphuretted hydrogen in those waters may be owing to the decomposition of the sulphates which they contain by vegetable matters, remarking that many of them, which contain little of that gas at their sources, acquire more of it by their flow through the soil. There can be little doubt of the correctness of this opinion.

A curious fact has also been brought to my recollection by my friend Mr. Fownes, with regard to a spontaneous change which a solution of litmus undergoes when excluded from the air. It becomes of a brown colour, but still it is not spoiled, for the colour is restored by exposure to air.

M. Vogel (*Ed. Journ.* 31, 157), who inquired into this curious fact, found that the solution always contains sulphate of potassa, which becomes gradually decomposed with the generation of sulphuretted hydrogen, to the deoxidating power of which the effect is owing. A few drops of sulphuretted

hydrogen solution produce the same effect in a few days; the solution becomes brown, but speedily recovers its colour upon contact with air. This case is the more interesting, inasmuch as the gas never exists in sufficient proportion to be discoverable by the usual tests; nevertheless it acts in these minute quantities with great energy.

But now a much more important and interesting question than that of the preservation of the copper sheathing of ships forces itself upon our attention, and that is, whether the existence of this deleterious gas in the atmosphere, which must necessarily accompany its solution in the waters, may not be connected with that awful miasma which has hitherto proved so fatal to the explorers and settlers of the deadly shores of Africa; and whether, if so, science may not suggest something to palliate an evil which is so dreadfully opposed to the progress of civilization in those parts.

When this matter was first brought under my consideration, I was surprised that the nauseous smell which must necessarily be evolved from water impregnated with this gas, at so high a temperature as that of the equinoctial regions, had not been noticed. I have in consequence turned to some of the accounts of the late travels in Africa, to seek for evidence upon the subject; and in the narrative of an expedition into the interior of Africa, by the river Niger, by Macgregor Laird and R. A. B. Oldfield, I found the following important observations:—

“The principal predisposing causes of the awful mortality, were in my opinion the sudden change from the open sea to a narrow and winding river, the want of the sea breeze, and the prevalence of the deadly miasma, to which we were nightly exposed from the surrounding swamps. *The horrid sickening stench* of this miasma must be experienced to be conceived: no description of it can convey to the mind the wretched sensation that is felt for some time before and after daybreak. In those accursed swamps, one is oppressed not only bodily but mentally with an indescribable feeling of heaviness, languor, nausea, and disgust, which requires a considerable effort to shake off.”

Now, these observations were made in the very locality from which some of the first waters which I examined were taken, and nothing more is wanting to identify the cause of the rapid decay of the ship's copper with that of the mortality of the climate.

It has been experimentally found, that so small a mixture as a fifteen hundredth part of sulphuretted hydrogen in the atmosphere, acts as a direct poison upon small animals, and the sensations of languor, and nausea, described by Mr. Laird, are exactly those which have been experienced by persons who

have been exposed to the deleterious influence in small quantities.

The symptoms, in cases where this gas is breathed in a state of concentration, well known to medical men, are sudden weakness, and all the signs of asphyxia: the individual becomes suddenly weak and insensible: falls down, and almost immediately expires. When the exposure has been too slight to cause serious mischief, the individual is affected with sickness, colic, imperfectly defined pains in the chest, and lethargy.

Now can it be deemed at all improbable that an agent which is capable of acting with this severity as a direct poison, when mixed in no very high proportion with the atmosphere, should in still less quantities greatly aggravate symptoms of morbid action, which may possibly have their origin in other causes?

In the very expedition, from the account of which I have already quoted an extract, a circumstance occurred which is almost an experimental confirmation of these views. The first sickness and death in that expedition began at Cape Coast Castle; three died before entering the river, and the great mortality took place before they reached Damuggoo at the extreme upper end of the Delta, where they only arrived after a voyage of thirty-six days, from the 11th of October to the 16th of November, or twenty-seven from their entrance of the river Nún.

Now it is worthy of remark, that just before entering the river, in “breaking out” the hold to lighten the vessel, it was discovered that the cause of a “disagreeable vapour, from which they had long suffered, was, that the bags containing the cocoa had rotted, and the cocoa had fallen into the salt bilge-water, and there become putrid.” Here, then, were the very ingredients for generating sulphuretted hydrogen to a great extent: the lamentable consequence has been before alluded to, namely, three deaths before reaching the river. There can indeed be no doubt that the disagreeable effluvium of bilge-water, which has been carelessly left undisturbed for a long time, is owing to similar decomposition.

It is doubtless the same circumstance which renders Mangrove swamp so notoriously pestilential in all parts of the torrid zone. The tree only thrives in salt water, and its decayed foliage is admirably adapted to act upon the sulphates; and it accounts for the observation, that malarious fevers diminish as we recede from the coast, although swamp and rank vegetation may still prevail.

The close investigation which I have since given to the subject, more and more convince me that the worst cases of *malaria* are generally connected with the presence of sulphuretted hydrogen.

There is a paper in the twenty-ninth

volume of the *Annales de Chimie*, p. 225, by Signor Gaetano Giorgini, which offers the strongest possible confirmation of my opinion.

“The observation of Signor Giorgini has been drawn to the state of the atmosphere in the neighbourhood of certain marshes on the borders of the Mediterranean; and by reference to historical data, and various documents, he has proved the great importance which attaches to the circumstance of their being, at times, in communication with the sea, so as to have a mixture formed between their waters and that of the sea. Both ancient and modern authors have announced the fatal effects produced in the neighbourhood of marshes by such mixture, and a local belief of the same thing is very common and strong.

“On the south of the Ligurian Apennines is a marshy shore, bounded on the west for twelve miles by the Mediterranean, on the south by the river Serchio, and on the north by the river Frigido, a torrent commencing at the foot of the Apennines, in the state of Massa di Carrara, running three or four miles over the land, and then falling into the sea. The plain is from two to four miles wide, and is traversed by a few short torrents or streams; among these are the rivers Camajore and Pietra Santa, which divide the plain into three separate basins. The rain and spring waters which flow into the three basins mentioned, are slowly discharged into the sea by natural or artificial canals, penetrating the sand-bank, which exist on the sea-side.

“The level of these stagnant waters is between that of high and low water in the neighbouring sea; there being but little difference between these two points in this part of the Mediterranean. In this state of things, formerly, when the waters of the sea rose from any circumstance (unless the waters of the marshes were very high), they used to return up the ditches, fill the basins, and inundate the country to the foot of the mountains; and with a north-west wind, the waves used to penetrate with force to the interior. The mixture of fresh and salt water thus formed, and which, in summer, was rarely changed, became corrupt, and spread infection over the neighbourhood of the most destructive kind.

“In this way the effects of the malaria were reproduced annually in the neighbouring country, with all their peculiar horrors: the population, though small, presented feeble infants and diseased men, old age being unknown there. All attempts to avoid the scourge, by living on the hills, or in the interior, and frequenting the plain when the business of cultivation essentially required it, were vain; they fell victims to the extensive influence; and such being the effects upon the

inhabitants of the country, much more rapidly did a stranger suffer from the deleterious atmosphere; one single night, in the months of August and September, causing inevitable death to the incautious traveller who should stay so long in this infested country.

“Such was the state of things until 1741. Previous to that time, Gemignano Rondelli, Eustachio Manfredi, and Bernardino Zondroni, had successively insisted upon the necessity of excluding the sea from these marshes; and, in 1740-41, a sluice, with folding doors, competent to give emission to the waters of the marsh, but prevent the sea from entering, was constructed at the mouth of the Burlamacca. The most complete and unexpected success immediately followed upon, and has continued with, this work. The year after its completion there were no appearances of the terrible maladies which previously appeared every year. The inhabitants soon recovered health, and the land being very fertile, the population rapidly increased, and is increasing at this moment. Viareggio has become a considerable town, and so completely has all suspicion of its insalubrity disappeared, that the first families of the city of Lucca have for years built their summer seats there. Notwithstanding the success of the precautions taken at this part of the coast, the neighbouring parts were long left a prey to the destroying influence of the mixed marsh-waters; and the inhabitants around the basins of Motrone and Perotto were not considered until the year 1804. In the years 1809, 1810, 1811, similar means were taken, with the best effects to the inhabitants of Montignosso and the vicinity; and, in 1812, a sluice was constructed on the Cinquale, which perfected the arrangements in this part, and made a large portion of country equally healthy with Viareggio. To complete the arrangement, it was now only required to guard the ditches of Motrone and Tonfalo with sluices; the former was finished in 1819, and the latter in 1821. Since that time the diseases of malaria have ceased so entirely at all points, that no other dangers are now incurred regarding the insalubrity of the atmosphere than such as may arise from neglect of these sluices, which the inhabitants of the country should regard as their palladium.”

I should weary you were I to multiply examples of the existence of the worst forms of malaria in places which we now know combine all the necessary circumstances for the generation of sulphuretted hydrogen.

In Mr. Darwin's interesting *Journal of the Voyage of the Adventure and Beagle*, he speaks of several such places, especially in Peru, in connexion with the well-known effects of miasma, but not with any suspicion of the real origin of the evil. He mentions repeatedly the efflorescence of the sulphates

of magnesia and soda upon the soil, and that the mud of the saline lakes is "black, and had a foetid odour." The inhabitants suffer in such situations from the worst attacks of ague. He remarks, p. 447, "The attacks of illness which arise from miasma never fail to appear most mysterious. The miasma is not always produced by a luxuriant vegetation with an ardent climate; for many parts of Brazil, even where there are marshes and a rank vegetation, are much more healthy than this sterile coast of Peru."

"So difficult is it to judge from the aspect of a country whether or not it is healthy, that if a person had been told to choose within the tropics a situation appearing favourable for health, very probably he would have named this coast."

Its peculiarity consists in the saline efflorescence upon its soil, of which the sulphates of magnesia and soda constitute a very great proportion.

May not the jungle fever in India depend for much of its malignity upon the same cause? The soil in many parts abounds not only with the nitrates of potassa and soda, but the sulphates of soda and magnesia; these become washed down by the periodical rains, and mingling with the decaying leaves, the mutual reaction takes place.

Is it not worthy again of the most exact inquiry, whether the fevers which periodically afflict the cities of New York and Charleston, in America, may not be connected with the mixture of animal and vegetable substances with the sea water in their lower districts, where they usually originate; and whether an attentive examination will not prove, that the same impregnation of sulphuretted hydrogen, which we have established upon the African coast, exists at the mouth of the vast rivers of the American continent? Indeed I have been informed by an officer high in the naval service, that during the war instances of the rapid decay of ship's copper, similar to that upon the African, were noticed upon the West-Indian station.

To bring the matter nearer home. Is it impossible that the minor insalubrity of parts of our own coast, such as that of Essex, may have some reference to the same causes acting in minor degrees? The mud of the river Thames may surely produce the same decompositions of the sulphates in the sea water as that which we have traced in other places. I find that there is a prevalent idea, which deserves investigation, that the ships in the Medway are liable to greater waste of their copper than those at Woolwich, or other places on the Thames.

Within the past week I have seen in the number of Liebig's *Annalen* for January last, that Dr. Clemm examined some water taken up in December 1839, in the open sea

off Barmouth, North Wales; also from Aberystwith and Tenby, in South Wales, and he observes, "In some of the bottles from the English coast the large proportion of sulphuretted hydrogen was very striking: both in smell and taste it was converted into a strong and sulphurous water." He seems to think that the accidental presence of some of the sealing-wax with which the bottles were closed, may have had something to do with the generation of the gas.

By the kindness of Sir Isambard Brunel, I have been favoured with the particulars of a very remarkable phænomenon of this kind, which attended the progress of the Thames Tunnel, and which proved of great annoyance, and was the cause of great suffering to some of the workmen. This was the evolution of great quantities of carburetted hydrogen mixed with sulphuretted hydrogen gases. In 1837 the smell was extremely foetid. "It frequently rushed out from between the small boards of the shield, used to scour the ground with great force, bursting into a flame, which sometimes extended over some feet, and was generally accompanied by a loud roaring or hissing noise."

In 1838, the sulphuretted hydrogen was unaccompanied by the great quantity of carburetted hydrogen with which it had been previously mixed, but its effects upon the men were very severe, producing sickness, giddiness and fainting, pains in the head, and in one case delirium.

The source of this enormous production of gas may probably have been the deposits of animal and vegetable matters in the mud of the river, acting however by the sulphates, which are contained in the former, in a manner perfectly similar to that which we have traced on the sulphates in the sea water.

And here again it may be asked, as with regard to the injurious effects of sulphuretted hydrogen upon the copper of ships, can science indicate a remedy, as well as point to the cause of the disease? And again I would reply, that by furnishing an easy method of detecting the evil, she furnishes you with timely warning to fly from the infected regions. No vessel should be allowed to cast anchor or linger in sulphuretted waters. But if paramount duty should oppose itself to such a course, we have a certain remedy to propose. You have seen how instantly chlorine destroys the gas. Chlorine and sulphuretted hydrogen cannot coexist together. Plentiful fumigations of chlorine would therefore infallibly prevent the deleterious effects; and the antidote is at once cheap, and incapable, under proper management, to produce any injurious effects to counterbalance its advantages.

The Lords of the Admiralty have received these suggestions with indulgence, and have

given instructions to their cruisers upon the African coast to test the waters at regular intervals. They have also abundantly supplied the African expedition with the means of chlorine fumigation ; and I have the gratification of knowing that the views which I have now had the honour of submitting to you, have tended to give confidence not only to the gallant band who have devoted themselves to one of the most disinterested enterprises which ever emanated from pure Christian charity, but to the numerous friends who wait the result with anxiety.

RECEIVED FOR REVIEW.

The Fourth Fasciculus of Anatomical Drawings, selected from the Collection of Morbid Anatomy in the Army Medical Museum at Chatham. Drawn on stone by George N. Ford. Taylor, Fleet Street.—(Some very valuable specimens of diseases of the heart and great vessels, capitally executed.)

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Friday, July 9.

John W. Griffith.—T. B. Flower.—Henry Cox Goodlake.—Peter Leigh.—Charles O. Woodford.—Thomas Osburne.—John O. Taylor.—Henry C. Stewart.—William Lane.—William Moorhead.—Thomas Stack.

Friday, July 16.

J. J. E. Porter, — J. Henderson. — E. W. Waters. — J. P. Lawrence. — B. Blaine. — R. Austin.—R. Pincott.—J. Scott —J. Jeffrer.

Monday, July 19.

D. K. Jones.—J. Coventry.—J. P. Porter.—J. Innes.—J. Jee.—E. Gregory.—W. Skinner.—T. J. Starling.—W. R. D. Salmon.—W. W. Gull.—D. Wheeler.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, June 24, 1841.

Thomas Fothergill M'Nay, Newcastle-upon-Tyne.—Wm. Newton, Newcastle-upon-Tyne.—Anthony Collins Brownless, Groundherst.—John Wilson Croker Pennell, Whitehaven.—John Williams, Swansea.—Samuel Secker, Wakefield.—George Thomas Gauntlett, Olney, Bucks.—John Godfrey Batcheler, Southwell, Notts. — John Lambert, Edinburgh.

Thursday, July 1.

James Penn Harris, Cowes, Isle of Wight.—John Ward, Sheerness.—George Cordy Edwards, Framlingham.

Thursday, July 8.

J. Prentice, Lowestoffe.—R. T. Tasker, Stratford-upon-Avon.—W. Stewart, Glasgow.—John M'Intyre, Muthill, Perthshire.—W. Smith, Scotland.—T. Robinson, South Park, near Hull.—James Ellison Whale, Peterborough.

Thursday, July 15.

John James, Hereford.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 10th July, 1841.

Small Pox	15
Measles	21
Scarlatina	18
Hooping Cough	39
Croup	2
Thrush	10
Diarrhoea	6
Dysentery	4
Cholera	0
Influenza	1
Typhus	25
Erysipelas	2
Syphilis	0
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	132
Diseases of the Lungs, and other Organs of Respiration.....	231
Diseases of the Heart and Blood-vessels	19
Diseases of the Stomach, Liver, and other Organs of Digestion	50
Diseases of the Kidneys, &c.....	6
Childbed	4
Ovarian Dropsy	0
Diseases of Uterus, &c.	2
Rheumatism	3
Diseases of Joints, &c.	2
Ulcer	0
Fistula	0
Diseases of Skin, &c	0
Diseases of Uncertain Seat	89
Old Age or Natural Decay.....	48
Deaths by Violence, Privation, or Intemperance	18
Causes not specified	8

Deaths from all Causes 755

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N. Longitude 0° 3' 51" W. of Greenwich.

July.	THERMOMETER.		BAROMETER.	
Wednesday 14	from 44 to 64		29·67 to 29·56	
Thursday . 15	46	64	29·57	29·69
Friday . . 16	50	62	29·80	29·92
Saturday . 17	44	66	29·94	29·85
Sunday . . 18	48	63	29·69	29·76
Monday . . 19	46	69	29·74	29·78
Tuesday . 20	53	63	29·58	29·50

Winds, S.W. and N.W. on the 14th ; W. and N. on the 15th ; S.E. and N.E. on the 16th ; S.E. on the 17th ; N. and W. on the 18th ; S.W. on the 19th and 20th.

On the 14th, generally cloudy ; rain in the afternoon. The 15th, morning clear, otherwise overcast ; a violent storm of thunder and vivid lightning, accompanied with heavy rain, from about one till seven p.m. The 16th, evening clear ; otherwise cloudy. The 17th, clear. The 18th, cloudy ; rain at times. The 19th, evening overcast ; otherwise clear. The 20th, general overcast, raining frequently during the day ; distant thunder about eleven a.m.

Rain fallen, 1 inch and ·07 of an inch.

CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, JULY 30, 1841.

LECTURES

ON THE

PRINCIPLES AND PRACTICE OF PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

—

LECTURE XLIII.
(Concluded.)

*Cretinism.—Medical and surgical treatment
of bronchocele.*

CRETINISM has a close, but an ill-understood, connexion with goitre. Wherever cretinism is endemic, bronchocele never fails to be abundant. But bronchocele may prevail in a place where there are no cretins. With but few exceptions, cretins are goitrous; whereas many of those who have bronchocele are not affected with cretinism. The two disorders either spring from the same cause, requiring for their joint production that this cause should be in active operation; or, if they have separate causes, these frequently co-exist and act in combination. It is said, I know not with what accuracy, that when both parents are goitrous for two generations in succession, the offspring, being in the third generation, are sure to be cretins. Certainly cretinism is most common where bronchocele is most common, and especially in mountainous places. It occurs in the Pyrenees as well as in the Alps, in the mountains of Syria, in the hilly parts of China, and in the Himalayan regions. Yet cretinism is confined within much more limited bounds than goitre. Saussure, Foderé, and Dr. Reeve, agree entirely as to the circumstances under which cretinism appears to be most commonly engendered in Switzerland. They say that the disease is usually met with in valleys which are nearly surrounded by high and steep rocks, where there is but little

circulation of air, and where the inhabitants are exposed to the direct rays of the sun, and to the reflection of them from the rocks; and also to effluvia from marshes. It is in the filthy habitations built in these close, hot, and humid situations, that cretinism abounds most. The children that are taken away from the low valleys, and carried up, when young, into the high grounds, escape the disease; or even get the better of it if removed soon enough. And the amendment is said to be perceptible even in a very few days. These facts have led many persons to conclude that cretinism, if not bronchocele, depends on some condition of the *air*. It appears to me probable that the exciting cause of both is the same, and that the local circumstances just now mentioned operate as predisposing causes only. Cretinism, as well as goitre, was observed, by Ramond, in the "open, well-watered, and well-ventilated valleys of the Pyrenees."

There are some difficulties opposed to the implicit reception of the opinions formed by Mr. McClelland and by others, respecting the origin of these diseases. And the facts upon which those opinions are grounded are not without apparent exceptions. Moreover, the actual substance which exercises or confers the noxious power has yet to be ascertained. This etiological problem, so full of interest, is not solved. One step more, and probably one step only, remains to be taken. We look to the medical geologist for its complete solution; and I trust that, now, we have not long to look. The deleterious agent has been traced, with tolerable certainty, to water: and thence to some element of the soil washed by that water. And if what at present is probable only, shall hereafter be proved,—namely, that the hidden cause of goitre and of cretinism lurks in some chemical quality of man's natural beverage—it can scarcely be doubted that chemistry will be found ready to supply a simple and effectual corrective of the evil.

This hope it is which makes it so important that medical men should be accurately possessed of the present state and bearings of the question; and prepared to take advantage of every opportunity that may arise for its practical determination. For surely it would be a noble achievement of our art, and a signal blessing provided for hundreds of human beings yet unborn, thus to prevent the deformity, the discomfort, and the sometimes danger, of bronchocele; and to forbid, in its very source and fountain, the more hideous and loathsome disfigurement, of mind as well as of body, that distinguishes the wretched cretin.

I ought, perhaps, to tell you, that other causes, many of them very vague and unsatisfactory, have, at different times, been assigned. Thus Valentin supposes the disease to be more common in women than in men, simply because women more frequently have the neck uncovered. It has been affirmed that young women who have taken the veil in catholic countries have lost their goitres in consequence of the change then made in their costume: and a medical man in Guatemala asserts that the same infirmity has sensibly diminished among the men in that part of the world, since cravats became fashionable there. These views of the matter accord with some whimsical notions entertained by the late Sir Anthony Carlisle. He held that the chief purpose of the thyreoid gland was to protect the delicate organ of the voice from the injurious influence of cold; and he consequently regarded the tumor of bronchocele as being rather of a sanative than of a morbid nature. He looked upon it as an additional greatcoat thrown over the important instrument of speech, in circumstances of extraordinary need. His theory agrees with some of the facts on which other theories, not perhaps more plausible, have been founded. Thus although snow water may not be, indeed I may say certainly is not, the cause of bronchocele from any peculiar quality that belongs to it, yet the disorder is confessedly frequent in many places where snow water is habitually drunk; *i. e.* as Sir Anthony would have reasoned, where *very cold* water is frequently drunk; the swelling being a provision of nature to obviate the hurtful effects which might else be produced on the larynx by these cold potations. The summer change for the better, described by Dr. Richardson, consists also with the same theory; which would suggest the covering an incipient bronchocele continually with warm clothing, such as flannel, to check its growth, to obviate its necessity, and so gradually to occasion its subsidence. And this plan is mentioned, I see, by Bouillaud, among the curative indications.

I mention these theories, not because I have any faith in them myself, but as being

curious specimens of the manner in which the human mind strives to account for obscure phænomena; and as shewing how readily facts may be culled and pressed into the service of very slender and infirm hypotheses.

Treatment of Bronchocele.—One very important point in the treatment is obvious; the removal of the patient, when that can be done, from the infested locality. When this has been effected, or when the disorder occurs sporadically, as it sometimes does, we may administer drugs with better hope and advantage. Now a great variety of empirical remedies have been recommended for the cure of bronchocele, concerning most of which it would be a waste of time for me to speak at all. The remedy which, as an internal medicine, has of late years superseded all others, and acquired the reputation of a specific against goitre, is iodine: and it certainly has a remarkable influence over the genuine unmixed form of the disease, whether endemic or sporadic—the hypertrophied gland: yet it does not, perhaps, merit the title of specific in the same absolute sense as that in which we say that bark is a specific for the ague, or sulphur for the itch. Dr. Coindet, of Geneva, was the first who made this remedy extensively known. Dr. Straub, however, of Hofwyl, has laid claim to the priority of its use. Probably that happened in this matter, which has often happened in others, viz. that the state of knowledge was ripe for such a discovery, and it was made by each of these physicians independently of the other. Dr. Coindet's name, however, has been inseparably connected with the application of iodine to the cure of bronchocele, in a work which, if he had no other claim to be remembered, would immortalize his memory;—I mean Sir J. Herschel's masterly and beautiful *Discourse on the Study of Natural Philosophy*;—a book which I should advise those among you who have not already read it, to make themselves master of as soon as they have leisure to do so. He thus strikingly illustrates a remark of his own that mighty benefits often accrue to science from the casual observation of even unscientific or illiterate persons. “A soap manufacturer remarks that the residuum of his ley, when exhausted of the alkali for which he employs it, produces a corrosion of his copper boiler, for which he cannot account. He puts it into the hands of a scientific chemist for analysis; and the result is the discovery of one of the most singular and important chemical elements—iodine. The properties of this being studied, are found to occur most appositely in illustration and support of a variety of new, curious, and instructive views then gaining ground in chemistry; and thus exercise a marked influence over the whole

body of that science. Curiosity is excited; the origin of the new substance is traced to the sea-plants from whose ashes the principal ingredient of soap is obtained; and ultimately to the sea-water itself. It is thence hunted through nature, discovered in salt-mines and springs, and pursued into all bodies which have a marine origin; among the rest into sponge. A medical practitioner, Dr. Coindet, of Geneva, then calls to mind a reputed remedy for the cure of one of the most grievous and unsightly disorders to which the human species is subject—the goitre—which infests the inhabitants of mountainous districts to an extent that, in this favoured land, we have happily no experience of, and which was said to have been originally cured by the ashes of burnt sponge. Led by this indication, he tries the effect of iodine on that complaint, and the result establishes the extraordinary fact that this singular substance, taken as a medicine, acts with the utmost promptitude and energy on goitre, dissipating the largest and most inveterate in a short time, and acting (of course, like all medicines, even the most approved, with occasional failures) as a specific, or natural antagonist, against that odious deformity.”

It is curious enough, and marks the accuracy with which the effects of remedies may be observed, that not only had burnt sponge been found efficacious in removing bronchocele, but the bladderwrack also, the *fucus vesiculosus*, the plant that, with others of the same family, yields the soda, with which the iodine was found combined. Dr. Gairdner, of Bolton Street, who was the first I believe in this country to write on the effects of iodine, accounts for the frequent failure of even large doses of the burnt sponge, by showing that it was much adulterated with charcoal. Dr. Manson, of Nottingham, has published the following tabular statement of the result of the employment of iodine by himself. He had treated 116 patients, of whom 15 were men, and the rest women. Of the 15 men, 10 were cured, three were improving and under treatment at the time of his publication, 1 was dismissed for irregular attendance, and 1 was much relieved. Of the 101 women, 66 were cured, 9 much relieved, 2 received no benefit, 10 were discharged for irregular attendance, and 14 were improving under treatment. Of the whole 116, therefore, there were 76 positive cures, or two-thirds of the entire number; and only two positive failures. Dr. Coindet was successful in about the same proportion of cases. This is strong evidence of the power of the remedy. Dr. Manson found that in some, but not in all individuals, after the preparations of iodine had been given internally for a certain time, they were apt to occasion headache, giddiness,

sickness of stomach, with some degree of languor, and inaptitude for exertion. His plan in such cases was to suspend the use of the medicine, or to reduce the dose. The following effects of the iodine are stated by Dr. Coindet as having occurred in his practice:—Acceleration of the pulse, palpitation, dry cough, watchfulness, marasmus, and prostration of strength; sometimes swelling of the legs, tremors, painful hardness of the bronchocele, diminution of the breasts, or a remarkable increase of appetite supervened; and he adds that in almost all the instances which he had observed, a very rapid diminution, or a disappearance more or less complete, took place, during these symptoms, of even hard, bulky, and old bronchoceles. His doses varied from somewhat less than a grain to somewhat more than a grain and a half. This was certainly, as Dr. Manson has suggested, too large a quantity of this powerful drug for the generality of patients. The management of the remedy is now better understood. I have never seen any of the unpleasant consequences enumerated by Dr. Coindet. These practitioners gave the iodine in the form of a tincture. But this is a bad mode of exhibiting it: for the tincture is decomposed in any aqueous menstruum, and the iodine thrown down to the bottom. Under such circumstances the patient may at one time get no iodine at all, and at another time a dangerous dose: for you are aware that iodine is an active irritant poison. But if you mix iodine with iodide of potassium, it is then *held* in solution; and this is the form in which it is now commonly given. In the last edition of the *Pharmacopœia* there is a *Liquor Potassii Iodidi compositus*, made by dissolving five grains of iodine, and ten of the iodide of potassium, in a pint of distilled water. This will doubtless, in future, be much prescribed. It is a dilute preparation. There is one grain of iodine in four ounces of the liquor. An ounce would, therefore, be a very *safe* dose. But you may begin with a couple of drachms, and increase the quantity, if need be, gradually: for not only Dr. Manson, but Dr. Copland also, who states that the remedy has been very successful in his hands, advocates *small*, unirritating, what are sometimes oddly called *alterative* doses. Simple friction is said to have had the effect of diminishing the tumor; and friction with some ointment or liniment containing iodine should be employed whenever the internal exhibition of the remedy is contraindicated; or *in addition* to its internal use. There is an authorized form for that purpose also in the New *Pharmacopœia*—the *Unguentum Iodinii Compositum*. A small portion of this may be rubbed upon the tumor night and morning. I need not add that besides great care in watching for the specific ill

effects of the iodine upon the system, no less care is to be taken in correcting the state of any other function which may be faulty, and in improving the general health.

Such is the best *medical* treatment of bronchocele: and such is the plan which you will always do well to make cautious trial of in the first instance: and with respect to surgical treatment, I may observe that so long as the disease is merely a deformity, so long as it does not interfere with any of the important functions of the body, nor produce serious discomfort—does not distress the respiration by pressing upon the trachea, nor impede deglutition by pressing upon the oesophagus, nor derange to any great degree the circulation through the head by pressing upon the great blood-vessels of the neck, nor grievously encumber the patient by its weight—we should not be justified (in my opinion) in performing or advising any surgical operation for the removal or the diminution of the tumor. Of such operations the three principal are, extirpation of the whole gland; the passing a seton through the tumor, and so exciting suppuration in it, by which its substance is broken down and destroyed; and tying the arteries which supply it with blood. The first of these methods, extirpation, has been performed when the wen was small; but few patients under such circumstances would consent to the operation; and few surgeons would now-a-days, I presume, advise or undertake it: and in cases where it might seem more expedient, that is, where the swelling is large, and suffocation is threatened by its pressure on the trachea, the execution of this measure would be exceedingly difficult and dangerous; for the arteries are so much dilated in these cases that perilous hæmorrhage might be expected from their division, especially when their extreme vicinity to the carotids is considered. Such large goitres come to involve also, by their lateral extension, very important nerves there situated. In one case where excision was attempted, the hæmorrhage was so alarming that the surgeon was obliged to desist in the middle of his task: and the patient actually died of hæmorrhage a few days afterwards. I believe that this operation may be considered as wholly abandoned by surgeons in the present day.

The introduction of a seton into the tumor has been more successful. This practice was revived somewhat more than twenty years ago by Dr. Quadri, of Naples; who supposed, indeed, that he had been the first to devise it. You will find an account of his mode of proceeding in the tenth volume of the *Medico-Chirurgical Transactions*, by Dr. Somerville. The seton is passed through the substance of the gland, and retained there a considerable time: the chief caution necessary is to avoid the enlarged blood-

vessels. Dr. Quadri affirms that the larger trunks of the thyreoid arteries will not be endangered unless the seton needle be passed so deeply as almost to touch the thyreoid cartilage; and that hæmorrhage from any of the branches of those arteries that may be wounded when the seton is inserted more superficially, will not be attended with hazard. This plan was followed in one case by ulceration and sloughing, and the patient died. An example of the success of this treatment occurred in the practice of Mr. James, of Exeter; the tumor was almost entirely removed, and the patient was for some time in London for the purpose of shewing himself to the medical men here. But he also had passed through a good deal of suffering and peril. The expedient of tying one or more of the thyreöideal arteries, and so *starving* the tumor, has been attended with varied success. It has been done on the continent; and several times in this country: there is a case of it by Mr. Coates, of Salisbury, in the same volume of the *Medico-Chirurgical Transactions*. His patient was much relieved for a time, and supposed herself cured. But the tumor gradually returned, and caused her death by suffocation. Sir B. Brodie has also performed a similar operation; and I have myself seen one very satisfactory instance of the same thing, the operation having been done by Mr. Earle in St. Bartholomew's Hospital. The patient was a young woman with an immense bronchocele, which for some time previously had obstructed her breathing, and of which the effect on the trachea seemed to be every day increasing. There was loud wheezing, and great difficulty of respiration, and cough, and extreme emaciation: and it was plain that the girl must soon die suffocated if nothing were done for her relief. One of the arteries, I think the superior thyreöideal, was tied, without much difficulty, on one side. After the operation the tumor on that side shrunk considerably; the distress of breathing was removed; and the patient presently recovered so much of her former strength, and flesh and comfort, that she was unwilling to have any thing farther done, and left the hospital. After some time, however, she came back again of her own accord, and requested that the artery on the other side might be tied also. This was accordingly done, and a farther reduction of the tumor took place. The shrinking, however, if I remember rightly, was not so striking or complete after the second operation as after the first: but the patient was certainly rescued, upon the whole, from imminent death, and put into a condition of ease and at least temporary safety; the tumor that remained constituting merely a deformity. I never heard of her afterwards. In a case very similar to this, related by Mr. Craw-

ford in the *Cyclopaedia of Practical Medicine*, Mr. Wickham, of Winchester, tied one of the arteries, with much immediate, but no ultimate benefit. The goitre gradually diminished for about six weeks, and then (in consequence, I presume, of the establishment of a collateral arterial circulation) it as gradually enlarged again till it was as big as ever.

Upon the whole, the *average* results of none of these surgical expedients have been sufficiently good to warrant their repetition, except in cases where life is put in jeopardy or made miserable by the swelling; and where other methods, and particularly the treatment by iodine, have been tried, and have failed. One exception, perhaps, I should here make. The tumor sometimes evidently contains a collection of fluid, either in one of its enlarged cells, or in a distinct cyst. Now the cell, or cyst, may in such cases be punctured, and the contained fluid let out, without much risk. This was done in one instance by my colleague, Mr. Arnott. He kept the orifice open; and the cyst shrunk, and was at last obliterated, and the woman was much gratified by this diminution of her load.

LECTURES
ON THE
FUNCTIONS OF THE NERVOUS
SYSTEM.

BY W. B. CARPENTER, M.D.

LECTURE VIII.

Functions of the Cephalic Nerves.

BEFORE proceeding to inquire into the functions of the different parts of the encephalon, it seems desirable to bring together what is known in regard to the functions of the nerves specially connected with them; so that, by tracing their connections, we may be able to obtain some light upon this very obscure, though most interesting and important subject.

That the first pair, or *olfactory* nerves, minister to the sense of smell, has long been known; yet it could not be predicated without experimental inquiry, that it is *not* a conductor of the impressions which produce ordinary sensation, nor that it is destitute of power of exciting muscular movement, either by direct or reflex action. The anatomist who studies this nerve in the Mammalia only, would not attain a proper idea of its character. When we examine it in Fishes, especially those of the cartilaginous division, we observe that the fibres which proceed to the organ of smell originate in two ganglionic masses, that are quite distinct both from the hemispheres and the optic ganglia;

and these are sometimes separated from the mass of the brain by peduncles of considerable length. On comparing this arrangement with that which we meet with in the higher Vertebrata, we see that what are commonly regarded as the trunks of the olfactory nerves are really the peduncles, by which the bulbous expansions lying on the cribriform plate of the ethmoid bone,—in other words, the olfactory ganglia,—are connected with the rest of the encephalon and with the medulla oblongata. As no other nerves proceed from these ganglia, however, than those distributed to the organ of smell, it is evident that experiments upon what are commonly regarded as the trunks of the olfactory nerves, will afford information nearly as satisfactory, in regard to the functions of the nerves proceeding from the bulbs, as if they were really so. Anatomical examination of the distribution of this nerve proves that it is not one which directly conveys motor influence to any muscles; since all its branches are distributed to the membrane lining the nasal cavity. Experimental inquiry leads to the same result; for no irritation of the peduncles or branches excites any muscular movement. Further, no irritation of any part of this nerve excites reflex actions through other nerves. Again, it is not a nerve of common sensation; for animals do not exhibit any sign of pain when it is subjected to any kind of irritation. Neither the division of the nerve, or the destruction of the olfactory ganglia, seems to inconvenience them materially. They take their food, move with their accustomed agility, and exhibit the usual appetites of their kind. The common sensibility of the parts contained in the olfactory organ is in no degree impaired, as is shewn by the effect of irritating vapours; but the animals are destitute of the sense of smell, as is shewn by the way in which these vapours affect them. At first they appear indifferent to their presence; and then suddenly and vehemently avoid them, as soon as the Schneiderian membrane becomes irritated. Moreover, if two dogs, with the eyes bandaged, one having the olfactory nerves and ganglia sound, and the other having had them destroyed, are brought into the neighbourhood of the dead body of an animal, the former will examine it by its smell; whilst the latter, even if he touches it, pays no attention to it. This experiment Valentin states that he has repeated several times, and always with the same results. Further, common observation shews that sensibility to irritants, such as snuff, and acuteness of the power of smell, bear no constant proportion to one another; and there is ample pathological evidence that the want of this sense is connected with some morbid condition of the olfactory nerves or ganglia. It is well known that Magendie has maintained, that the fifth

pair in some way furnished conditions requisite for the enjoyment of the sense of smell; asserting that, when it is cut, the animal is deprived of this. But his experiments were made with irritating vapours, which excite sternutation or other violent muscular actions, not through the olfactory nerve, but through the fifth pair; and the experiments of Valentin, just related, fully proves that the animals are not sensitive to *odours*, strictly so called, after the olfactory has been divided. It is by no means improbable that the acuteness of the sense of smell may be diminished by section of the fifth pair; since the olfactory membrane is no longer duly moistened by its proper secretion; and, when dry, it is not as susceptible of the impressions made by those minute particles of odoriferous substances, to which the excitement of the sensation must be referred.

That the second pair, or *optic nerves*, have an analogous character, appears alike from anatomical and experimental evidence. No chemical or mechanical stimulus of the nerve produces *direct* muscular motion; nor does it give rise, so far as it can be ascertained, to indications of pain; whence it may be concluded, that this nerve is not one of common sensation. That the ordinary sensibility of the eyeball remains when the functions of the optic nerve are completely destroyed, is well known; as is also the fact, that division of it puts an end to the power of vision. Valentin states that, although the optic nerve may, like other nerves, be in appearance completely regenerated, he has never been able to obtain any evidence that the power of sight has been in the least degree recovered. He remarks that animals suddenly made blind exhibit great mental disturbance, and perform many unaccustomed movements; and that the complete absence of the power of vision is easily ascertained. Morbid changes are sometimes observed to take place in eyes whose optic nerve has been divided; but these are by no means so constant or so extensive as when the fifth pair is paralyzed; and they may not improbably be attributed to the injury occasioned by the operation itself to the parts within the orbit. It is well known that, when amaurosis is produced by a morbid condition of the optic nerve alone, the eye retains its usual appearance; but, if the amaurosis be complete, the texture of the retina undergoes a remarkable change, ceasing to exhibit that peculiar structure which normally characterizes it. Neither primitive nervous fibrils, nor nucleated globules, can be distinguished in it; and the yellow spot of Soemmering becomes paler, and is at last undistinguishable. But if a very slight degree of sensibility to light remain, these changes are much less decided.

Further, it is well known that, when the sight is destroyed by a disease or injury which prevents the passage of light through the pupil, the whole eye becomes more or less atrophied; and the retina and optic nerve, although previously sound, are found after death (if the morbid condition have lasted sufficiently long) to have lost their characteristic structure. It seems evident, then, that the continuation of the functional operations of nerves is a necessary condition of the maintenance of their normal organization; and we can very well understand that this should be the case, from the analogy of other parts of the system.

The optic nerve, though analogous to the olfactory in all the points hitherto mentioned, differs from it in one important respect;—that it has the power of conveying impressions which shall excite reflex muscular motions. This is especially the case in regard to the iris, the ordinary actions of which are regulated by the degree of light impinging on the retina. When the optic nerve is divided, a contraction of the pupil takes place; but this does not occur if the connection of this nerve with the third pair, through the nervous centres, be in any way interrupted. After such division (if complete), the state of the pupil is not affected by variations in the degree of light impinging on the retina, except in particular cases in which it is influenced through other channels. Thus, in a patient suffering under amaurosis of one eye, the pupil of the affected eye is often found to vary in size in accordance with that of the other eye; but this effect is produced by the action of light on the retina of the sound eye, which produces a motor change in the third pair on both sides. Further, as has been formerly stated, the *impression* only of light upon the retina may give rise to contraction of the pupil, by reflex action, when the optic nerve is itself sound, whilst no sensations are received through the eye in consequence of disease in the sensorial portion of the nervous centres. Another cause has been pointed out by Valentin for the influence of light in causing contraction of the pupil, and *vice versa*;—that, if the rays impinge upon the iris, a reflex stimulation is produced through the fifth pair; and he remarks that the susceptibility of the iris to this kind of influence seems much increased after the optic nerve has been divided. Besides the contractions of the pupil, another action, which has been sometimes spoken of as reflex, is produced through the optic nerve,—the contraction of the orbicularis. But this cannot be produced by any mechanical stimulation, and it evidently involves *sensation*; in fact, it is a movement of an *emotional* kind, produced by the painful effect of light, which gives rise to the condition well characterized by the

term *photophobia*. The involuntary character of it must be evident to every one who has been engaged in the treatment of diseases of the eyes; and the effect of it is aided by a similarly involuntary movement of the eyeball itself, which is rotated upwards and inwards to a greater extent than the will appears able to effect.

It will be convenient next to advert to the *auditory* nerve; the functions of which are easily determined, by anatomical examination of its distribution, and by observation of pathological phenomena, to be analogous to those of the two preceding. Atrophy or lesion of the trunk destroys the sense of hearing; whilst irritation of it produces auditory sensations, but does not occasion pain. From experiments made upon the nerve before it leaves the cranial cavity, it appears satisfactorily ascertained that this nerve has no motor power, either of a direct or reflex character, and that it is not endowed with common sensibility. It is interesting to remark, that microscopic examination of its structure clearly indicates its intermediate character, between the nerves of special sensation issuing from the anterior part of the cranium,—namely, the optic and olfactory,—and those whose function is to minister either to common sensation, or to that of taste, which approaches nearly to it,—namely, the fifth pair and the glosso-pharyngeal, which issue from the posterior part of the encephalon, and are more nearly analogous with the spinal nerves. The primitive fibres are not so soft as those of the olfactory, nor so slender as those of the optic; and they are softer than those of the glosso-pharyngeal. Moreover, the auditory nerve forms a plexus with the facial, to which there is no analogy in the optic and olfactory nerves, but to which a similar one exists in the glosso-pharyngeal. This intermediate structural character is interesting when we compare it with the intermediate character of the function; for the impressions made upon it are produced through the vibrations of a material fluid,—instead of being, as in the case of sight, the result of changes so subtle as to be almost inscrutable to our means of research,—or, as in the case of taste and touch, being produced by the direct contact of the substance which gives rise to the sensation.

Passing by for the present the motor nerves of the orbit, as constituting a distinct subject for future inquiry, we may advantageously proceed with the other sensory nerves connected with the encephalon. It should be noticed, however, that the third pair, or motor oculi, certainly possesses some degree of sensibility, as is evidenced by the signs of pain given by the animal when it is cut or compressed; but this sensibility is not nearly so great as that of the fifth pair; and it may be doubted whether it is

possessed by it in virtue of its direct connection with the nervous centres, or whether it does not derive it by an anastomosis with the fifth pair, some filaments of which may pass backwards as well as forwards, so as to confer sensibility on the third pair, both before and after their junction with it. No sensory fibres can be proved to exist in the fourth and sixth nerves.

We next come to the fifth pair, the true nature of the functions of which was ascertained in part by Sir C. Bell; his views receiving modification, however, from the experimental researches of others. As formerly stated, it possesses two distinct sets of roots, of which one is much larger than the other; on the larger root, as on the posterior root of the spinal nerves, is a distinct ganglion; and the fibres arising from the smaller root do not blend with the others, until after the latter have passed through this ganglion. The trunk of the nerve separates, as is well known, into three divisions,—the ophthalmic, the superior maxillary, and the inferior maxillary; and it can easily be shewn by careful dissection, that the fibres of the smaller root pass into the third of these divisions alone. When the distribution of this nerve is carefully examined, it is found that the first and second divisions of it proceed almost entirely to the skin and mucous surfaces, a very small proportion only of its fibres being lost in the muscles; but of the branches of the third division, a large part are distinctly muscular. Hence analogy, and the facts supplied by anatomical research, would lead to the conclusion, that the two first divisions are nerves of sensation only, and that the third division combines sensory and motor endowments. Such an inference is fully borne out by experiment. When the whole trunk is divided within the cranium (as Magendie, by frequent practice, has been able to accomplish), evident signs of acute pain are given. After the incision has been made through the skin, the animal remains quiet until the nerve is touched; and when it is pressed or divided, doleful cries are uttered, which continue for some time, showing the painful effect of the irritated state of the cut extremity. The common sensibility of all the parts supplied by this nerve is entirely destroyed on the affected side. The jaw does not hang loosely, because it is partly kept up by the muscles of the other side; but it falls in a slight degree, and its movements are seen, when carefully observed, to be somewhat oblique. If the trunk be divided on each side, the whole head is deprived of sensibility; and the animal carries it in a curious vascillating manner, as if it were a foreign body.

If the anterior or *ophthalmic* branch only be divided, all the parts supplied by it are found to have lost their sensibility, but their

motions are unimpaired; and all experiments and pathological observations concur in attributing to it sensory endowments only. The only apparent exception is in the case of the naso-ciliary branch; since there is good reason to believe that the long root of the ciliary ganglion, and the long ciliary nerves, possess motor powers; but these appear to be derived from the sympathetic nerve. When the whole nerve, or its anterior branch, is divided in the rabbit, the pupil is exceedingly contracted, and remains immovable; but in dogs and pigeons it is dilated. The pupil of the other eye is scarcely affected; or, if its dimensions be changed, it soon returns to its natural state. The eyeball speedily becomes inflamed, however; and the inflammation usually runs on to suppuration and complete disorganization. The commencement of these changes may be commonly noticed within twenty-four hours after the operation; and they appear to be due to the want of the protective secretion, which (as will be explained when the direct influence of the nervous system upon the organic functions is considered) is necessary to keep the mucous surface of the eye in its healthy condition, and which is not formed when the sensibility of that surface is destroyed.

The *superior maxillary* branch, considered in itself, is equally destitute of motor endowments with the ophthalmic; but its connections with other nerves, through the sphenopalatine ganglion and its anastomosing twigs, may introduce a few motor fibres into it.

The *inferior maxillary* branch is the only one which possesses motor as well as sensory endowments from its origin; but its different subdivisions possess these endowments in varying proportions, some being almost exclusively motor, and others as completely of a sensory character. The latter is probably the nature of the lingual branch; and there seems good reason to believe, as will presently be shown, that this ministers not only to the tactile sensibility of the tongue, but to the sense of taste. The muscles put in action by this branch of the fifth pair are solely those concerned in the masticatory movements.

The *portio dura* of the *seventh* pair, or facial nerve, has been supposed, since the first researches of Sir C. Bell, to be a nerve of motion only; but some recent physiologists have maintained that it both possesses sensory endowments, and arises by a double root. The latter assertion is quite fallacious; and the most carefully-conducted experiments do not bear out the former. By exposing the roots of the seventh pair within the cranium, Valentin ascertained that it possesses no sensory endowments at its origin; since, when these were touched, the animals gave no signs of pain, though violent muscular movements were excited in the

face. Subsequently to its first entrance into the canal by which it emerges, however, it anastomoses with other nerves; and thus sensory fibres are introduced into it from many different sources,—anteriorly, from the fifth pair, and posteriorly, from the cervical nerves—which cause irritation of various of its branches to produce pain. The number and situation of the anastomoses vary much in different animals; so that it is impossible to make any very comprehensive statement in regard to them. Experimental researches leave no doubt that the seventh pair is the general motor nerve of the face, ministering to the influence of volition and of emotion, and also being the channel of the reflex movements concerned in respiration and other associated movements of the muscles, but not being in the least concerned in the act of mastication.

The functions of the *glosso-pharyngeal* nerve have been heretofore alluded to in part; but there still remain several questions to be discussed in regard to them. Reasons have been given for the belief that it is chiefly, if not entirely, an afferent nerve, not having any *direct* power of exciting muscular contraction, but conveying impressions to the medulla oblongata, which produce reflex movements of the other nerves. Some experimenters assert, that they have succeeded in exciting direct muscular actions through its trunk. This is by no means impossible; but if the truth of the statement be admitted, it does not invalidate the inferences regarding the *general* function of the nerve, deduced by Dr. Reid from minute anatomical investigation, and from a large number of experiments.

Much controversy has taken place on the question, whether this nerve is to be regarded as ministering, partly or exclusively, to the sense of taste; and many high authorities have ranged themselves on each side. The question involves that of the function of the lingual branch of the fifth pair; and it is partly to be decided by the anatomical relations of the two nerves respectively. The glosso-pharyngeal is principally distributed on the mucous surface of the fauces, and on the back of the tongue. According to Valentin, it sends a branch forwards, on either side, somewhat beneath the lateral margin, which supplies the edges and inferior surface of the tip of the tongue, and inosculates with the lingual branch of the fifth pair. On the other hand, the upper surface of the front of the tongue is supplied by this lingual branch. The experiments of Dr. Alcock, whose conclusions are borne out by Dr. J. Reid, leave little doubt in my own mind, that the gustative sensibility of *this* part of the tongue is due to the latter nerve, being evidently impaired by division of it. Moreover, cases are by no means

rare, in which the gustative sensibility of the anterior part of the tongue has been destroyed, with its tactual sensibility, when there was no reason to suppose that any other than the fifth pair of nerves was involved. On the other hand, it is equally certain that the sense of taste is not destroyed by section of the lingual nerve on each side; and it seems also well ascertained that it is impaired by section of the glosso-pharyngeal nerve. Considering how nearly allied is the sense of taste to that of touch, and bearing in mind the respective distribution of these two nerves, it does not seem difficult to arrive at the conclusion, that both nerves are concerned in this function; and there seems good reason to believe that the glosso-pharyngeal is the one through which the impressions made by disagreeable substances taken into the mouth are propagated to the medulla oblongata, so as to produce nausea and to excite efforts to vomit.

The *par vagum*, as stated in a former lecture, is certainly to be considered, in regard to its trunk at least, as a nerve of double endowments. The chief function of its afferent portion is to convey to the medulla oblongata the impression produced by venous blood in the capillaries of the lungs, or of carbonic acid in the air-cells: this impression may give rise, as we have seen, to the respiratory movements, without producing sensation; but, if from any cause stronger than usual, the sense of uneasiness which it occasions is very distressing. This impression may be imitated by pressure on the nerve, which induces an immediate inspiratory movement. That the nerve is capable of conveying those impressions, which become *sensations* when communicated to the sensorium, is further evident from the fact that, when its trunk is pinched, the animal gives signs of acute pain. Besides the pulmonary impressions, this nerve also conveys to the medulla oblongata those which originate in the mucous surface of the larynx, trachea, and bronchi, as well as on the lower part of the œsophagus and the stomach. The purpose of these is to stimulate various movements, which are performed through the motor portion of the trunk. If the experiments of Valentin are to be relied on, this portion is derived, as formerly stated, from the *roots* of the spinal accessory nerve; which, according to him, are purely motor, whilst those of the *par vagum* are purely afferent. Whether or not this be true, it is certain that the trunk of the *par vagum* contains a large number of motor filaments, through which are excited the actions of the muscles of the pharynx and larynx, of the œsophagus, and in some degree, of the stomach and respiratory tubes.

The section of the *par vagum* produces, as would readily be expected, great disorder

of the functions of respiration and digestion, to which it ministers. It is an operation which has been very frequently performed; and the statements of its results vary considerably amongst each other, being generally influenced, in some degree, by the preconceived views of the experimenter. I shall employ, as in my opinion the most worthy of confidence, the experiments of Dr. J. Reid, on whose accuracy I have strong personal reasons for placing reliance, and whose anatomical and pathological attainments are such as to render him fully competent for the task. The section of the *par vagum*, when practised with the view of ascertaining the influence of the nerve upon the lung and stomach, is usually made in the neck, between the origins of the superior and inferior (or recurrent) laryngeal branches. Hence the muscles of the larynx are paralyzed; and, if the animal should struggle violently, the ingress of air is likely to be obstructed by the flapping down of the arytenoid cartilages, and by the closure of the glottis. This is especially the case in young animals, in which the larynx is small. But in those that are full grown, and have a large larynx, an adequate quantity of air may still find its way through the aperture, if the animal refrain from any violent effort. In a considerable number of Dr. Reid's experiments, therefore, he did not find it necessary to introduce the trachea tube, which other experimenters have generally employed; an opening was made into the trachea, however, in those instances in which, from any cause, the entrance of air was obstructed.

The real character of the morbid changes in the lungs, which are induced by cutting the *par vagum*, the order in which they arise, and the causes to which they are immediately due, constitute very interesting subjects of investigation; and the knowledge of them will probably throw light upon many ill-understood morbid phenomena. In the first place, it has been fully established by Dr. Reid, that section of the *vagus* on one side only does not necessarily, or even gradually, induce disease of that lung; and hence the important inference may be drawn, that the nerve does not exercise any *immediate* influence on its functions. When both *vagi* are divided, however, the animal rarely survives long; but its death frequently results from the disorder of the digestive functions. Sometimes, however, the power of digestion is restored sufficiently to re-invigorate the animals; and their lives may be prolonged for a considerable time. In fifteen out of seventeen animals experimented on by Dr. Reid, the lungs were found more or less unfit for the healthy performance of their functions. The most common morbid changes were a congested state of the blood-vessels, and an effusion of

frothy serum into the air-cells and bronchial tubes. In eight out of the fifteen, these changes were strongly marked. In some portions of the lungs, the quantity of blood was so great as to render them dense. The degree of congestion varied in different parts of the same lung; but it was generally greatest at the most depending portions. The condensation was generally greater than what could be accounted for by the mere congestion of blood in the vessels, and probably arose from the escape of the solid parts of the blood into the tissue of the lung. In some instances the condensation was so great, that considerable portions of the lung sunk in water, and did not crepitate; but they did not present the granulated appearance of the second stage of ordinary pneumonia. In five cases in which the animals had survived a considerable time, portions of the lungs exhibited the second, and even the third stages of pneumonia, with puriform effusion in the small bronchial tubes; and in two gangrene had supervened.

One of the most important points to ascertain in an investigation of this kind, is the first departure from a healthy state;—to decide whether the effusion of frothy reddish serum, by interfering with the usual change in the lungs, *causes* the congested state of the pulmonary vessels and the laboured respiration; or whether the effusion is the *effect* of a previously congested state of the blood-vessels. The former is the opinion of many physiologists, who have represented the effusion of serum as a process of morbid secretion, directly resulting from the disorder of that function produced by the section of the nerve; the latter appears the unavoidable inference from the carefully-noted results of Dr. Reid's experiments. In several of these, only a very small quantity of frothy serum was found in the air-tubes, even when the lungs were found loaded with blood, and when the respiration before death was very laboured. This naturally leads us to doubt, whether the frothy serum is the cause of the laboured respiration, and of the congested state of the pulmonary vessels, in those cases where it is present; though there can be no doubt that, when once it is effused, it must powerfully tend to increase the difficulty of respiration, and the impeded circulation through the lungs. Dr. Reid has satisfied himself of an important point, which has been overlooked by others,—that this frothy fluid is not mucus, though it is occasionally mixed with it, but is the frothy serum so frequently found in cases where the circulation through the lungs has been impeded before death. From this and other facts, Dr. Reid concludes “that the congestion of the blood-vessels is the first departure from the healthy state of the lung, and

that the effusion of frothy serum is a subsequent effect.”

The next point, therefore, to be inquired into, is the cause of this congestion; and this is most satisfactorily explained, upon the general principles regulating the circulation of the blood, by remembering that section of the par vagum greatly diminishes the frequency of the respiratory movements, and that the quantity of air introduced into the lungs is, therefore, very insufficient for the due aeration of the blood. I think that it may be regarded as one of the best established principles in physiology, that the activity of the changes which the blood undergoes in the capillary vessels, does, in some way or other, regulate its movement through them;—that, when these changes are proceeding with activity, the capillary circulation is proportionably accelerated;—and that, when they are abnormally low in degree, the movement of the blood in the capillaries is stagnated. There is now abundant evidence, in regard to the pulmonary circulation in particular, that, to prevent the admission of oxygen in the lungs, either by causing the animal to breathe pure nitrogen or hydrogen, or by occlusion of the air passages, is to bring the circulation through their capillaries to a speedy check. Hence we should at once be led to infer, that diminution in the number of respiratory movements would produce the same effect; and as little or no difference in their frequency is produced by section of one vagus only, the usual absence of morbid changes in the lung supplied by it is fully accounted for. The congestion of the vessels, induced by insufficient aeration, satisfactorily accounts, not only for the effusion of serum, but also for the tendency to pass into the inflammatory condition, sometimes presented by the lungs, as by other organs similarly affected. Dr. Reid confirms this view, by the particulars of cases of disease in the human subject, in which the lungs presented after death a condition similar to that observed in the lower animals after section of the vagi; and in these individuals, the respiratory movements had been much less frequent than natural during the latter part of life, owing to a torpid condition of the nervous centres. The opinion (held especially by Dr. Wilson Philip) that section of the par vagum produces the serous effusion by its direct influence on the function of secretion, is further invalidated by the fact stated by Dr. Reid,—that he always found the bronchial membrane covered with its true mucus, except when inflammation was present.

“The experimental history of the par vagum,” it is justly remarked by Dr. Reid, “furnishes an excellent illustration of the numerous difficulties with which the physio-

logist has to contend, from the impossibility of insulating any individual organ from its mutual actions and reactions, when he wishes to examine the order and dependence of its phenomena." In such investigations no useful inference can be drawn from one or two experiments only; in order to avoid all sources of fallacy, a large number must be made; the points in which all agree must be separated from others in which there is a variation of results; and it must be then inquired to what the latter are due. These observations apply equally to the other principal subject of inquiry in regard to the functions of the par vagum,—its influence upon the process of digestion. The results obtained by different experimenters have led to differences of opinion as to its action, no less remarkable than those on the question just discussed. Dr. Wilson Philip has long maintained that the par vagum controlled the secretion of gastric juice, which he stated to cease when the nerve is divided; and he further stated, that the influence of galvanism, propagated along the nerve, would re-establish the secretion. This statement has been quoted and re-quoted, as an established physiological position; and, when united with the well-known fact, that galvanism would excite muscular contraction, it has seemed to Dr. W. Philip and other physiologists sufficient to establish the important position, that galvanism and nervous influence are identical. The statement, however, has been disputed by many other experimenters, who have satisfied themselves that the secretion of gastric juice continued, but have attributed the impairment of the digestive power, which is certainly a result (for a time at least) of the operation, to the paralysis of the muscular coat of the stomach.

The experiments of Dr. Reid do not furnish grounds for positive conclusions on the subject; but they furnish important correction of the results obtained by others. He has succeeded, as formerly stated, in producing movements of the stomach, by irritations of the vagi; but that these movements may be excited in other ways is evident from the fact that, in several of his experiments, food was digested and propelled into the duodenum subsequently to the operation. The same fact, which he appears to have fully substantiated, is an incontrovertible proof, that the secretion of gastric juice is *not dependent* on nervous influence supplied by the par vagum, though doubtless in part regulated by it. The first effects of the operation, however, are almost invariably found to be vomiting (in those animals sensible of it), loathing of food, and arrestment of the digestive process; and it is not until after four or five days that the power seems

re-established. In the animals which died before that time, no indication of it could be discovered by Dr. Reid; in those which survived longer, great emaciation took place; but when life was sufficiently prolonged, the power of assimilation seemed almost completely restored. This was the case in four out of the seventeen dogs experimented on; and the evidence of this restoration consisted in the recovery of flesh and blood by the animals, the vomiting of half-digested food permanently reddening litmus paper, the disappearance of a considerable quantity of alimentary matter from the intestinal canal, and the existence of chyle in the lacteals. It may serve to account in some degree for the contrary results obtained by other experimenters, to state that seven out of Dr. Reid's seventeen experiments were performed before he obtained any evidence of digestion after the operation; and that the four which furnished this followed one another almost in succession; so that it is easy to understand why those who were satisfied with a small number of experiments should have been led to deny it altogether.

It may be hoped, then, that physiologists will cease to adduce the oft-cited experiments of Dr. Wilson Philip, in favour of the hypothesis (for such it must be termed) that secretion is dependent upon nervous influence, and that this is identical with galvanism. Additional evidence of their fallacy is derived from the fact mentioned by Dr. Reid, that the usual mucous secretions of the stomach were always found; and they are further invalidated by the testimony of Müller, who denies that galvanism has any influence in re-establishing the gastric secretion, when it has been checked by section of the nerves. Another series of experiments was performed by Dr. Reid, for the purpose of testing the validity of the results obtained by Sir B. Brodie, relative to the effects of section of the par vagum upon the secretions of the stomach, after the introduction of arsenious acid into the system. According to that eminent surgeon and physiologist, when the poison was introduced after the par vagum had been divided on each side, the quantity of the protective mucous and watery secretions was much less than usual, although obvious marks of inflammation were present. In order to avoid error as much as possible, Dr. Reid made five sets of experiments, employing two dogs in each, as nearly as possible of equal size and strength, introducing the same quantity of the poison into the system of each in the same manner, but cutting the vagi in one, and leaving them entire in the other. This *comparative* mode of experimenting is obviously the only one admissible in such an investigation. Its result was in every instance opposed to the statements of Sir B.

Brodie; the quantity of the mucous and watery secretions of the stomach being nearly the same in each individual of the respective pairs subjected to experiment; so that they can no longer be referred to the influence of the eighth pair of nerves. Moreover, the appearances of inflammation were, in four out of the five cases, greatest in the animals whose vagi were left entire; and this seemed to be referrible to the longer duration of their lives after the arsenic had been introduced. The results of Sir B. Brodie's experiments may perhaps be explained by the speedy occurrence of death in the subjects of them, consequent (it may be) upon the want of sufficiently free respiration, which was carefully guarded against by Dr. Reid.

So far as the results of Dr. Reid's experiments may be trusted to, therefore, (and I am myself disposed to rely on them almost implicitly), all the arguments which have been drawn, in favour of the doctrine that secretion depends upon nervous agency, from the effects of lesion of the vagi upon the functions of the stomach, must be set aside. That this nerve has an important *influence* on the gastric secretion, is evident from the deficiency in its amount soon after the operation, as well as from other facts. But this is a very different proposition from that just alluded to; and I shall employ Dr. Reid's very happy illustration of the difference. "The movements of a horse," he observes, "are independent of the rider on his back,—in other words, the rider does not furnish the conditions necessary for the movements of the horse;—but every one knows how much these movements may be influenced by the hand and heel of the rider."

It only remains to notice the influence of section of the vagi upon the actions of the heart. It has been already stated that mechanical irritation of these nerves, especially at their roots, has a tendency to excite or accelerate the heart's action. It remains to inquire if its movements are dependent upon its influence, or if it is the channel through which they are affected by emotions of the mind, or by conditions of the bodily system. In regard to the first point, no doubt can be entertained; since the regular movements of the heart are but little affected by section of the vagi. In regard to the second, there is more difficulty; since the number of causes which may influence the rapidity and pulsations of the heart is very considerable. For example, when the blood is forced on more rapidly towards the heart, as in exercise, struggling, &c. the stimulus to its contraction is more frequently renewed, and they become more frequent; and when the current moves on more slowly, as in a state of rest, their frequency becomes proportionably diminished. If the contractions of the heart were not

dependent upon the blood, and their number were not regulated by the quantity flowing into its cavities, very serious and inevitably fatal disturbances of the heart's action would soon take place. That this adjustment does not take place through the medium of the nervous centres is evident from the fact that, in a dog, in which the par vagum and sympathetic had been divided in the neck on each side, violent struggling, induced by alarm, raised the number of pulsations from 130 to 260 per minute. It is impossible to ascertain, by experiment upon the lower animals, whether simple emotion, unattended with struggling or other exertion, would affect the pulsation of the heart after section of the vagi; but, when the large proportion of the ganglionic nerves proceeding to this organ are considered, and when it is also remembered that irritation of the roots of the upper cervical nerves stimulates the action of the heart through it, we can scarcely doubt that both may serve as the channels of this influence, especially in such animals, as the dog, in which the two freely inosculate in the neck.

In regard to the functions of the *spinal accessory* nerve, also, there has been great difference of opinion; the peculiarity of its origin and course having led to the belief that some very especial purpose is answered by it. We shall first examine what evidence of its character may be obtained from its anatomy only. Its filaments come off from the middle column of the spinal cord, most frequently as low down as the origins of the sixth and seventh cervical nerves. In its course upwards to the foramen magnum, it lies between the posterior roots of the spinal nerves, and the ligamentum denticulatum. It sometimes receives filaments from these roots, and is generally connected especially with the first cervical; according to Bellingeri, however, who has paid great attention to the subject, the filaments coming from the posterior roots do not form part of the trunk of the nerve, but leave it again to enter the posterior root of the first cervical. It may be doubted whether this is entirely true; as some experiments appear to shew that the spinal accessory is in some degree a sensory nerve, even at its roots. As the trunk passes through the foramen lacerum, it divides into two branches; of which the internal, after giving off some filaments that assist in forming the pharyngeal branch of the par vagum, becomes incorporated with the trunk of that nerve; whilst the external proceeds outwards, and is finally distributed to the sterno-cleido-mastoideus and trapezius muscles, some of its filaments inosculating with those of the cervical plexus. It is interesting to remark, that the junction of the anterior branch with the par vagum

beyond the point at which the latter swells out into its superior ganglion, increases the analogy, which has been sustained upon other grounds, between the compound trunk thus formed, and that of the spinal nerves; the par vagum being regarded as the sensory root, and the spinal accessory as the motor. According to Valentin, however, there is not a mere passage of filaments from the spinal accessory to the par vagum, but an absolute interchange—the trunk of the former containing some sensory fibres derived from the latter. When the roots of the spinal accessory are irritated, as appears from the experiments of Valentin, no decided indications of sensation can be obtained; but all the motor actions of the par vagum manifest themselves. When the external branch is irritated, before it perforates the sterno-mastoid muscle, vigorous convulsive movements of that muscle, and of the trapezius, are produced: and the animal does not give any signs of pain, unless the nerve is firmly compressed between the forceps, or is included in a tight ligature. Hence it may be inferred that the functions of this nerve are chiefly motor, and that its sensory filaments are few in number. Further, when the nerve has been cut across, or firmly tied, irritation of the lower end is attended by the same convulsive movements of the muscles; whilst irritation of the upper end in connexion with the spinal cord is unattended with any muscular movement. Hence it is clear that the motions occasioned by irritating it are of a direct, not of a reflex character. The same muscular movements are observed on irritating the nerve in the recently-killed as in the living animal. According to Sir C. Bell, the spinal accessory is a purely respiratory nerve, whose office it is to excite the involuntary or automatic movements of the muscles it supplies, which share in the act of respiration; and he states that the division of it paralyses the muscles to which it is distributed, as muscles of respiration, though they still perform the voluntary movements through the medium of the spinal nerves. Both Valentin and Dr. Reid, however, positively deny that this is the case. Dr. Reid's method of experimenting was well adapted to test the truth of the assertion. Considering that, in the ordinary condition of the animal, it might be difficult to distinguish the actions of particular muscles, beneath the skin, when those in the neighbourhood were in operation; and also that the usual automatic movements might be simulated by voluntary action, when the breathing might be rendered difficult; he adopted the following plan:—A small dose of prussic acid was given to an animal in which the spinal accessory had been previously divided on one

side; and after the convulsive movements produced by it had ceased, the animal was generally found in a state similar to that which we sometimes see in apoplexy; the action of the heart going on, and the respirations being slow and heaving, and the sensorial functions appearing to be completely suspended. The respiratory movements always ceased before the action of the heart; but they continued, in several of the animals experimented on, sufficiently long to allow the muscles of the anterior part of the neck to be laid bare, so that accurate observations could be made upon their contractions. In the dog and cat the sterno-mastoid does not appear to have much participation in the ordinary movements of respiration; for in several instances it could not be seen to contract on either side, though the head was forcibly pulled towards the chest at each inspiratory movement, chiefly by the action of the sterno-hyoid and thyroid muscles. In two dogs and one cat, however, in which the head was fixed, and these respiratory movements were particularly vigorous, distinct contractions were seen in the exposed sterno-mastoid muscles, synchronous with the other movements of respiration: these were, perhaps, somewhat weaker on the side on which the nerve had been cut, but were still decidedly present. In one of these dogs, similar movements were observed in the trapezius, on the side on which the nerve had been divided. As the condition of the animal forbade the idea that volition could be the cause of these movements, it can scarcely be questioned that Sir C. Bell's statement was an erroneous one. As far, therefore, as these experiments afford any positive data in regard to the functions of this nerve, it may be concluded that they are the same as those of the cervical plexus, with which it anastomoses freely. "Future anatomical researches," as Dr. Reid justly remarks, "may perhaps explain to us how it follows this peculiar course, without obliging us to suppose that it has a reference to any special function in the adult of the human species." The study of the history of development has accounted satisfactorily for the peculiar course of the recurrent laryngeal, which may be traced passing *directly* from the par vagum to the larynx, at a time when the neck can scarcely be said to exist, and when that organ is buried in the thorax. As this rises in the neck, the nerve, which at first came off below the great transverse blood-vessels, has both its origin and its termination carried upwards; whilst it is still tied down by these vessels in the middle of its course.

ON
SOME POINTS IN THE PATHOLOGY
OF THE CIRCULATION.

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*Being the substance of the Gulstonian
Lectures for 1841.*

[Concluded from p. 683.]

THE foregoing experiments and considerations lead to the conclusion, that the most essential character of inflammation consists in an increased motion or determination of blood to the affected part, with a tardy or obstructed flow through the part; the force of the increased motion being partly expended in the arterial portion of the dilated capillaries, and partly diverted into the collateral channels, so abundantly supplied by the anastomoses of vessels. The obstruction in the vessels of an inflamed part, we found reason to ascribe in part (especially at its commencement) to the increased mass in the smaller vessels, and the diminished elasticity of their coats; and in part also (and this is the chief cause in severe or confirmed inflammations) to the unusual formation of colourless globules, which adhere to the walls of the tubes and to each other.

It must be held in mind that the artificial mode by which we excite inflammation, by the application of irritants, is not the mode by which it is more usually excited in disease. The direct and intropulsive action of cold, and the sudden suppression of secretions or discharges, are the most common causes of inflammation; and these we have already found to produce simple congestion. To convert this congestion into inflammation, the additional element, determination of blood to the part, is necessary; and this local determination frequently arises out of the excitement of re-action which ensues after the operation of the causes of inflammation just noticed.

But if, according to the views now advanced, congestion, determination of blood, and inflammation, owe their first distinctive characters to definite physical differences in the quantity of the blood, and the distribution of its motion, we should be led to expect that these differences will vary much in

their degrees: that there may be intermediate conditions between these several states; and that one condition may pass into another. For example, that congestion, by the super-addition of determination of blood, may be converted into inflammation: that determination of blood, by overdilating the capillaries, may become inflammation: and the converse, by removing from incipient inflammation, the element determination of blood, we may reduce it to simple congestion; or in slighter cases, by removing its congestive part, by constringing the capillaries, we may convert it into simple determination of blood.

Such results should we deduce from the foregoing principles; and need I point out how exactly these are the results which disease and its treatment are daily exhibiting to us? It would be tedious, and is not necessary, to dwell on the more acknowledged of these results; but there are a few which have been little noticed or explained, which are yet of common occurrence, and sufficiently simple when tried by this key to vascular pathology.

Every one in the habit of studying disease, especially with the aid of physical signs, must have remarked how frequently congestions, even from mechanical causes, diseases of the heart, gravitation, or vascular obstruction, become converted into inflammations. Take a few examples:—

The lungs become congested from obstructive or regurgitant disease of the mitral or aortic orifice: the congestion is hypostatic, occupying the postero-inferior parts of the lungs: dyspnoea is induced: the heart is excited to increased action: its left cavities are comparatively empty: the right, and the pulmonary artery, are full: determination is thereby added to congestion, which thus becomes pneumonia. Hence, after death from such diseases of the heart, parts of the lungs are almost always found in the engorged or hepatized stages of inflammation, and occasionally with traces of purulent infiltration. The inflammatory character of these conditions is even more equivocally seen in films of recent lymph on the serous lining of the chest. The same appearances are met with after death preceded by prolonged coma from apoplexy or narcotism; and they were found by Dr. J. Reid in animals dead from the fatal

dyspnœa induced by dividing the eighth nerves. By impairing the respiration these causes immediately produce congestion in the lungs; but in an organ so vascular and so intimately associated with the heart, congestion rarely remains long without causing more or less re-action; imperfect it may be in these states, but sufficient to evolve from the congestion those low inflammatory products that are met with in these cases. So, too, as the function of respiration is commonly much impaired in fevers and various other diseases, in which the sinking which precedes death is prolonged, here too we find traces of pneumonia (termed by Laennec *pneumonia agonisantium*) which must be referred to the same category.

If in other organs the conversion of congestion into inflammation is less common than in the lungs, this must be referred to their being more remote from the heart, the source of motion. But instances are not wanting: in the brain after congestive apoplexy, and in the mucous membranes after the cold or congestive stage in fevers, inflammations arise out of congestions.

What other, in fact, is the operation of *cold* in exciting internal inflammations? Cold contracts the superficial vessels, and thus repels the blood to the internal organs, which become thereby congested. At the time of re-action after the cold, increased momentum is communicated to the congested vessels through the enlarged arteries. If the tone of the congested vessels be not much impaired, the new motion may excite them to contract, which will remove the congestion and the irregular distribution of force which it occasions; and this event will be more likely to occur as the re-action, or renewed distribution of blood, is general, and not confined to the congested vessels. This precisely corresponds with the phenomena and results of healthy re-action after exposure to cold. In cases where the vessels have been longer congested, they may not be relieved without a partial discharge of their contents, in form of watery secretion or flux, or a hæmorrhage, which equally accompanies the re-action. But if the tone of the capillaries be more deeply impaired, (and this will depend much on the degree and duration of the congestion), the determination of blood does not remove it, but converts it into inflamma-

tion, the phenomena of which shew themselves, and its products result.

As these several states, congestion, determination of blood, and inflammation, may pass into each other, so there are intermediate conditions, which it is difficult to refer to either by their phenomena, but which are better known by their results. This leads me to consider briefly, in conclusion, the local effects or results of these three states.

As the functions of all parts of the body are sustained by a due supply of blood through them, so these functions may be variously modified by differences in this supply: diminished supply reducing them; increased supply exalting them within certain limits; and an altered distribution changing and perverting their constituent elements. These elements are the vital properties—*contractility, sensibility, and sensorial powers, the excito-motory, and sympathetic property, and those of secretion, nutrition, and absorption*, added to *physical properties*. By thus considering them in the abstract, we shall be enabled to sketch the general effects of congestion, determination, and inflammation. The memory and observation of any one familiar with disease, will readily supply examples to complete the picture.

Congestion generally impairs contractility and sensibility, but by distension may cause pain and even spasm. It sometimes disturbs other nervous properties, causing various morbid irritations or sympathies, but in an uncertain manner. It tends to impair natural secretion; the secernent portions of the vessels no longer receiving the due force of the circulation*; but there may be increased transudation from the whole distended capillaries, causing watery saline, and slightly albuminous

* It may seem that this is taking too mechanical a view of the process of secretion; but be it remembered, that I do not ascribe secretion to any mechanical agency, but only assert what is known to be the fact, that a due force of the circulation is a condition favourable to this process. In lately inspecting the beautiful injected preparations of Mr. Dalrymple and Mr. Toynbee, I was particularly struck with the distribution of the capillaries of secreting surfaces, such as synovial and the simpler mucous membranes. These capillaries proceed pretty straight from the minute arteries, and end in loops and ampullæ on the surface, the returning vessels passing back as straightly. The physical effect of this provision is obviously to direct the chief force of the circulation on those loops and terminal sacs, which appear to be the seat of the secreting process.

exhalation, as in the case of flux and dropsy. In some cases of highly congested lungs and heart from structural disease of the latter organ, I have found, after death, the liquor sanguinis in the sacs of the pericardium and pleura, spontaneously separating into fibrinous clot and serum after removal from the body. Congestion in time affects the slower process of nutritive secretion, causing an increased deposit; or hypertrophy. Thus organs long congested at last increase in weight; this is seen in the enlargements of the spleen and liver under the influence of malaria, and by the increased substance of most internal organs in connection with morbus cordis. The latter example has been long familiar to me; and it has been lately established metrically by Dr. Clendinning. But the hypertrophy is not uniform: it affects chiefly the softest and most distensible parts of the structure; hence there is change as well as increase. Granular degeneration of the kidney, and some analogous changes in the liver and other organs, I believe to originate with congestion. It is unnecessary to remark, that hæmorrhage may be a result of congestion.

Determination of blood generally exalts contractility, sensibility, and other properties of the nervous system; causing excitement, spasm, pain, irritation, and sympathetic disorder. It tends to increase natural secretion; and thus we have fluxes consisting in excess of the natural secretion, as in the case of mucous and urinary fluxes, &c. The nutritive function is likewise increased, and more naturally than from congestion; the result being more simple hypertrophy. The process of absorption, although favoured by the current being increased without distension, is often not equal to the effusion; hence, in sacs and cells, determination of blood may cause dropsy.

Inflammation combines many of the results of congestion and determination, with others peculiar to itself. It at first much exalts sensibility and contractility; causing tenderness, and by tension pain, spasm, or rigid contraction. But the obstructed circulation, which follows, causes suspension of these properties in the centre of the mischief; whilst in surrounding parts, the seat of determination, they are still exalted. The sympathetic rela-

tions of the inflamed part are also commonly increased: the chief of these is with the heart and vascular system, the irritation of which is the chief constituent of inflammatory fever; others, more local, are those naturally associated with the organ inflamed. These nervous irritations connected with inflammation, often supersede others which existed previously.

Inflammation either suspends natural secretion, or changes it by adding various modifications of the serous and albuminous parts of the blood. It may be presumed that these are exuded chiefly by the arterial portion of the capillaries, the coats of which continue to receive the full tensive beat of the heart's pulse. Such effusions most abound where the arterial enlargement and its force are greatest, and the capillary obstruction most complete, as in acute sthenic inflammation. But the effect will also vary according to the structure of the part inflamed. Thus in complex secreting organs, such as glands, inflammatory injection and interstitial effusion very soon impair the mechanism of the organ, and may stop the secretion. In less complex structures, such as mucous membranes, the secretion is not stopped, but rendered saline and albuminous, by added modifications of serum or liquor sanguinis, in form of mucus, lymph, or pus. In the simplest textures, again, as in serous or cellular membrane, the secretion or effusion differs from the natural in its augmented quantity, as well as in the increased proportion of its fibrinous contents, which appear as gelatinising or coagulable lymph and serum, or pus globules, or both.

It might be questioned whether the slow process of nutrition would be changed under an influence so rapid in its effects as that of acute inflammation; yet analogy favours the supposition that the effusion of albuminous matter just described is a kind of exaggeration and overflow of the natural nutritive secretion, appearing on the exterior of simple membranes, and in the interstices of the more complex, and of parenchymata, causing thickening and consolidation. This view is strengthened by the fact that the other part of the renovating process of nutrition, absorption, is also augmented, diminishing the cohesion of the texture: hence the softening caused by acute

inflammation. In chronic inflammation, on the other hand, the removal of the old molecules does not precede the firm cohesion of the more abundant new: the result is a tough or hard thickening or consolidation.

But in considering the solid products of inflammation, we must not forget that their character and future history may depend less on the vessels than on the *quality of the blood*—another important element in the processes of secretion and nutrition. This, independently of the degree of vascular action, may determine whether the product shall be *plastic*, as organizable lymph; or *aplastic*, as pus or opaque tubercle. It will also be concerned in determining the degree of plasticity of the lymph; whether *enplastic* (Lobstein), susceptible of the same or a kindred degree of organization with the tissue the vessels of which have effused it: or *cacoplastic*, susceptible of only a degraded vitality, contractile, dense, and scarcely or not vascular,* nourished by organic molecular imbibition from adjoining more living parts: of this description are fibro-cartilage, cirrhosis, granular tubercle, and the hard tough formations allied to it. This subject, full of interest and practical importance, is too extensive to be considered here.

To revert to the constituents of inflammation. We have hitherto considered its results in the arterial portion of the capillaries which receives the impulsive force of the blood. But what takes place in the portion beyond, which is the seat of obstruction? If the obstruction be complete, from coagulation, the part, no longer receiving

its supply of blood, dies*. If it be of small extent, it is dissolved by the chemical agency of the animal fluids, as any other dead organised matter introduced into living structures (Prout); and being dissolved is absorbed away, giving place to the new products effused by the still active arterial portions, these products being lymph or pus; whence *ulceration*, *suppuration*, and *abscess*. If the dead portion be of larger extent, it is dissolved only on its circumference, and thus separated from the living parts in the form of a *slough*. And if it be to a still greater extent, the dissolving and separating process will not be accomplished before decomposition ensues, which produces the changes called *gangrene* and *sphacelus*.

Some of the preceding arguments proceed on the assumption that absorption is still active; how is this warranted by the known condition of the parts? All the vessels are in a state of distension except the veins beyond the obstruction, and the lymphatics. These are free, and by the motion communicated to them from the current of the neighbouring anastomosing branches, they are ready to convey away all the fluids that can pass through their coats. Such are the thinner fluids, and by their solvent action we have supposed some of the solids may likewise pass away: but not so the large globules of pus. On this point I cannot speak from observation, as the only animals in which I have traced the phenomena of inflammation are frogs, in which pus is never formed. But we can readily understand why pus globules, when once outside the vessels (howsoever they get there), cannot, from their size, pass through the coats of absorbent vessels, veins, or lymphatics; but must press on, and cause the death, solution, and absorption, of adjoining tissues. Direct observations are wanted on these points. Those of Gendrin have not been confirmed by others; and those of Henle and Mandl have not sufficient reference to the accompanying state of the vessels.

It would be easy to confirm the preceding views by showing their conformity with the detailed phenomena of disease, and especially with the

* There is reason to suppose that nutrition, especially in the less vital structures, is less closely associated with the blood-vessels than it is commonly supposed to be. Some investigations recently made by my friend, Mr. Toynbee, are very interesting on this point. I made a small pin hole in a frog's web; the capillaries that were divided yielded no blood, and became obstructed; but the circulation continued, although sluggishly, in those adjoining, which were distinct from the hole the length of six or eight diameters of the blood discs. The next day the circulating vessels were no nearer, yet the hole was partly filled up; and on the third day it was completely so; yet no moving blood could be seen near it. On the fifth day the distribution of visible vessels was not altered, but the matter with which the hole was filled had contracted and become opaque, so that the adjoining vessels were drawn nearer together, and the opacity prevented my seeing whether any passed through the cicatrix. The process of nutrition is probably wrought by living molecules (cytoblasts); the vessels only conveying the material.

* A somewhat similar view of the process of suppuration has been advanced by Dr. Billing. (Elem. of Med.)

known results of treatment; and one of its greatest recommendations, is that it explains and simplifies much that has been considered anomalous in the action of remedies. But this occasion does not admit of these illustrations; and it were well to wait for fuller confirmation of some of the observations on which these views have been chiefly founded.

CONTRIBUTIONS
TO THE
CHEMICAL PATHOLOGY OF SOME
FORMS OF MORBID DIGESTION.

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[*For the London Medical Gazette.*]

[Continued from p. 648.]

HAVING examined somewhat in detail the general physical and chemical properties of saliva secreted in health, as well as under the influence of the stimulus of certain abnormal actions, we have acquired such an amount of knowledge regarding the differential chemical characters of some of its principal modifications, as to be enabled, in the majority of cases, to recognise the existence of the salivary secretion when it constitutes a considerable proportion of a fluid poured out from the mouth, providing the amount of aqueous constituents as above, and the diminished specific gravity, is not too considerable. To make this necessarily imperfect knowledge of the chemical and physical characters of the saliva available in distinguishing the source of the fluid vomited in certain forms of dyspepsia, it will be necessary to examine the general properties of the other colourless fluids which are poured out by the secerning apparatus connected with the œsophageal and gastric mucous membrane. These secretions may be divided into two kinds: the one, which is found on the surface of the lining membrane of the mouth, fauces, pharynx, œsophagus, and stomach, a mucous covering, and under the influence of irritation or inflammation becoming frequently increased in quantity to an enormous extent, often to several pints in the twenty-four hours:

this constitutes the true mucous secretion. The second species includes the true gastric juice which is alone poured out in health, under the influence of mechanical irritation, as when pebbles or pepper-corns are swallowed, and generally from the stimulus of food. Great obscurity has been thrown over the chemistry of digestion, by the two secretions, gastric juice and gastric mucus, having been confounded with each other. Their totally distinct nature has been now so satisfactorily shewn by the researches of Dr. Beaumont, and other physiologists, that an excuse can scarcely be offered for confounding them with each other. It is, indeed, peculiarly important to attend to this distinction, as it explains very many important discrepancies existing between the results obtained by the earlier physiologists; for several certainly completely failed in their attempts to produce the phenomena of artificial digestion, from their supposing every fluid in the stomach to consist of gastric juice, whereas it is, as we now know, nothing but mucus, except under the circumstance of irritation before referred to.

The chemical characters of true gastric mucus are of great importance in determining the question of the source of the fluid of pyrosis, &c., especially as many lines of distinction, totally unwarranted by the nature of the secretion, have been drawn between closely allied, if not identical affections: thus, in a late ingenious monograph on this subject, an hypothesis has been hazarded that "pyrosis is a disease of the terminal exhalants, stomach-gleet, or one of the follicular glands;*" thus making, as indeed appears from the context, the fluid of pyrosis to be of a serous, and that of gastrorrhœa of a mere mucous character: this distinction, we shall have occasion to shew, is not by any means borne out by the two secretions. The great difficulty of procuring gastric mucus in sufficient quantity for accurate examination, has very materially interfered to prevent our obtaining an accurate acquaintance with its properties and composition. From what few results have been obtained on this subject, it appears probable that gastric, œsophageal, and bronchial mucus, closely resemble each

* Treatise on Pyrosis Idiopathica. By Thomas West, M.D. 1841, page 32.

other: the chemical properties of the latter were, many years ago, most elaborately examined by Dr. Pearson, and since then by many others. An account of my own observations has been already made public*, and hence it is unnecessary to relate them in this place.

When the stomach is free from food, the observations of Beaumont have proved that a glairy viscid mucus bathes its surface; and, in some cases, I feel very confident that this increases in quantity, and is thrown up with a very slight effort, although accompanied by a sensation of nausea. Certain it is that a mucous matter, totally distinct from simple bronchial mucus, is occasionally voided from a fasting stomach, when too little evidence of local action is present to lead to the conclusion of its being the result of diseased action. It has happened to me to observe this most frequently in the nausea so frequent in certain forms of cerebral disease; and it is by no means unfrequent to find this in people who have suffered from hemiplegia, and other forms of paralysis.

CASE VII.—Elizabeth Pearson, æt. 11, came under my care, at the Dispensary, for epilepsy following an attack of arachnitis a year before. The fits occurred nearly weekly; the mind was unaffected, and the body well developed. I ordered her half a grain of the ammonio-sulphate of copper to be taken thrice daily, under which treatment the fits ultimately vanished; the dose of the drug being gradually raised to three grains. The first dose of the medicine was swallowed in the form of a pill, in the morning, about an hour before breakfast; it almost immediately produced a feeling of nausea, followed by the escape of an extremely viscid transparent fluid from the stomach: this invariably occurred whenever the pill was swallowed on an empty stomach, and being generally voided in one gush, was probably tolerably free from adventitious matter: the pill was frequently found in the vomited fluid.

The fluid thus obtained was above four fluid-ounces in quantity, nearly as limpid as water, but so extremely viscid as to form a continuous rope some inches in length, when poured from one vessel to another; it acted

energetically on reddened litmus paper, restoring its blue colour. The saliva of the patient, examined a short time after the escape of mucus from the stomach, was found to be perfectly neutral.

The specific gravity of the mucus was exactly 1.00267, being considerably below the average density of saliva. When submitted to the action of re-agents the following results were observed:—

a. Ebullition did not in the slightest manner affect the limpidity of the fluid.

b. Chlorine gas, transmitted in a current of minute bubbles through the fluid, rapidly produced a copious white pulverulent deposit, rendering the whole as opaque as cream. This deposit was soluble in a few drops of a solution of potass, and in every character appeared identical with the precipitate obtained by passing the gas through saliva and bronchial mucus, and could not be distinguished from the coagulated albumen thrown down from serum of blood by alcohol, or by carbonic acid from an alkaline albuminate.

c. Hydrochloric acid, produced a slight trembling.

d. Nitric acid, also rendered the fluid slightly opaque.

e. Sol. potassa, no change.

f. Sol. alum, an opacity.

g. Proto-chloride of tin, slight opacity.

h. Nitrate of lead, dense precipitate.

i. Sesqui-chloride of iron; orange-yellow colour, disappearing instantly on the addition of a drop of hydrochloric acid.

k. Sulphate of copper, dense opacity.

l. Bichloride of mercury, white troubling.

m. Nitrate of silver, copious caseous precipitate.

n. Alcohol, dense, white troubling.

o. Tincture of galls, copious precipitate.

p. A weak voltaic current, produced the same effect as chlorine; an evolution of that gas, as shewn by its peculiar odour, accompanying the change.

On comparing the results thus obtained, by adding the same re-agents to gastric mucus as we used in examining saliva, we cannot fail to remark that there exists a very close analogy between the two secretions, for nine of the reagents (*b, e, f, g, h, k, l, m, o,*) acted in an analogous manner on

* Guy's Hospital Reports.

both fluids. Yet certain discriminating marks between them certainly exist, although not so well defined as might be desired; thus ebullition, which does not disturb the limpidity of healthy saliva, renders the gastric mucus opaque; nitric acid also acts in a similar manner. Proto-chloride of tin throws down a copious precipitate from every form of saliva (see Table, page 646), whilst it merely produces a slight opacity when added to gastric mucus; whilst the converse holds good with tincture of galls, which copiously precipitates the mucus, and but slightly affects saliva. Nitrate of silver also produces a much denser precipitate with mucus than with saliva. It is also very remarkable that the specimen of saliva of the lowest specific gravity I have met with, (column C, page 646) 1·0043, was infinitely more fluent than this viscid mucus of much less density, or 1·0027.

The cause of the orange colour produced by the addition of the per-salt of iron, is doubtful; it cannot justly be attributed to sulphocyanogen from an admixture of saliva, for we have seen that all evidence of the presence of this matter, from the addition of a salt of iron, nearly disappears when the density of the fluid is below 1·0043; and we have seen that the amount of reddening produced in saliva by a salt of iron is nearly in a ratio with its specific gravity. These circumstances, in addition to the *instant* destruction of the colour on the addition of a minute quantity of hydrochloric acid, warrants the assumption that we must regard alkaline lactates or acetates present as the source of the orange-yellow colour.

When gastric mucus is secreted in large quantity, under the influence of stomach or cerebral irritation, we can procure it in abundance, and readily submit it to chemical examination; and it is very remarkable how little these specimens of mucus, obtained from many different individuals, differ among themselves; in fact, whether secreted under the irritation of slight dyspepsia, or the most severe pyrosis, the chemical properties of the mucous fluids are nearly identical.

The chief reason that has led to the generally received opinion of the various properties possessed by the mucous fluids discharged from the stomach in dyspepsia, is found in the accounts

given by patients of the sensation they experienced when the fluids were in the mouth. Thus I have repeatedly had the characters of icy-cold, burning-hot, bland and tasteless, intensely sour and corroding the teeth, brackish and salt, alkaline and pungent, all applied by different patients, or by one at different times, to fluids which were positively tasteless, and, chemically speaking, identical. How is this to be explained? I cannot help thinking that the more or less irritable state of the mucous membrane, and consequently of the sentient extremities of the nerves supplying it, is the real cause, and thus that a tasteless fluid may to a tolerably healthy membrane appear bland; whilst, under circumstances of irritation, it may impress the sensation of painful pungency upon the membrane. A parallel to the state here assumed is seen in the case of gonorrhœa, in which the urine produces a sensation of almost insufferable pungent heat in passing along the inflamed and irritated membrane of the urethra, although it can be proved not to differ in the slightest manner from fluid, which before the disease appeared, had flowed through the canal without producing the slightest uneasiness.

Perhaps the most remarkable instance of the little confidence that can be reposed in the account given by a patient of his own sensations, as indicative of the properties of a vomited fluid, is met with in those cases of gastrorrhœa in which the gastric secretion is said to be intensely sour, and to have set the teeth an edge; whilst, on dipping a piece of litmus paper into the supposed acid fluid, its perfect neutrality is shewn by the absence of alteration in the blue colour of the paper.

Acid or alkaline state of gastric colourless fluids.

These fluids, when vomited when fasting, or when very copious in quantity, are generally neutral, or faintly alkaline; but when voided from the stomach at other times, especially during the act of digestion, they become more or less acidulous, from an admixture of gastric juice; but in no instance have I ever seen them so acid as to excite effervescence on the addition of an alkaline carbonate, and in general the quantity of free acid present is so minute as but just to redden delicately tinted litmus paper. In the colourless

fluid vomited in fourteen different cases, three were neutral, six alkaline, and five slightly acid, shewing a marked distinction between these secretions and those vomited in scirrhus pylorus, and some other important organic diseases of the stomach, in which the vomited fluids are generally brown, and so acid as to effervesce on the addition of a solution of carbonate of potass.

Specific gravity of the vomited fluids.

In the state of health, the density of gastric mucus obtained by vomiting is exceedingly low, and, as already stated, is about 1·0027. When, however, it is thrown out under the irritation of diseased action, its specific gravity re-

markably increases: the lightest fluid secreted under these circumstances was of specific gravity 1·0058; and the densest 1·0209, in a case of extensive ulceration of the œsophagus. The following table gives, at one view, the density of the vomited fluid, its acid or alkaline character, the age of the patients, and the more prominent disease under which they laboured, in fourteen cases. In all, I may remark, the chemical properties of the vomited fluids were so exactly alike as to lead to the conviction of their sometimes being nearly identical; differing probably more in the ratio of their dilution than in any thing else.

Name.	Age.	Duration of Vomiting.	Acid or alkaline state.	Sp. gravity.	Most prominent disease under which the patient laboured.
Eliz. Pearson	11	?	Alkaline	1·0027	Epilepsy following arachnitis.
Lucy Barker	52	3 years	Faintly alkaline	1·0062	Extensive emphysema of both lungs.
Lucy Barker	52	3 years	Faintly acid	1·0110	Extensive emphysema of both lungs.
Mrs. Barker	38	6 months	Alkaline	1·0128	Ditto; fluid vomited in the evening.
Mrs. Barker	38	6 months	Acid	1·0135	Ditto; fluid vomited in the morning.
Mary Davis	44	1 week	Acid	1·0058	Hemiplegia of left side.
Caroline Peters	25	10 years	Alkaline	1·0091	Irritable uterus; amenorrhœa.
Sarah Williams	25	9 months	Alkaline	1·0121	Menorrhagia.
Mary Clark.	47	3 weeks	Neutral	1·0121	Intense headache at the menstrual periods, followed by gastric discharge.
Peter Hanrits	39	4 years	Faintly acid	1·0110	Pyrosis, apparently produced by depressed ensiform cartilage.
Joseph Miles	64	9 months	Neutral	1·0091	Pyrosis; vomited in the evening.
Joseph Miles	64	9 months	Alkaline	1·0080	Pyrosis; vomited in the morning.
Eliz. Smith	31	1 month	Acid	1·0080	Pyrosis.
Miss S.	27	6 years	Neutral	1·0209	Ulceration of œsophagus; mechanical alteration in the position of stomach.

The mean density of the mucous fluids vomited in these cases is 1·0097, being considerably above the specific gravity of bronchial mucus, or of gastric mucus when collected under circumstances of as little local irritation as possible. The great density of the vomited fluids arises not from the increased quantity of animal matter present, but from the large proportion of saline matter; and, as will appear from the cases detailed in another part of this paper, something like a ratio appears to exist between the specific gravity of the vomited fluids and the amount of irritation or organic disease present. One circumstance of considerable importance appears pretty evident from the chemical characters of the fluids examined in the cases enumerated in the above table, viz. the absence of any

real ground of distinction between different cases, on account of the acid or alkaline state of the vomited fluids. To this point I shall have occasion hereafter to revert, for the purpose of shewing the futility of attempting to treat certain forms of dyspepsia on chemical principles, merely because the secretions happen to be acid or alkaline. In two cases inserted in the table we find the action of the fluids on litmus paper to differ when collected at different periods of the day; and thus, according to a lately proposed hypothesis, the patients might be said to have been labouring under acid dyspepsia in the morning, and alkaline in the evening.

Physical characters of the vomited fluids

These differ remarkably not only in

different cases, but often in specimens of fluid vomited at different periods of the day. In general the fluids are slightly opaline, generally capable of forming a continuous thread some inches in length when poured from one vessel to another, and sometimes so adhesive as to allow the reagents used to be mixed with extreme difficulty. It is not a little curious that the more ropy and viscid the secretion, the lighter, *cæteris paribus*, is its specific gravity; at least this so frequently occurs as to be considered as the general rule.

I have in no case of stomach affection attended with discharge of white glairy fluid, whether depending upon a functional or organic cause, observed the colour to change to brown, or even in consistence to assume the completely watery character of the dark fluids vomited in scirrhus pylorus. It is true that the discharge of white glairy fluid (gastrorrhœa, or stomach-gleet) is very constantly present in cases of this disease as an *avant-courier* of the characteristic brown fluid vomiting; but when once this has occurred, indicating a serious organic change in the organ, the simple glairy discharge seldom, if ever, re-appears. This is an interesting circumstance in relation to the diagnosis between cases of the simple follicular gastric dyspepsia and those attended with severe organic lesion. The most frequent change in the physical appearance of the vomited fluid, where no important organic mischief is present, is the appearance of a black inky matter diffused through it; this generally separates by repose, and on examination is found to consist of altered red particles. Hence the occurrence of this black vomit may be regarded as indicative of congestion of the stomach, and as constituting a form of hæmatemesis: it is seldom constant, and generally lasts but a few days, and often only a few hours.

ANALYSES AND NOTICES OF BOOKS.

“L'Auteur se tue à alonger ce que le lecteur se tue à abréger.”—D'ALEMBERT.

The Surgeon's Vade-Mecum. By ROBERT DRUITT. Second Edition, illustrated with 50 Wood Engravings. London, 1841. 8vo. pp. 524. Renshaw; Churchill.

THIS work merits our warmest com-

mendations, and we strongly recommend it to young surgeons, as an admirable digest of the principles and practice of modern surgery. It is divided into five parts: the first two are especially devoted to the principles, and the three others to the practice of surgery. We have looked completely over the most important chapters, and in all of them we find proofs of Mr. Druitt's practical knowledge, and of the tact with which he can give extremely instructive and yet condensed accounts of the various subjects that ought to be contained in a compendium of surgery.

MEDICAL GAZETTE.

Friday, July 30, 1841.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”

CICERO.

THE PHARMACEUTICAL SOCIETY.

WE have lately read, with great pleasure, the first number* of a new periodical, the “Pharmaceutical Transactions,” and a pamphlet which accompanied it, entitled “Observations on the Pharmaceutical Society,” by Jacob Bell. The spirit of these pamphlets is so accordant with that which we have long been anxious to see prevalent among the chemists and druggists, that it claims our almost unreserved approbation; and the statements which they contain respecting the condition of the pharmaceutical chemists agree closely with those on which, on more than a few recent occasions, we have founded our opinion of the urgent necessity of reform among them.

If we have ever spoken with harshness of chemists, it has only been with reference to those who, neglecting their legitimate occupation, interfere with the proper province of the licensed medical practitioner. And against these

* As this was passing the press, we received the Number for August, but too late to refer to it.

we shall never cease to offer strenuous opposition, unless they consent to undergo the ordeal of such an examination as may prove them to be competent to practise in the treatment of the minor medical and surgical affections. If they will do this, then let all those who, either for the sake of economy, or owing to the trifling nature of their disorders, would not employ the medical practitioner of the higher order, become the legitimate patients of the licensed chemist; let him treat them legally, and it may be judiciously, whom now he treats illegally and ignorantly. Indeed, we have little doubt that the course of a few years will see some system of this kind established: the public will not consent to be without cheap medical advice, good or bad, and if it is not to be found in the ranks of the regular profession, they will go to chemists, or any other persons who will offer it to them—it matters not with how great an amount of false pretensions.

But in all our remarks we have never sought to depreciate the character or the social position of the honest pharmaceutical chemist. On the contrary, we have always admitted that he now occupies at least a portion of the field that once almost peculiarly belonged to the medical profession—that, namely, of dispensing apothecary. From this portion of their occupation the great majority of general practitioners have retired; having raised themselves from it very far towards the position of the physician. In the country, even more than in London, apothecaries are not now expected to dispense the casual prescription of a physician; they dispense the medicines which they themselves prescribe, and generally those which physicians in consultation with them prescribe; but the majority would refuse (and justly, though not legally,) to dispense an unknown physician's prescription, or to take money

for medicines ordered for cases of disease that they have never seen. Yet it cannot be doubted that it is absolutely essential for the public safety that a qualified body of general dispensers of medicine should exist; and it is now essential, since the apothecaries licensed in part for this purpose have refused to perform it—since they have almost universally given up pharmacy and the preparation of medicines—in a word, since apothecaries have become almost physicians, it is essential that chemists should, in the same measure, become almost apothecaries. They must occupy that part of the field of medicine from which apothecaries have removed; and the better they occupy it, the more thoroughly and exclusively they cultivate it with all the aids of modern science, the more will the great body of the medical profession and the whole public be benefited.

The establishment of the Pharmaceutical Society seems to us well adapted for the furtherance of this advantage. Its objects are the union of the chemists and druggists of Great Britain into one ostensible recognized and independent body, the protection of their general interests, and the advancement of the art and science of pharmacy. If we may suggest a few considerations to the Society, the very first of them would be that they should not so prominently put forward their words of *union, protection, independent privileges, unjust restrictions*, and so on. All these really mean very little, and they sound like the language of a mere trade-union: no restrictions would ever be thought of, if the whole body of chemists were such as the Society would make them; nor will restriction, in any bad sense of the term, be hinted at, if the public find the whole class ready to submit to self-control. We should advise the Society therefore to

pay little regard for the present to their political rights; whatever they may be, they will only suffer by being set higher than the average character of the class can maintain; and they will be advanced and supported only by raising the honourable and scientific reputation of the whole or a great part of the body.

The better objects of the Society are educational. They consider, and very justly, that no one should become an apprentice in their business who has not had the advantage of an adequate fundamental education; and that no person should dispense medicines who has not undergone an examination as a test of his competence to perform that important office. Here, again, a most sound discretion will need to be exercised. The principle must be carefully kept in view, that fitness for an occupation, and eminence in any calling, depend on a man's knowledge in *that* calling, and that only. To leave our own profession, (in judgment on whose members we might not sit impartially), and looking at the legal, we ask, who is the most esteemed lawyer, and who of all is the one that most exalts the character of his profession? Certainly not he, of whom it has been said, though perhaps too severely, that if he knew a little law he would know a little of every thing. Nor would his case be better if he knew more than a little of every thing, and yet not more than many others know of law. For a just eminence in any profession, it is essential that a man be pre-eminent before his brethren: it matters not what his knowledge is of other things, this he must be: the rest are secondary, though doubtless the more he knows of them the better. And what is true of one of a class is true of the whole in cases in which their general reputation is concerned: if pharmaceutical chemists are anxious to hold the highest

position that can possibly be assigned to them, let them stick to pharmacy, and become pre-eminent in that: let all their education have a direct tendency thither; and let not their society split on that rock which has seen the shipwreck of so many fair designs—the attempt to do too much.

Both in the scheme of preliminary education, and in that of the teaching which is to be exclusively professional, there will be danger of falling into error. For the latter, one guide will be sufficient: it should be exclusively directed to the knowledge of the compounding, manufacturing, and preparation of drugs; unless it should be determined that all or a certain number of chemists should be licensed to a limited practice of medicine and surgery. And for the former, let this rule (which it would have been well if it could have been established in our own profession) be observed—that no preliminary education should be required beyond that which persons may be expected to have received whose pecuniary means are such that in after life they may be sufficiently respectable, and yet not too exalted, to pursue the ordinary business of a chemist and druggist. At the best, the preliminary education to be required of a man who desires to enter a profession is but a test of the property, and therein most probably of the respectability and social position, of himself and his connections: of his actual knowledge or fitness for the study of any profession, it is only a very feeble security; and therefore, in all cases of this kind, the object of requiring a certain preliminary education should be, first, to exclude those whose position in society is not good enough to ensure that they will not disgrace the profession they enter; and secondly, not to offer an encouragement to those for whom the profession is not likely to offer a remuneration sufficient to main-

tain them in the rank in which they have been educated.

Within these due limits the designs of the Pharmaceutical Society seem at present to have been confined; and we sincerely hope that the prosperity which we trust they may meet with will not make them in these things unwisely confident. The present general objects are said to be, to establish a school of pharmacy, to institute a regular course of study for the members of their Society, to found lectures on pharmacy and the subjects connected with it (of which the fewer the better), and to assemble periodically for the purpose of scientific discussion, and reading papers on such subjects as relate to their daily avocations and researches. All this is sound and judicious: such measures, if fairly carried out, cannot fail to exalt both the social and the scientific character of the pharmacutists; and so long as they thus strictly confine themselves to their proper province, they may rely on it that all the members of the medical profession will feel it not less their duty and pleasure, than it is their interest, to give them their cordial support.

But it is not difficult to foresee that a Society of this kind, however laudable its objects, will be limited in its capabilities of doing good. Possessing neither the advantages of a charter, nor the exclusive privilege of granting licenses, it cannot be expected to exert a general, much less a universal influence; and it will be open to opposition from other sections of members of the same calling. In its present state the Pharmaceutical Society is more analogous to the Medical and Chirurgical Society, than to the College of Surgeons, and though, like the former, it may grant honour to its associates, it cannot grant even so much of privilege to them as the latter

can. It is to be hoped that its present is only an embryonic state of the Society, and that the designs of its founders may, ere long, be fully carried out. There can be no doubt that "the support of government in completing the success of the measures," which, it is said, may "reasonably be anticipated," should be one of the main and first things sought for. Nothing would be a better security against the dreaded evils of a reform forced upon them by others, than for the whole body of Pharmaceutists, through their Society as a representative organ, to seek for leave and power to reform themselves, and to offer to submit to laws which they can prove to be desirable for the public as well as for themselves. If they need examples of the benefits that would accrue from such a plan, they are to be found in the recent progress of self-reform in our medical institutions; the result of which has been a complete disarming of many whose hands could not, without much danger, have been admitted to the work of reforming others.

The first number of the Pharmaceutical Transactions is a good earnest for the future proceedings of the Society. Its publication has been undertaken only as an experiment, for the purpose of illustrating the advantage of scientific discussions, and in the hope that similar meetings to those at which the papers now printed were read, will shortly be appointed by the Pharmaceutical Society. Its contents are, papers on the Constitution of the Society, by Jacob Bell; on the Rise and Progress of Pharmacy, by Mr. Morson; on Hippuric Acid and its Tests, by Dr. Alexander Ure; and on the Preparation of Extracts, by Mr. Redwood. They are all interesting. From Mr. Morson's paper, our recent correspondent, Dr. Ayres, will be glad to learn that "The most ancient che-

sated by pre-eminence in one or two other subjects; and thus several of those who have been rejected this year are gentlemen of undoubted talent, who have highly distinguished themselves at the colleges or schools where they received their medical education, and were found, at the University examination, to be excellently qualified in one, two, or even three departments, but failed on the fourth. The candidates appear to have been unaware that the Examiners are bound to require a competent knowledge in each department; and that an exclusive acquaintance, however pre-eminent, with one or two subjects, will not avail them in the hour of trial. The favourite study appears to have been anatomy. To chemistry, and more especially to *materia medica* and botany, considerably less attention seems to have been paid.

ROYAL MEDICAL & CHIRURGICAL SOCIETY.

Tuesday, July 6, 1841.

DR. CLENDINNING IN THE CHAIR.

Two Cases of Dislocation of the Tendon of the Long Head of the Biceps Humeri from its Groove. By JOHN SODEN, Jun. Esq. (Communicated by R. Part-ridge, Esq.)

THE first case is that of a man of advanced years, who injured his right shoulder by falling upon his elbow; in six months afterwards he sustained a second accident, a compound fracture of the skull, of which he died; and an opportunity was thereby afforded for examining the nature of the first injury. The symptoms of the injury of the shoulder were always obscure, on account of an alteration in the relative positions of the bones of the joint, which did not apparently depend on a fracture, and could not be considered to amount to a partial dislocation, to which, however, it appeared to be more closely allied than to any other known injury.

The joint was flattened at the posterior and outer parts, and the head of the humerus was unduly prominent in front, and closely drawn up in contact with the under surface of the acromion, grating against it on motion, and becoming locked with it by the upper edge of the greater tubercle striking against that of the acromion on abduction of the arm.

The underhand motions were not much interfered with, except that the patient had no power to raise any object from the

ground, on account of the severe pain induced by exercise of the biceps muscle. On examining the joint the accident was found to be a dislocation of the tendon of the biceps from its groove, unaccompanied by any other injury. The joint exhibited extensive traces of general inflammation, and the capsule was thickened and contracted.

The author infers that the altered position of the bones was dependent on the displacement of the tendon, and he explains its influence in the following manner:—

The head of the humerus being placed on an almost flat surface, and not inclosed in a bony cavity, is subject to the control of the capsular muscles, which invest it on three sides. These muscles may be said to arise from the upper three-fourths of the circumference of a circle, to the centre of which, represented by the head of the humerus, they converge.

To enable the bone to maintain its equilibrium, it is necessary that the capsular muscles should exactly counterbalance each other; and as there is no muscle from the ribs to the humerus to antagonize the upper capsular muscles, it is suggested that this office is performed by the singular course of the long tendon of the biceps, which, by passing over the head of the bone, when the muscle is put in action, tends to throw the head downwards and backwards; it follows, therefore, that the tendon being removed, the head of the bone would rise upwards and forwards.

Allusion is then made to the frequency with which injury of the tendon is involved, in accidents to the shoulder-joint. A paper by Mr. Gregory Smith, in the 14th vol. of the *MEDICAL GAZETTE*, on the "Pathological Appearances in Seven Cases of Injury of the Shoulder," is quoted to shew, that in all those instances which were accidentally met with in the dissecting room, and are consequently without histories attached, the tendon was either ruptured or displaced; and the same altered position of the bones, as in the present case, was noticed in some of them.

The subject of partial dislocation of the humerus is next considered with reference to the probability of an injury of this tendon being involved in the production of that accident. Only three dissections of partial dislocations are on record; they are to be found in a paper, by Mr. Hargrave, in the *Edinburgh Medical and Surgical Journal*. One fell under the observation of Mr. Hargrave himself, and the others he quotes from Sir Astley Cooper's large work, and from Dupuytren's *Leçons Orales*. In Mr. Hargrave's case, the tendon was ruptured; in Sir Astley Cooper's it had been, but had subsequently become reunited; and in Dupuytren's its condition is not mentioned.

The second case is one of a man, who, among other injuries, sustained a dislocation forwards of the humerus. Great difficulty was experienced in the reduction; and after death, for the man only lived a few days, the joint was examined: it was found that the tendon was dislocated, and that it had passed completely over the head of the bone on its inner side, and was lying at the back of the joint. The author attributes the difficulty of reduction to this complication, with the displacement of the bone.

On absorption and regeneration of the neck of the thigh-bone after fracture within the capsular ligament. By W. W. BEEVER, Esq. of Manchester. (Communicated by Mr. Partridge.)

The patient, a woman aged seventy-three, lived nearly four years after the accident. On examination, no vestige of the neck remained, except a triangular portion of the under surface, three-fourths of an inch in length, which, from the obliquity of the fracture, had not been detached from the head. This was articulated by a distinct capsule to a second fragment jutting out from the shaft, and firmly united to it immediately anterior to the lesser trochanter. This adventitious joint, and a band of ligamentous structure extending from the posterior edge of the head to the capsular ligament, formed the only connection between the head and body of the femur. From the large quantity of callus thrown out by the trochanter and head of the bone, the author infers the possibility of bony union being effected.

Case of fatal peritonitis caused by effusion of bile into the peritoneal cavity through an ulcerated opening in the gall-bladder.
By WILLIAM BELL, M.D.

This case is viewed by the author as one of rare occurrence; the inflammation, which precedes ulceration of the gall-bladder, tending to adhesion of the adjacent viscera with its coats. The facts disclosed by dissection were as follows. Effusion of a cinnamon-coloured fluid among the small intestines; convolutions of the bowels glued together by coagulable lymph. The peritoneum investing the convolutions of the small intestines presenting on their anterior aspect a well-defined bright red stripe. Marks of recent inflammation of caput coli, and of the peritoneum lining the abdominal parietes. Liver not morbid; gall-bladder containing two concretions, and perforated on the side next the stomach by an ulcer of sufficient size to admit a crow-quill.

The duration of the attack of peritonitis was sixty-three hours. Its symptoms were not unusual or extraordinary. They were treated first by remedies intended to subdue

presumed irritation; these quieted irritability of stomach, but did not relieve the pain. Then leeches, fomentation, and the other remedies indicated by presumed inflammation, were used. Little relief seems to have been attained, until an opiate enema was thrown up. All pain then ceased, and the patient continued comfortable, and apparently in a favourable state for some hours. The pulse now became sharp, contracted, quick, and feeble, without, however, any return of pain; the skin became hot and dry; and the patient gradually sunk till she expired.

[The preceding papers were accidentally omitted in a former number.—ED. GAZ.]

MEDICAL REFORM.

CENTRAL MEDICAL REFORM ASSOCIATION.

To the Editor of the Medical Gazette.

SIR,

AT a meeting held this day in the Theatre of the Charlotte Street School of Medicine, according to public announcement, after the enrolment of several members, the address to the profession, proposed at the last meeting, was submitted for approval, and unanimously adopted, upon which it was

Resolved—That a copy of the address be forwarded for insertion to the editors of the MEDICAL GAZETTE, Lancet, Medical and Surgical Journal, and Medical Times; also to the Morning Advertiser, Chronicle, Post, Times, and Courier newspapers.

WM. W. BROWN, M.D., Hon. Sec.

Professional Brethren,—

The present crisis is most opportune for the advancement of the question of medical reform; the equally balanced state of parties affords a great intellectual and influential body, like the medical profession, a favourable opportunity to press this subject upon the attention of the legislature. Men of ability and of great weight on both sides of the House of Commons have declared their intention to support the principles of self-government in the members and licentiates of the different corporations, which, if once conceded, will place the management of their affairs and of their funds in the hands of those to whom it properly, legitimately, belongs, will give due control over their officers and administrators, and invest the members and licentiates with a voice in the enactment of the laws which regulate the profession, and ultimately lead to the establishment of a practical uniform system of examination, which the preservation of public life and health demands, and by which the respecta-

bility, utility, and dignity, of the profession would be incalculably promoted. By union, organization, and active co-operation, success is certain ; these just, simple, and constitutional demands attainable. You are aware that the public power of all associated bodies is a hundred-fold increased above that of individuals, however numerous, however ardent in their isolated and single capacities. We have had unions, but they failed in fulfilling their mission. They never won the confidence or secured the accession of great numbers of the profession. They took wrong ground. They embodied in them, from their commencement, the elements of their own dissolution. They sought too much : they got nothing. Their objects were preposterous and extravagant. They supposed, contrary to the natural progress of events, that they could step from positive wrong to positive right, and not listening to the precepts of experience, which prefer the gradual alteration of our institutions, so as to suit the wishes and exigencies of the profession, they endeavoured to annihilate existing authorities, and to establish in their place a theory that has never been tried in this country, and which would not guarantee us against the abuses of which we seek redress.

Intrigue, jealousy, partisanship, marred their exertions. The elections to their own reforming councils were partial—were farces. The appropriation of the money of the members frequently frivolous and often unjustifiable. Their policy, as unwise as their proceedings were inefficient, explains the reasons why so many have seceded, and that they never enrolled in their ranks more than one hundred and fifty members.

They were guilty of the faults which they themselves denounced in others. They remind us of the words of Medea, in the Greek play : “ I know and approve what is right, and at the same time I do what is wrong.” The cause must not suffer for the weakness and deficiencies of a few of its advocates.

We purpose to avoid those errors. No money will be demanded more than is required to meet contingencies as they arise. Every legally qualified practitioner is invited to join us. The common cause is sufficient introduction.

In our numbers our strength will consist, which will furnish the best evidence of the grievances under which we labour. Our power will owe much of its energy to our hopes and perseverance. “ Possunt quia posse videntur.”

Five hundred members in the metropolis, with corresponding branch associations in large towns, bringing their influence to bear upon their representatives, will not only extort the right of representation from the

medical corporations, but enforce still further reforms upon those bodies which have so long sacrificed the public to their private interests.

The legislature will not deny our inherent right to regulate our own concerns. It will no longer suffer a profession that dignifies our moral and intellectual nature, that extends its benefits to every member of the community, to be unpossessed of that protection which it has extended to every subject in the realm, and which is essentially necessary for the government of a body of men to whose mutual agreement the lives and safety of society are committed.

The difficulties to be overcome are daily diminishing. The objects to be sought are worthy of even toilsome attainment. The corporations, alarmed at the discontent which so generally prevails, have proffered certain concessions, which the profession have justly rejected with indignation. We have only to become united to be confederated by a spirit of determination, which only results from a sympathy of suffering and a participation in wrongs. Our efforts have only to be more general, more simultaneous, to be successful.

The six thousand signatures petitioning Parliament for a reform of our profession prove that we have rescued ourselves from the charge of being apathetic, under the complication of degradation and injustice which we endure. Remember, we seek not to mislead : we look only for the simple principle of self-government, which justice, equity, custom, the constitution, award to us. We are opposed to the crude and impracticable speculations, which, like bubbles on the waters, burst at the moment of their birth, that extreme section have propounded, and which have only served to divide and misdirect the energies of the profession. We war not with existing institutions. We are opposed to the principle of self-election, of self-perpetuation of their irresponsible power. We struggle to remove these evils, without making them run the gauntlet of hazardous experiment. We want to guard ourselves against the bias of self-interest on one hand, and the extreme indignation and desire of destruction, the consequences of long-continued neglect, and unjust exclusion, on the other. We wish to steer between the two extremes.

Professional Brethren—it is a duty which you owe to that profession, whose interest and respectability you are sworn to uphold, to join and assist in the work of disenthralment.

July 12, 1841.

GLANDERS IN THE HUMAN SUBJECT.

DR. HUTTON said that as four or five cases of glanders in the human subject had, within a comparatively short period, come under his own notice, or that of the surgeons of the House of Industry, he was anxious to lay them briefly before the Society, and also to exhibit a specimen of the disease as it had manifested itself in the lungs of a patient who died about two days before. Previous, however, to entering on this case he would read the details of another, in which some experiments were made with the view of testing the character of the poison, and ascertaining whether it was glanders or not. One of the results of these was, that an ass, inoculated with matter taken from the patient, was in due course attacked with the disease. The case was recorded by Mr. Rutherford, one of the resident pupils of the Hospital, for whose accuracy Dr. Hutton could vouch. The subject, a young man named P. Kelly, aged about twenty, was admitted into the Richmond Hospital on the 26th of August, 1838. On admission, his face presented that peculiar aspect which is so characteristic of glanders; the left half was very much swollen, tense and shining, the redness fading away gradually and becoming lost in the surrounding integuments. Both eyes, but particularly the left, were closed, from inflammation and œdema of the lids. The left ear was swollen, of a dark red or livid colour, and the patient was quite deaf on that side. The glands of the left side of the jaw and face were enlarged and indurated, and he complained of a feeling of numbness in the whole of that side of the head and face. About an inch and a half in front of the ear there was a large flaccid vesicle; there were also two pustules on the face, one of which had burst and was sloughing. On various parts of the body there were numerous pustules in different states, from the first to the more advanced stages. In the first stage, the skin in the situation where the vesicle afterwards occurred was of a peculiar pale, whitish appearance; in the next stage the vesicle appeared, not, however, exactly in the centre of the pale spot, but rather to one side. In a more advanced stage it became sero-purulent, then pustular, and some time afterwards the pustules began to shrink and become depressed in the centre. The mucous membrane of the mouth was inflamed and covered with a viscid adhesive mucus; the schneiderian membrane was also inflamed, but there was no discharge of purulent matter from it. The patient had the ordinary symptoms of irritative fever; his head was confused, but he had no pain or raving; his bowels rather free; his urine high coloured.

He stated that he had been always healthy, and when questioned as to the nature of his occupation, said that he had been employed for the last four months in attending horses which were labouring under glanders; that he had been retained specially for that purpose, and groomed the animals once a day. He did not recollect that he had had a wound or sore on either hand; he had not drank out of any vessel used by the horses, nor had he slept in the stable. He attributed his illness to fatigue after a long journey, and said that the first symptoms he had noticed were pains in his knees, followed by headache. Four days afterwards the left side of the face and head began to swell, with increase of fever and depression of strength. On the 27th, the day after admission, his symptoms were progressing; the tumefaction of the head and face increased, and several livid vesicles made their appearance, accompanied by severe pain in both jaws. Several vesicles now began to show themselves on the anterior part of the arms and chest; his pulse became smaller, and rose to 120; his respiration was somewhat suspicious; his breath foetid; and he felt pain when the ends of the long bones were pressed on in the vicinity of the joints. His head was still confused, but he had no raving. Towards eight o'clock in the afternoon there was a further exacerbation of his symptoms. He made water tolerably well, but did not seem to be aware of passing it. He was ordered to take ten grains of sulphate of quinine three times a day. On the 28th the eruption was still extending; his pulse 140, and weak; his thirst excessive, and he raved frequently. At half-past 3, P.M., he was restless and tossing about in bed, with constant involuntary motions of the lower extremities, quick small pulse, and hurried respiration. Twelve new spots had now made their appearance; his fever and delirium were increased, and he was passing both urine and fæces involuntarily. There was a discharge of sanious fluid from the left ear, but none from the nostril. On the 29th a further exacerbation of symptoms took place. The left elbow joint was swollen and painful; the pustules increased in number and size, and were intermixed with gangrenous bullæ; and along the internal and anterior parts of the thighs, in the situation of the absorbents, pale, rose-coloured swellings began to appear. His breath was foetid, and the odour from his whole body almost insupportable. He had no discharge from the nostrils, but, on examining the nose, Dr. Hutton observed a small ulcer on the left side of the septum narium. The man died during the course of the night.

On examination a great number of small circumscribed abscesses or purulent depots were found in the extremities; as many as

thirty were found in the left arm. There were two or three of the same kind in the pectoralis major, and several of the same description in the recti of both thighs, all circumscribed and imbedded in the muscular tissue. In the left lung there was a small depot of pus, surrounded by a dark, livid border; another of the same kind was discovered in the right lung. On the posterior surface of the heart there were dark-coloured spots, and the blood was remarkably fluid in all the vessels. There was a deposition of pus under the mucous membrane of the larynx, and also on the posterior surface of the epiglottis; the left half of the face was in a semi-gangrenous state.

Shortly before this, another case of the same kind had occurred in the female ward. An ass which had been procured for the purpose, was inoculated with purulent matter taken on the fourth day from the patient. No disease was produced. On the 27th of August, the day after Kelly's admission, the experiment was again repeated on another with matter taken from the vesicles and pustules on his body. The lymph was inserted into the left nostril of the animal, the pus into the opposite one; it was also inserted into the ear. On the following evening the ass appeared unwell, and next day had enlargement of one of the glands of the jaw on the left side, with increased heat and tenderness, accompanied by feverish symptoms. The left ala nasi swelled, and the line of absorbents from this to the glands on the side of the jaw could be distinctly traced. Next day there was a profuse watery discharge from both nostrils, particularly the left; and on the following day, the fifth after inoculation, the discharge was purulent. Soon afterwards the animal was killed with nuxvomica, it having been previously ascertained by Mr. Ferguson, V.S., that the animal was really glandered.

On examination, a cluster of pustules having a tubercular aspect were found in the left nostril; in the right there were circular patches of ulceration. Similar ulcers were found in the interior of the stomach, and there was a cluster of pustules in the anterior lobe of one lung. There was no morbid appearance in the larynx or trachea. Dr. Hutton exhibited several drawings to shew the condition of the various parts, particularly the nostrils, stomach, and lungs. The next case, to which he would merely allude, as it was about to be published by Dr. M'Donnell, who had charge of it, had occurred a short time ago at the Richmond Hospital. The patient was admitted for an accident of which he recovered, but before he left the hospital he was seized with an affection of the joints, followed by an eruption of pustules along the side of the nose, which were recognised as being connected with glanders. Mr. Smith procured some of the matter, and

inoculated an ass, which in the course of four or five days became sick, and was subsequently attacked with glanders. The same phenomena as observed in the latter case of inoculation were present; the cartilages of the joints were also found to be ulcerated. Dr. Hutton exhibited some drawings to shew the condition of the parts. He also exhibited a drawing of a case which had occurred some years back at the Richmond Hospital under the care of the late Dr. M'Dowell, before the disease was sufficiently known. The drawing had been made by Mr. Conolly, and Dr. Hutton observed that he had represented the features of the disease with great accuracy, and had depicted most faithfully the white areola which encircles the vesicles. Since that period the areola has been invariably found to be present in every case, and is regarded as one of the pathognomic features of the disease. It has been noticed independently by Dr. Hutton, Mr. Adams, and other observers, and forms one of the marks by which the disease is distinguished from phlebitis. Dr. Hutton said that he should next proceed to read the notes of a case which had recently come under his observation. The patient, J. Butler, a boy about five years of age, was admitted into the Richmond Hospital on the 13th of Dec., 1840. It was stated that he had been always a fine healthy child up to the period of his illness. He complained at first of sickness and pain in his bowels, and on the following day had pains in his knees. About three days afterwards the left side of the face and eyelid became swollen, and the usual symptoms of irritative fever set in, accompanied with thirst, restlessness, quick pulse, and scanty urine. On the 5th of December the fever was increased, and the other side of the face was involved in the swelling; on the 7th a number of pimples with white tops appeared on the inflamed surface. On the 13th, the date of his admission, his face was greatly swelled and inflamed, and presented a number of pustules mixed with several ash-coloured ulcers: he had also an eruption of pustules over his body. Some of these were flattened and somewhat vesicular, like chicken-pock, some were conical and pustular, some in a state of incrustation. Around some of them, particularly those which were in the earlier stage, the peculiar white areola was still visible. Several of the joints were swelled and painful, and there was evident effusion into the left elbow-joint. The child was extremely feverish and irritable, tossing about in the bed, and raving: the smell from his body was extremely offensive. He continued in this way, with little change in his symptoms, until the 16th, when he expired. All that could be learned of his history was, that the father was a labourer, and kept a horse, which was said to be labouring under a discharge from his nostrils, the result of

cold, but Dr. Hutton said he had not as yet examined the animal. On examination after death there was an effusion of pus discovered in the left knee-joint. In the thorax there was a small collection of pus close to the edge of the left lung. The lung was of a deep red colour, and presented several ecchymosed spots on its surface, and contained two small abscesses. The right lung presented a few flattened tubercles. These were pointed out to the attention of the meeting by Dr. Hutton.—*Dublin Journal of Medical Sciences.*

LOCAL BATHS.

M. MAYOR, of Lausanne, has lately presented to the Royal Academy of Sciences in Paris an apparatus for bathing any limb, or any part of one, separately, and in any possible position. This apparatus consists of a hollow cylinder of copper, the two extremities of which are provided with caoutchouc discs, each of which has at its centre a large opening through which the limb is introduced. Varied according to the parts to which they are to be applied, these apparatus form a kind of sleeves, or gloves, or boots, according as they have to enclose the arm, the hand, or the foot and leg.

This kind of portable bath has, M. Mayor states, numerous advantages. It is cheap. It permits the limbs to be bathed without obliging the patients to keep in an irksome position. When, for instance, the elbow-joint is to be bathed, two pieces corresponding to the arm and the forearm may be articulated together, [by caoutchouc tube, we suppose], so as to be capable of movement on each other, and the patient suspending the apparatus in a sling may walk about, and even make use of the movement of flexion and extension of his arm. Only a small quantity of water is needed, and this advantage, though unimportant in the case of simple baths, is considerable when medicated baths are ordered. The liquid will preserve its temperature a long time, and this, added to the convenience of employing it, will enable the bath to be used longer than is customary. The fluid may be removed or added to by two small openings made above and below through the wall of the cylinder. The apparatus may also be used for constant irrigation by making one of these openings communicate with a tube conveying water to the cylinder, and the other one to carry it off.—*Gazette Médicale, Mai 26, 1841.*

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, July 22, 1841.

Constantine Wright, Sussex.—William Boxall, Petworth, Sussex.—Richard Gilbertson, Cardiganshire.—Thomas Barker Smart, Yorkshire.—Robert Couchman, Temple Balsall, Warwickshire.—Henry Runcorn, Manchester.—John Johnstone, Duckinfield.

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS. Friday, July 23.

Stephen Spranger.—William Henry Sandham.—John Wyatt Barnard.—Edward Octavius Hocken.—Winter Moody.—Arthur Newell Jones.—John Myers Cockcroft.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 17th July, 1841.

Small Pox	14
Measles	17
Scarlatina	8
Hooping Cough	23
Croup	8
Thrush	5
Diarrhœa	9
Dysentery	1
Cholera	0
Influenza.....	1
Typhus	19
Erysipelas	4
Syphilis	1
Hydrophobia.....	0
Diseases of the Brain, Nerves, and Senses ..	168
Diseases of the Lungs, and other Organs of Respiration.....	234
Diseases of the Heart and Blood-vessels	13
Diseases of the Stomach, Liver, and other Organs of Digestion	47
Diseases of the Kidneys, &c.....	2
Childbed	4
Ovarian Dropsy	1
Diseases of Uterus, &c.	1
Rheumatism	3
Diseases of Joints, &c.	0
Ulcer	0
Fistula	0
Diseases of Skin, &c	1
Diseases of Uncertain Seat	96
Old Age or Natural Decay.....	42
Deaths by Violence, Privation, or Intemperance	24
Causes not specified	15

Deaths from all Causes 761

METEOROLOGICAL JOURNAL.

July.		THERMOMETER.	BAROMETER.
Wednesday	21	from 55 to 67	29.44 to 29.55
Thursday	22	53 61	29.62 29.79
Friday	23	51 63	29.85 29.95
Saturday	24	52 63	30.02 30.08
Sunday	25	52 67	30.06 30.03
Monday	26	52 69	30.00 29.98
Tuesday	27	55 69	29.95 29.91

Winds, very variable, West prevailing. On the 21st, clear from 2 till 3, P.M. otherwise a general overcast, with frequent rain. The 22d, evening clear, otherwise cloudy; lightning and thunder, accompanied with heavy rain, from about 11 till half-past 11, A.M. The 23d, generally cloudy; a little rain fell in the evening. The 24th, cloudy. The 25th, generally clear. The 26th, morning cloudy, otherwise clear. The 27th, generally cloudy; a shower of rain about 20 minutes past 12, P.M. Rain fallen, .38 of an inch.

NOTICE.—We regret that we cannot insert Dr. Burne's note, except in the *extra limites* department. Both parties have been heard twice, and we cannot continue the discussion except as above stated.

WILSON & OGILVY, 57, Skinner Street, London

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, AUGUST 6, 1841.

LECTURES

ON THE

PRINCIPLES AND PRACTICE OF PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XLIV.

Cynanche Parotidæa. Spontaneous Salivation. Aphthæ. Cynanche Tonsillaris.

THE Greek writers on medicine applied the terms *συναγχη* and *κυναγχη* to inflammatory affections occurring about the *throat*, and more or less interfering with the functions of respiration and deglutition; and the Latins employed the word *angina* in nearly the same sense. And Cullen, in his *Nosology*, has made a *genus* of *Cynanche*, although the diseases which he has included in that genus have but little connexion, except in so far as that the parts they occupy lie *near* to each other. Some of them indeed have their seat in different, though almost contiguous, portions of the *same membrane*; and are apt, sometimes, on that account, to pass one into the other. In general they are allied rather by proximity of situation, than by community of symptoms.

I mention these things, because there being a great disposition in the present day to *re-name* diseases, and to affect a more precise and scientific nomenclature than sufficed for Cullen, if I adopted the more modern appellations without adverting to the old ones, which have been current so many years, you might experience some difficulty in your reading, in determining what disease was intended, when it was merely *named*. For my own part, I think there is much inconvenience in altering the established nomenclature; and especially in changing such arbitrary terms as, though they may not be *scientific*, are yet *definite*, and con-

vey no erroneous notions respecting the nature of the disorder. I shall give you, however, in most cases, both the old and the new denominations.

Cynanche parotidæa.—Now one of the disorders in Dr. Cullen's genus *Cynanche*—though not the first in the order he follows—is *cynanche parotidæa*. It is an inflammatory affection of the salivary glands, and of the parotid gland in particular. Accordingly it is called *parotitis* now-a-days. It is not, however, mere inflammation of the parotid, arising from any cause whatever; and therefore *parotitis*, unless some epithet be added, is less exact than *cynanche parotidæa*. The vulgar have given it just as good an arbitrary name as the learned; and they call it, in this country, the *mumps*; and the Scotch call it, I believe, the *branks*.

This disorder need not detain us long. The parotid swells; tumefaction takes place beneath the ear; and if the submaxillary and sublingual glands are not implicated in the outset, they soon participate in the tumefaction, in most instances; so that the swelling extends from beneath the ear along the neck, towards the chin, and the swelled parts are hot and painful, and very tender on pressure. The aspect of the patient becomes curiously deformed. Sometimes one side only is affected; sometimes both sides at once; but most commonly of all, first one side and then the other. These local symptoms are attended with slight fever. But the only function that is materially affected is the motion of the lower jaw, which is impeded by the swelling. The inflammatory condition almost always terminates after a few days, in resolution, under the use of the antiphlogistic regimen, and the application of external warmth. The disease reaches its height in about four days, and then begins to decline; and its whole duration may be stated, on an average, at eight or ten days.

This complaint often prevails epidemically:

when it affects one person in a family, or school, it usually affects several others, simultaneously or in succession. It chiefly attacks children and young persons. There can be no doubt that it spreads by contagion; and it seldom happens that the same person is twice affected by the mumps. These are remarkable circumstances, and give the malady a peculiar and specific character. I do not dwell upon them now, because they belong also to a very interesting *group* of diseases, which will require to be particularly considered hereafter.

Another curious circumstance connected with the disease, and one which has some bearing upon its treatment, is that, in many cases, upon the subsidence of the swelling of the neck and throat, and particularly when it subsides *quickly*, the *testicles*, in the male sex, become swollen and tender, and the *mammæ* in the female. It is said, but I do not know whether the observation be constantly true, that the testicle, or the breast, of *the same side* with the inflamed parotid, suffers. Sometimes the testicle wastes away after the swelling recedes; a circumstance which is known occasionally to happen when inflammation of that part arises from other causes. This, however, is not usual. In general the inflammation subsides and ceases in the one gland as it does in the other; the swelling is neither very painful nor long continued. But sometimes a more serious transference takes place, from the testicle to the brain: this I have never witnessed; but then, to say the truth, I have not often been called upon to treat the mumps, and my personal experience of it is limited. I find it stated that the metastasis to the testicle is considered as rather a fortunate circumstance, because it serves as a sort of protection against metastasis to the brain; but I suspect this to be a mistake. Inflammation of the brain, or of its membranes, has sometimes occurred on the disappearance of the *parotid* swelling; but it has much oftener supervened, I believe, upon the retrocession of the inflammation in the *testicle* or *mamma*. It is said also that the inflammation sometimes returns from the testicle to the parotid, and back again; oscillating thus two or three times between the two glands. Fortunately, the metastasis to the brain is much more rare than that to the testicle.

The treatment of the mumps is simple. It consists in the observance of the antiphlogistic regimen; mild diaphoretics; laxative medicines if the head aches, or the bowels are confined; and warm fomentations, or dry warm flannel, to the neck and throat. The tendency observed in this complaint to a change of place—to metastasis to more important organs—forbids us from using very active measures to *check* or *subdue* the

inflammation. Nor are such measures necessary. We are not to bleed, nor violently to purge such patients, nor to apply cold to reduce the swelling. Luckily, hot applications are not only the most safe and proper, but the most grateful also to the feelings of the patient. If suppuration should ensue—which is unusual and unlikely, but which sometimes does occur from extension of the inflammation to the neighbouring cellular tissue—poultices must be substituted for the fomentation. Warm applications, and rest in the horizontal posture, are to be recommended when the inflammation leaves the salivary glands, and attacks the testicles; or if the patient will not, or cannot, lie up, the testicle must be supported by a suspensory bandage—a bag truss. If the inflammation of the testicle or mamma be very violent, we must apply leeches, and afterwards poultices; but this will not often be required, or advisable. Finally, if the inflammation should fly to the brain, we must lay aside our previous caution, and treat the disease in that active manner which the inflammation of so important a part of the body demands. No *worse* metastasis *can* occur on the cessation of the phrenitis. I have fully spoken heretofore of the treatment to be pursued in that disease, and I have nothing to add respecting it now, except that it may be right, as an auxiliary expedient, to try to reproduce the inflammation in the parotid, or testicle, or mamma, by irritating applications—mustard poultices, for example—in the hope of thus producing what is called *revulsion*, and of diverting the disease from the brain to the part which it previously occupied.

Mercurial Parotitis.—You know that there is another specific form of *parotitis*, which is apt to be induced by mercury. Of this I have already spoken. When it is severe, it may be treated by leeches, without any dread of such metastasis as occurs in the mumps. It is usually, though not always, accompanied by a profuse discharge of the secretion proper to the glands affected; and it is attended also by sponginess and swelling of the gums.

I presume that when inflammation of these salivary glands is *not* attended with ptyalism, the parenchyma of the gland, or the cellular tissue which enters into its composition, is principally affected; and that when there *is* much salivation, the membrane lining the secretory and excretory ducts are implicated. We see the same distinctions in other analogous organs.

Spontaneous salivation.—Profuse ptyalism sometimes occurs without any obvious cause; and is then said to be *idiopathic*: and this is a circumstance which it concerns you to be aware of, both as practitioners, and as medical jurists. The same *tender-*

ness and swelling of the salivary glands, the same copious secretion and excretion of saliva, nay, even the same fœtor, or a smell which can scarcely be distinguished from it—the same collection of symptoms which is familiar to you as indicating the specific action of mercury upon the human system, will arise sometimes (but very rarely) when not a particle of mercury has been administered. Several other substances are well known to have the occasional effect of producing an increased, and even a profuse flow of saliva: preparations, for example, of gold, of copper, of antimony, and of arsenic. The castor oil is said to have sometimes the same consequence. Digitalis certainly has; and the iodide of potassium; and sometimes, I believe, opium. Now and then ptyalism is met with as a symptom, among others, of pregnancy. Occasionally it results from some local irritation within the mouth: from a decayed or misplaced tooth. But what I principally wish to call your attention to is the fact that *salivation* may occur as an *idiopathic complaint*. In the twenty sixth volume of the *London Medical and Physical Journal*, there is an instance of it described by Mr. Davies, in which two or three pints of saliva were discharged daily for sometime. This flux at length ceased under the use of laxative medicines. In the second volume of the *Transactions of the College of Physicians* is an extraordinary example of the same thing, related by Mr. Power. A young lady, of sixteen, spat from sixteen to forty ounces of saliva daily for upwards of two years. Mr. Power believed that the ptyalism in this case was originally excited by wool, which he found, in a foetid state, in her ears. In the *Revue Médicale* there is an account given of a patient who was cured of a spontaneous ptyalism after spitting nine pints daily for nine years and a half. You may see another instance as related by Dr. Prout in the old series of the *Annals of Philosophy*. Dr. Pereira states that he has seen a dozen such cases; and he describes one which was fatal, not from the ptyalism however, but from sloughing of the cheek: and this is no uncommon circumstance. In certain cases of idiopathic inflammation and ulceration of the gums or cheeks, from some constitutional unsoundness, there may be extensive sloughing, ptyalism, and a very offensive odour, much resembling that which mercury produces. I have met with one example only of well-marked spontaneous ptyalism; and some of its circumstances were so peculiar, that they may be worth relating. I was taken out to Bayswater, by a medical friend, in the beginning of the year 1833, to see a little girl, ten years old, who had been in a state of salivation from the 5th of November in the preceding year. Up to that

time she had been a healthy lively child, with nothing very remarkable about her, except that she was habitually subject to profuse perspirations, which had a very acid smell: so that the washerwoman was always aware which were her clothes, when she came to wash them, by this *smell*. She then suddenly became indisposed, had a little headache, and began to spit a good deal. This was noticed by her mother, and pointed out to her medical attendant, before *any* medicine was given her; and mercury, on that account, was religiously withheld. But in spite of all treatment the ptyalism went on increasing. When I saw her she was spitting three pints of saliva in twelve hours; transparent, rather dark-coloured, and with a small quantity of foam on its surface. There was nothing amiss with her teeth, or her gums, and no fœtor of the breath. She was greatly emaciated, and resembled, in some respects, a person worn down by diabetes. From the very commencement of the spitting, the acid perspiration had ceased, and even the vapour bath failed to make her sweat. A great variety of remedies were tried, under Dr. Nevinson's superintendence, but without the least good effect. At last came the visitation of the influenza, in April of that year. The girl became severely affected by that disorder; and thereupon the salivation disappeared, and has not returned. I heard to-day that she is in excellent health.

Should you meet with cases of the same kind, you will search for some cause of irritation in the neighbourhood of the salivary glands, and especially in the state of the teeth and gums; and finding none, you will seek farther for the cause of the salivation in some deviation from the natural condition of one or other of the principal functions of the system: and you will regulate your treatment accordingly. I do not know of any specific plan of cure to be recommended: but it is certainly of importance that you should be acquainted with the fact, that ptyalism sometimes exists as a separate and independent malady. Astringent washes are found, sometimes, of service; a solution of alum, or the infusion of catechu.

Aphthæ.—Before I proceed to the diseases pertaining to the interior of the fauces and throat, let me take this opportunity of saying a few words in respect to *aphthæ*. They form the characteristic symptom of an especial disease of infancy, and they are apt to occur in the course of other diseases in adult age; and they are then of some importance as guides in forming our prognosis, and even in determining our treatment.

Aphthæ consist in small, irregular, but usually roundish white specks, or patches, scattered over the surface of the tongue, and the lining membrane of the cavity of the mouth and

fauces ; the angles of the lips, the cheeks, the palate, the pendulous velum, the tonsils, the pharynx. They look like little drops of tallow, or morsels of curd, sprinkled over those parts ; they project a little above the surrounding surface ; and, in fact, they are mostly formed by elevated portions of the mucous epidermis, covering a small quantity of a serous or gelatinous fluid, which separates the epidermis from the subjacent corium. These portions of the epidermis detach themselves, and fall off ; leaving behind them a reddish raw-looking surface, or sometimes a foul and ash-coloured spot : and successive crops of these aphthæ are apt to be formed.

Now children in arms who exhibit these aphthæ, are said to have the *thrush*. This occurs at an early age ; seldom, or never, I believe, after the period of lactation is over. The spots occasion some inconvenience in themselves—the mouth is rendered hot and tender by them. The child may be eager enough to take the breast, but is observed to do so with pain and wailing whenever the mouth is applied to the nipple, and attempts to suck or to swallow are made.

But these aphthæ, thus occurring in infants, are attended with other symptoms of disorder : such as drowsiness, sickness, diarrhoea, and some feverishness. And, I believe, a general notion prevails, that the same aphthous condition which is visible in the tongue and mouth, pervades, in such cases, the whole of the alimentary canal. But this must be a mistake. That *some* morbid condition exists throughout that tract is highly probable, but true aphthæ can only form on those mucous surfaces which are provided with a continuous *epidermis*. This erroneous notion has been strengthened, perhaps, by the observation of aphthous spots on the pharynx and œsophagus. The complaint sometimes appears to be the result of improper diet, in children brought up by hand ; or of milk of a bad quality, from an unhealthy or intemperate nurse. It generally lasts eight or ten days. It is not attended with much danger, except in certain cases, when the surface is left brown or bluish after the loosening and separation of the crusts. In such cases, the local affection is apt to run into a bad kind of gangrenous ulceration, and the discharges from the bowels become slimy and shreddy.

In almost all instances of the thrush in children, there is acidity of stomach present. Care, of course, is to be taken to discover and to correct any error of diet ; and any unwholesomeness in the quality of the food. And *antacids* are to be administered. I know of no form of medicine better adapted to remedy the diarrhoea of infants, than the *Pulvis Sodæ cum Hydrargyro* of our hospital Pharmacopœia ; composed of two parts

of the *Pulvis Cretæ Compositus*, two parts of the dried Carbonate of Soda, and one part of *Hydrargyrum cum Cretâ*. From three to five grains of this powder may be given thrice daily : and for the local affection of the tongue and mouth, the *mel boracis* is a capital application. It may be painted on the aphthous parts with a camel's hair pencil.

Aphthæ occurring in adults, in the course of other diseases, are often the harbingers of dissolution. They denote considerable *debility* ; and they point out the propriety of sustaining the patient's strength, by bark, wine, and nourishing food. It is remarkable how treatment of this kind will sometimes *tell*. I had a patient last summer who lived for some months, and in tolerable comfort, after a second attack of apoplexy. Every now and then he would have a crop of aphthæ appear, which was always an admonition to us that he not only would bear, but that he required some tonic. A more generous diet, with bark, would dissipate them in a day or two.

Borax is an excellent application for aphthæ, whether they occur in adults or in infants. I have known it afford great comfort to patients who were in the last stage of phthisis, and to whom the aphthous state of the mouth was a considerable source of distress. Equal parts of *Mel Boracis*, and *Syrup of Poppies*, is a good form. Or an agreeable as well as useful gargle may be made, by mixing two drachms of *Borax*, with half an ounce of *Mel Rosæ*, three ounces of *Decoction of Quince Seeds*, and four ounces of water.

Aphthæ seem sometimes to depend upon mere derangement of the stomach. A nobleman who is well known as a *bon vivant*, can never eat shell-fish (so I am told by his physician) without finding, within two hours, that his mouth is full of aphthæ. Even lobster-sauce will serve him thus. I look upon this as a sort of internal urticaria.

Cynanche tonsillaris. — Hard by the salivary glands lie the tonsils : and one of Cullen's species of cynanche is the *cynanche tonsillaris* ; in more modern language, tonsillitis, or amygdalitis ; or, in the vernacular, quinsy, common inflammatory sore-throat : a disease which, though internal, is yet within the reach of our sight, and easily recognized.

The popular term quinsy is in truth traceable—through the French *esquinancie*—to the scientific term cynanche.

This common and troublesome disorder occurs with very unequal severity in different cases. The difference depends upon the extent of the disease, and the number and variety of the parts which it involves : for it is seldom limited entirely to the tonsils, but

spreads to the uvula, the velum palati, the salivary glands, the pharynx, and even to the root of the tongue, and the neighbouring cellular tissue. When the inflammation is superficial it does not produce any great distress, even though it may be diffused. When it penetrates through and beyond the mucous membrane, it is apt to end in suppuration, and to harass the patient much: the tonsils swell to an enormous size, and at length deep abscesses form in them. The disease is worst of all when the back part of the tongue, and the muscular and cellular tissue thereabouts, become implicated: it may chance to reach even the larynx, and then it is always and extremely perilous.

Under its more ordinary forms, cynanche tonsillaris generally manifests itself, at first, by a slight degree of uneasiness and difficulty in swallowing; with a constant dryness and sense of constriction in the fauces, and a feeling as if some foreign substance were sticking there. Upon inspecting the throat, more or less of inflammatory redness and swelling is seen of one or both of the tonsils. Sometimes both of them are affected at once. Very frequently one only is first attacked; and the swelling begins in the other as it ceases in the first. This is just what occurs in many instances also of cynanche parotidæa. The uvula is commonly enlarged and elongated, and of a scarlet colour. Sometimes it drags upon the back part of the tongue, or hangs into the pharynx, causing the disagreeable sensation of a foreign body continually present, and provoking, by its mere contact, painful and fatiguing acts of deglutition. More frequently the uvula may be seen to be adherent to that tonsil which is most swollen. The dryness of the fauces soon gives place to a copious secretion of transparent mucus, which is frothy and viscid, and sticks to the inflamed surface, so as to be detached with difficulty; and the patient is tormented by continual and painful efforts to hawk up, or to swallow, this mucus. In an early stage of the disease opaque whitish spots appear upon the red tonsil. They are exudations from its surface, or the discharged contents of the mucous crypts there situate. It is important that you should be aware of this, that you may not mistake such specks for ulcerating or sloughing points, such as occur in some other affections of the throat, but which are not common, at least in the outset, of this.

When the inflammation is violent, the submaxillary and parotid glands sometimes swell, and become tender on pressure; and, less frequently, the patient is troubled by profuse ptyalism. In other words, the inflammation spreads from the tonsils to the salivary glands, and secondary parotitis occurs; sometimes with and sometimes without an augmentation of their natural

secretion. Unable, or unwilling to swallow the abundant saliva, the patient allows it to dribble from his mouth.

Now and then, although the act of swallowing is difficult and painful, you perceive, on looking into the fauces, no appearance which can account for these symptoms. The inflammation is seated lower down in the throat; out of sight. This cannot with propriety be called cynanche tonsillaris; indeed, it forms a distinct species, the *cynanche pharyngea* of Cullen. I mention it here because it really does not require any separate consideration.

The pain in cynanche tonsillaris is felt almost solely during the act of deglutition; which is difficult also from the mechanical narrowing of the passage by the enlarged glands. When both tonsils are affected at once, and much swollen, they push forwards the anterior pillars of the velum palati, and project, in the shape of two great balls of flesh, into the arch of the fauces, so as to leave a very small space only between them; and they sometimes even meet and touch each other, and cause ulceration by their mutual pressure. When attempts are made to swallow liquids, they are apt to return through the nose; the backward passage can no longer be shut in consequence of the tumid and fixed condition of the velum palati. The patients are unable to swallow even soft solids; indeed the pain of swallowing is so great, that they are not easily persuaded to try. In severe cases pain shoots from the throat to the ear along the course of the eustachian tube; and this is considered important, as being indicative of suppuration. I believe that suppuration does occur in the majority of the cases which are attended with this symptom. Sometimes there is tinnitus aurium, and partial deafness, from the obstruction produced to the passage of air through the eustachian tube; either by closure of its extremity in consequence of the swollen state of the parts about it, or by some thickening of its lining membrane from an extension of the inflammation along its channel.

When the inflammation is intense, and involves the root of the tongue, the patient becomes unable to open his mouth sufficiently to allow the fauces to be seen; and the inflamed parts can be examined only by means of one's finger. In some instances the power of separating the jaws is so limited, as not to admit the introduction of the finger: and the tongue is incapable of any motion.

In ordinary cases, however severe the disease may be, there is scarcely any affection of the breathing. But the throat is so blocked up, and the free play of the velum palati so impeded, that the speech is altered; becoming thick, guttural, and inarticulate.

You may often recognize the disease by the peculiar tone of the patient's voice, without looking into his throat. When the swelling of the tonsils is very great, the breathing does sometimes become impaired; and it is in such cases alone, or *nearly* in such cases alone, that this disease is at all alarming.

Cynanche tonsillaris is commonly attended, from the very outset of the inflammation, by smart inflammatory fever; severe headache often, and a rapid pulse, rising to 120 beats in the minute, or more. The constitutional disturbance runs higher than we might have expected, considering the limited extent of the local inflammation, and the comparatively small importance of the part inflamed. At the same time there is very little of that debility and anxiety which are observed, as we shall see hereafter, in common continued fever.

The inflammation often terminates by resolution; but when it is violent, or long-continued, it very frequently leads to the formation of pus. When the mucous secretion increases in quantity, and becomes less viscid, when the difficulty of swallowing diminishes, and the febrile symptoms decline, resolution may be expected. Suppuration, on the other hand, may be looked for when the inflammation is unusually intense, when, by the swelling of the tonsils, the breathing is impeded, when a pulsating pain is felt, shooting to the ear, when the patient can scarcely open his mouth, or protrude or move his tongue, when there is more than usual *external* swelling, and when the symptoms increase, or even fail to remit, after five or six days have elapsed. Rigors sometimes accompany and announce the suppuration; and afterwards the pus may be often seen shining through the membrane covering the tonsils. In many cases, however, it lies so deep that it cannot be detected by the most careful examination. At length the little abscess bursts; and the relief thereupon experienced by the patient is sudden and striking. All at once the pain ceases; he can swallow, and he feels himself well; and often indeed he is well, or nearly so. The matter discharged has always a nauseous taste and a remarkably fetid smell: and sometimes the fœtor, or the ill taste, is the only sign, besides the improvement in the symptoms, that indicates the rupture of the abscess; the pus being so small in quantity as, when mixed up with other matters, easily to escape notice, or (what frequently happens), passing backwards into the stomach by an involuntary movement of deglutition. Suppuration sometimes, but rarely, takes place externally, in the cellular tissue of the neck, as well as internally.

The termination in gangrene is spoken of in books; and it is just possible, but it never happens, I imagine, except in unhealthy con-

stitutions; or when tonsillitis occurs as a complication of more general disease, such as scarlet fever. It is much more common to see repeated attacks of cynanche tonsillaris produce, what used to be absurdly called scirrhus, a permanent enlargement and hardening of the tonsils. While they are in this state, a low kind of inflammation is very readily reexcited in them. The enlarged tonsils are generally irregular, and notched; something like the surface of a walnut shell; and a white or yellow secretion is often to be seen lying in the hollows. This appearance may easily be mistaken, by an inexperienced eye, for ulceration.

There is not much risk of your confounding cynanche tonsillaris with any other complaint. The various species of cynanche enumerated by Cullen are separated each from the others by certain broad lines of distinction: all derived, however, from different combinations of two symptoms;—*dyspnœa* and *dysphagia*. Thus, in cynanche tonsillaris, deglutition alone is difficult; and when you look into the throat you see at once why it is difficult. There is equal difficulty of swallowing, and equal freedom of respiration, in cynanche *pharyngea*: but the cause of the dysphagia is not visible. In cynanche *trachealis*, the respiration is much affected; the facility of swallowing not affected at all. In cynanche *laryngea* both these functions are implicated: there is difficulty of swallowing as well as difficulty of breathing. Cynanche *parotidæa* is distinguished by the absence of both the symptoms: its title to the name of cynanche is therefore equivocal. Many of these points will soon come under our notice again.

Cynanche tonsillaris is supposed to be most frequent among the young and plethoric. But it certainly occurs very often also in persons who are pale, and spare, and feeble; and in those of middle age. Some individuals appear to have, by nature, a strong predisposition to the disease; and in them its attacks are more or less periodical; recurring at particular seasons, and commonly during the variable weather of spring or autumn. This peculiarity runs sometimes in families. The liability to the complaint is also very much increased by repetitions of the attacks.

The only exciting cause worth mentioning, almost the sole cause indeed, is exposure to cold. Not unfrequently it assails so many persons at the same time as to be epidemic; and for that reason it has sometimes been thought contagious. But it has no contagious property whatever. Although we often see several members of the same family affected by it at once, yet we may learn, upon careful inquiry, that its commencement in the different cases has been too nearly simultaneous to admit the supposition of its

having been communicated from one to another. The patients have all been exposed to the same unwholesome influences, which operating upon similar constitutions, such as those who are sprung from the same parents may be expected to possess, have produced similar effects. This prevalence of the disorder at certain times and places, is connected, no doubt, with some peculiar conditions of the atmosphere.

The prognosis is almost always favourable. Not but what death may be produced by this disease, under peculiar circumstances, and when the inflammation is unusually violent and extensive. The late Dr. Gregory, of Edinburgh, used to mention in his lectures one instance, the only one he had met with among many hundred cases, of death from cynanche tonsillaris. He did not see the patient till he was moribund; and he suspected that it was combined with typhus fever. The only fatal case that I ever witnessed occurred some years ago at the Middlesex Hospital, in one of my own patients. He was a stout young man, 26 years old, a private coachman. The complaint was clearly traced to his having got wet through, more than once, a day or two before it came on. Besides the ordinary symptoms of cynanche tonsillaris, there was great external swelling on both sides of the throat, and the patient was unable either to open his mouth, or to move his tongue. The inflammation involved not the tonsils merely, but the base of the tongue, the salivary glands, and the surrounding cellular tissue. At length suppuration took place. The abscess broke internally, and pointed also externally, just below the symphysis of the chin, where it was opened with a lancet. Two days after there was a sudden gush of blood from the mouth. So immovable were his jaws that it was impossible to determine from which side the hæmorrhage proceeded; it was stopped, however, apparently by the treatment adopted. A fortnight later, the bleeding recurred profusely. It was now evident that the blood was arterial, and that it came from the left side of the throat. Preparations were made for tying the common carotid on the left side; but just as the operation was about to be begun, the patient expired, in our presence. His death was frightful, but full of pathological interest. He did not sink, as you may have supposed, in the way of syncope, from loss of blood; but by suffocation. The blood passed down the trachea and into the lungs; and he had been so weakened by the previous hæmorrhage, that he could not expel the blood so introduced, which actually choked him. A large clot was afterwards found, filling up the windpipe. I felt this man's pulse beat firmly and regularly for a minute perhaps after his last effort to breathe. On examining the body it was discovered that

the abscess had opened internally behind and below the left tonsil. The lingual branch of the carotid artery crossed the site of the abscess; and had been severed and laid open by ulceration. From this vessel the fatal hæmorrhage had come.

It should be borne in mind also that cynanche tonsillaris does sometimes, by extension of the inflammation to the neighbouring parts, superinduce that very formidable species of cynanche, of which I am soon to speak, the *cynanche laryngea*. All cases in which the *breathing* is in any degree affected, should excite suspicion, and strict scrutiny; although the dyspnoea may be produced by the mere swelling of the tonsils.

You will understand, then, that cynanche tonsillaris *may*, under unusual and untoward circumstances, prove a fatal disease; but that it is so very rarely indeed. In almost all cases we may say that the life of the patient is not in danger.

Treatment.—In the uncomplicated and milder form of the disease, when the inflammation is superficial and the fever slight, no great activity of treatment is requisite. The patient should be kept within doors, and even in bed: for a troublesome tendency to a recurrence of the disorder may be fostered by neglect or imprudence. Cooling saline purgatives will be proper, and the antiphlogistic regimen. A strip of flannel may be put round the neck, and some stimulating embrocation applied to the exterior of the throat, beneath the ramus of the jaw: the compound camphor liniment is well adapted to this purpose. Some such plan as this will generally suffice, not indeed to stop the inflammation of a sudden, nor to put an end at once to the fever, but to cause the complaint to run its course evenly, and to go on to resolution in a few days. Commonly it is not completely over until both the tonsils have been attacked in succession.

When you catch the disorder in its very outset, I believe you may sometimes succeed in cutting it short by an emetic: a scruple of ipecacuanha for example, with a grain of tartarized antimony.

A great variety of astringent, acid, and other gargles, have been employed in this disease; and their good effects have, I apprehend, been much over-rated. Many cases would do quite as well, or better, without them: for in the early stages strong astringents, and the straining and movements of the throat that accompany their use, may even be hurtful, and increase the pain and the inflammation. The only gargle which I should consider admissible in the commencement of the malady is a gargle of warm milk and water. I have known of one instance in which quinsy suddenly attacked a gentleman who was extremely anxious to

use his throat, in public speaking, the next day. He occupied himself perpetually, for some hours, in this sort of fomentation of the tonsils with hot water; and with such good effect that on the day following he was able to accomplish his object. Still there are cases in which, at certain stages of the disease, detergent gargles are serviceable, by assisting the excretion of the mucus that collects in the fauces, and by correcting fetor. A weak solution of chlorine in water answers well. In more chronic sore-throats, stimulating gargles may often be employed with advantage. When the inflammation is violent a slightly stimulant linctus is preferable; it *cuts the phlegm* as they say, *i. e.* it promotes its detachment and removal. Of this kind, currant jelly is one of the best.

But far better than any thing else, as a local application to the inflamed fauces, is the steam of hot water: whether we are hoping for resolution of the inflammation, or whether we desire to promote and hasten the process of suppuration already begun. The inhaler introduced by myself into the Middlesex Hospital, and elsewhere, though somewhat clumsy in appearance, is the most convenient and effectual that I am acquainted with. I show it to you. It was invented in Edinburgh by a friend of mine long since dead, Mr. Hercy. It will stand upon a table, or lie upon a pillow; and a large volume of steam is carried inwards against the fauces by the mere natural breathings of the patient. Most of the inhaling machines that I have seen require a sucking effort, like that made in smoking a pipe: an effort that is apt to be irksome and fatiguing, especially in pulmonary diseases; for some of which this method of applying vapour directly to the suffering part is as useful as it is for sore-throats.

Blistering the outside of the throat is a favourite remedy with many. When early applied a blister often does much good, and probably prevents suppuration in some cases. But I have found blisters of uncertain efficacy; they leave a mark which lasts for some time, and which patients of the other sex are apt to complain of. For these reasons I prefer mere rubefacients; the liniment I mentioned before, or the compound soap liniment, or a mustard poultice folded between two layers of thin linen.

In more severe cases leeches applied to the upper part of the throat, just below the angles of the jaw, have been found to give sensible relief: and in the worst degrees of the disorder, when there is much outward swelling, and the jaws and tongue are fixed, leeches are absolutely requisite. It will frequently be proper to take blood from the arm also. The necessity for active depletion must be measured by the severity of the

local symptoms, the intensity of the fever, and the general strength and condition of the patient: and of these things a little experience will teach you to judge.

It is not to be expected that either leeches or blisters will be of much use after the process of suppuration has commenced; nay they may sometimes be injurious by retarding it. It is frequently a difficult matter to determine whether pus has yet formed or not.

I have already admonished you to make a careful examination of the throat, and to watch your patient narrowly, whenever he experiences any difficulty of breathing. Dyspnoea may be produced by the mere swelling of the inflamed part: and when it concurs with much enlargement of the tonsils you had better pierce them with a lancet. If they contain matter, it will be evacuated; and if not, the bleeding produced by the puncture will generally reduce the swelling somewhat, and relieve the patient. There is an instrument made on purpose for this small operation, consisting of a lancet enclosed in a flat silver sheath, from the end of which it is made to protrude, to a certain extent only, by pressing upon a spring. The instrument should be directed towards the centre of the fauces, and not outwards, in order to avoid wounding important vessels or nerves. Dr. Cullen indeed says "this does not require much caution:" but notwithstanding this high authority I must warn you that puncturing or scarifying the tonsils is an operation not to be carelessly, or rashly, or wantonly performed. Portal mentions a case in which a skilful surgeon in scarifying the tonsil of his patient, wounded as he supposes some ramification of the internal carotid, and the patient was presently dead. That artery lies, as you know, very near the tonsil; and only a very few years ago, in Ireland, it was struck by a surgeon while scarifying a gentleman's tonsil; and the gentleman died in three minutes. This I was told by the late Dr. Barclay. The case I related just now of fatal hæmorrhage from the lingual artery points to the same danger: and since that case occurred two others involving similar hazard have fallen under my own notice, and impressed me with a strong feeling of the necessity of caution. A man was brought into the hospital with profuse hæmorrhage from the right tonsil or its immediate neighbourhood, the consequence of syphilitic ulceration of those parts. He had lost three or four quarts of blood, and was nearly dead. His life was saved by Mr. Mayo, who tied the common carotid on that side.

Last February (1838) a boy, from Harrow School, was placed under my care, in whom cynanche tonsillaris came on during convalescence from scarlet fever. So much

swelling was there of both tonsils, that they met, and pushed the uvula outwards before them, and the breathing was much impeded. A surgeon who was in attendance with me punctured the tonsils. The next day a good deal of hæmorrhage took place; and this recurred, several times, to a considerable and even an alarming amount. When the clots that formed were wiped away with a sponge, the blood could be seen welling out in a little stream, with a pulsating motion, from a small incision in the left tonsil. The hæmorrhage was ultimately, after much trouble and anxiety, arrested, by applying a pencil of lunar caustic freely, within the bleeding orifice. Lint, wetted with the muriated tincture of iron, or with a saturated solution of alum, is a fit application in similar accidents.

Mr. Lawrence, who saw this case, told me that he once knew a patient die of hæmorrhage from the tonsillar artery.

I ought, perhaps, here to add, that, very recently, Mr. Joseph Bell, of Barrhead, has strongly recommended the internal administration of powdered guaiacum, in large doses, as being almost specific in the cure of cynanche tonsillaris. He gives as much as half a drachm, suspended, by means of mucilage, in a draught, every six hours. Mr. Bell has no doubt that this remedy, if timely administered, will cut the disease short in ninety-nine cases out of a hundred. It has been found successful in other hands also. I have never had an opportunity of trying it.

The chronic enlargement of the tonsils, to which I have already adverted, is sometimes productive of great inconvenience and distress, and even of danger. Its occasional consequences are—an habitual trouble in swallowing; confused and inarticulate speech; deafness in various degrees, from occlusion of the eustachian tubes; more or less impediment of breathing; and even spasm of the glottis, and impending suffocation. The enlargement may in such cases be somewhat reduced, I believe, by repeatedly passing a stick of lunar caustic over the surface of the tonsils; but a much readier and better plan is to amputate them, in part at least. This may be done by a ligature; or still better by scissors, or by a sort of small guillotine invented for that purpose. It is not a very painful operation. Mr. Arnott removed one lately for one of my hospital patients; and a very few days ago (Dec. 1838) Mr. Mayo brought two, in a piece of paper, to the hospital. He had just before cut them off for a patient whose respiration they had much embarrassed.

ON

DEFECTIVE CLOSURE OF THE TRICUSPID FORAMEN AS A FREQUENT CAUSE OF DROPSY.

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[For the Medical Gazette.]

NUMEROUS morbid appearances have been found in the hearts of persons, who, during life time, have had serous infiltration, and other symptoms indicative of obstruction existing at the centre of the circulation; and such morbid appearances have been usually considered as the causes of this obstruction. Those most commonly met with are diseases of the different valves, by which the respective openings to which they are attached have been contracted, or rendered permanently patulous—an increase or diminution of the substance of the walls of the heart, and of the capacity of its cavities.

Now, with the exception of those diseases which render the tricuspid valves incomplete, there is not one of these appearances which may not be met with in the bodies of persons who have never presented the slightest trace of dropsy.

As regards valvular disease, Bouillaud relates a case in which the mitral foramen was contracted to an opening of three lines diameter; and although this had in all probability existed three years, the ankles only became slightly œdematous a little before death. I have in my possession two preparations in which the same foramen is much contracted and rendered patulous. During life-time there were several attacks of cerebral and of pulmonary apoplexy with hæmoptysis, but no dropsy.

Dr. Hope describes a case in which the aortic orifice was reduced to the size of a pea, without any serous infiltration; and in this paper two instances of extreme narrowing of the same orifice will be adduced, which had evidently been of long standing. In one of these, dropsy did not come on till a few months before death, and in the other the ankles became œdematous only a few weeks previous to that event.

* Read at the Royal Medical and Chirurgical Society of London, Jan. 12, 1841.

The phenomena observed in these two cases may, in some measure, tend to explain why such extensive obstruction to the circulation may exist at the origin of the aorta without greatly affecting the general health. The heart's action was fluttering, and was accompanied by a very *prolonged* rasp sound, thus indicating the continued action of the ventricle, whereby the blood was at length squeezed through the contracted foramen, and explaining the cause of the thready vibratory motion which replaces the pulse in such cases. Andral relates a case* of extreme narrowing of the aortic orifice by ossific deposit, and states it as certain that many aged persons are similarly affected without being attacked by dyspnoea or dropsy. He attempts to account for the absence of such effects by the fact of the slowness of the circulation in aged persons; but it appears to me that the explanation given, based on the observation of Cases XI. and XIII. is more satisfactory.

Since, therefore, serous infiltration does not seem to depend on the degree of obstruction to the circulation existing at the left side of the heart, it is probable that there is some other obstacle more direct in its action and more constantly present. Dr. Hope, who takes this view, considers hypertrophy and dilatation of the heart to be the cause of obstruction. He remarks, "So long as the heart remains free from dilatation, hypertrophy, or softening, the valvular disease, according to my observation, is not in general productive of great inconvenience. This opinion is founded on the following grounds:-- I have seen individuals, who were affected in an eminent degree with disease of the valves of the aorta, maintain for years a very tolerable state of health so long as there was no hypertrophy or dilatation of the heart: but, in proportion as these supervened, the symptoms of valvular obstruction became more and more developed, and eventually assumed their most aggravated form. I have reason to believe that in these cases the symptoms were attributable in a great measure to hypertrophy or dilatation, because I have seen a greater valvular contraction produce less severe symptoms when the hypertrophy or dilatation was less con-

siderable. I have not only seen numerous cases of a mechanical obstacle unattended with passive congestions, dropsy, &c. but I have seen a still greater number of instances in which all the phenomena of an obstructed circulation were occasioned by dilatation alone, as no other obstacle capable of accounting for them could be detected in the course of circulation." And again, "I have repeatedly witnessed cases in which a well-marked, if not a considerable obstacle, as a contracted valve, a regurgitation, or a dilatation, or aneurism of the aorta, had subsisted for a long period, even for years, without producing any material symptom of an obstructed circulation; but the moment that dilatation of the heart supervened the symptoms made their appearance in an aggravated form."

Fully agreeing with Dr. Hope as to the frequency of hypertrophy and dilatation of the heart in cardiac dropsy, more especially dilatation, I do not think that they can be considered as the direct causes of it. "Every day (says Bouillaud) we find otherwise skilful physicians attributing to dilatation of the heart a number of evil effects, which in fact are only accidentally joined with it, such as passive congestion and the sense of suffocation which generally accompanies it." Active hypertrophy of the left ventricle may so increase its power that too much blood may possibly be thrown into the capillaries, thereby giving rise to congestion and serous infiltration; and concentric hypertrophy, whereby a diminution of the capacity of any one cavity of the heart takes place, must create a certain obstruction to the circulation; and yet numerous cases could be adduced in which each of these morbid states of the heart existed without having given rise to dropsy.

If the heart be generally dilated, and at the same time attenuated, it may be considered that it has not sufficient power to carry on the circulation properly, so that congestion may supervene; but this is only one particular case.

On the other hand, when there is general hypertrophy and dilatation of the heart, so that the proportion between all the cavities is preserved, no cause of obstruction can be recognised; and yet such cases are very commonly accompanied by dropsy. Andral, al-

* Clinique Médicale, Brussels, 1834, vol. i. p. 50.

luding to such cases, observes, "What can produce dropsy in such a case? For all the cavities being in the same physical condition, ought they not to receive and forward the blood with equal proportion and freedom? If there is no obstacle to the circulation, no stagnation, no retardation of the course of blood in the heart, there ought to be no dropsy."

I think, however, that I shall be able to shew that there does exist an obstacle to the circulation in such cases, and if so it will be useless to search for the cause of dropsy, with Andral, "in the excess of the capacity of the heart relative to that which has been preserved in the blood-vessels."

As, indeed, dilatation of the heart, with or without hypertrophy, is, of all others, the most constant alteration of this organ, coexisting with cardiac dropsy, it is highly probable that some additional obstacle to the circulation is somehow connected with it, if not dependent upon it: and as the most direct obstruction to the venous circulation must exist on the right side of the heart, it is still further probable that it is connected with dilatation of that side.

Now the dilatation of the right ventricle necessarily gives rise to the enlargement of the tricuspid foramen*, unless in such cases where the fibrinous zone, from which its valves spring, shall have to a certain extent lost its natural elasticity. One such case I have seen. The tricuspid valves intended to effect the closure of this aperture during the contraction of the ventricle, are only just sufficient for this purpose. Hunter, and at a later period Dr. Adams and Mr. King, have observed that they do not very effectually close the orifice they are attached to in its healthy state. Consequently, if this orifice become dilated, the valves can no longer effect its closure, except they also increase in size. In some cases of hypertrophy such is the case, (Case V.) but I think not so generally as Dr. Hope supposes; at least, as far as my own observations, confirmed by those of Bouillaud, would lead me to suppose.

Hence, regurgitation must not unfrequently take place through the tricuspid foramen during the systole of a dilated right ventricle; and thus a most powerful obstacle is opposed to the venous current, by a quantity of blood being constantly forced back upon it: and although this may in certain cases act as a safety-valve to the lungs, as supposed by Mr. King, yet if at all extensive and continuous, it can hardly fail to offer a most effective obstruction to the circulation returning from the system. This regurgitation through the tricuspid foramen I propose to shew, by the following cases, is a frequent and direct cause of cardiac dropsy*. The same obstruction would be produced by any other causes which prevent the tricuspid valves from closing the foramen to which they are attached, whether dilated or not. There are two causes of this sort which have hardly received the attention they deserve. The one is a shortening and thickening of the *cordæ tendiniæ*, which Mr. Hodgson tells me he has frequently seen; and the other, a partial or total adherence of the valves to the walls of the ventricle, unaccompanied by any other traces of disease. Instances of both kinds will be found in this paper. (Cases I. II. X. XI.)

A difference of opinion seems to ex-

* Dr. Copland, in his Dictionary, Part V. article Heart, page 212, observes, "Dilatation gives rise to no indications of its existence, unless it is so considerable as to permit a reflux of the current of blood, and even then the signs are equivocal. This influx is one of the causes of the bellows sound and of the purring tremor. When it takes place through the right auriculo-ventricular opening, it causes venous pulsations." But there is not a word of dropsy; nor can we suppose from this that he considers regurgitation through the tricuspid foramen in any way intimately connected with dropsy, much less that it is a frequent and direct cause of cardiac dropsy. It will be shewn in this paper that such regurgitation does not necessarily give rise to venous pulsations. Dr. Adams writes, "Mr. Hunter, in his treatise on the Blood, has remarked that the valves of the right side of the heart do not so completely close the arterial and auricular openings as those of the left; but this circumstance, in my opinion, has not been sufficiently noticed, nor the influence that such a structure may have upon the circulation in its natural or morbid state considered." He then goes on to consider the safety-valve function, and the mechanism of venous regurgitation and pulsation; but there he stops, and does not insist upon the additional obstacle to the circulation produced by morbid regurgitation. Farther on he recommends accurately measuring the relative proportion between the valves and the opening, in all cases of regurgitation, looking especially to the presence or absence of pulmonary obstruction in these cases.—*Dublin Hospital Reports*, vol. iv. pp. 437-8.

* Dr. Copland, in his Dictionary, Part V. article Heart, p. 210, remarks, "When both the ventricle and auricle of the same side are much dilated, the intermediate orifice is generally also widened, and the valves insufficient to close it."

ist between writers on diseases of the heart, as to the sufficiency of the proofs of regurgitation through the tricuspid foramen, both as drawn from observation during life and autopsy after death. Thus pulsation of the jugular veins, synchronous or nearly so with the systole of the ventricles, which have been considered by most writers as indicative of such regurgitation, are looked upon by Dr. Hope as produced by the force of the right ventricular systole, and altogether independent of the completeness or incompleteness of the tricuspid valves.

It must be borne in mind, however, that considerable hypertrophy of the right ventricle is not unfrequently found in the bodies of persons who during life-time presented no appearance of jugular pulsation. And were it possible that the ventricle could ever contract with such force as to communicate a shock to the valves and blood above them, which should run up the jugular veins, it could only be in cases of extreme hypertrophy of the right ventricle, and therefore would be of rare occurrence, and would be accompanied by signs of such hypertrophy.

When, therefore, pulsations or obscure fluctuations of the jugular veins are observed, unaccompanied by any very strong heaving impulse of the heart, it is, I think, highly probable that regurgitation takes place through the tricuspid foramen. A perusal of the cases brought forward in this paper will be found very much to favour this view, inasmuch as in all those where venous pulsations or undulations were observed during life-time, the tricuspid foramen was found incomplete, either from its dilatation or the imperfect action of its valves, or from both causes.

That the force of pulsation of the jugular veins is a measure of the power of the right ventricle, and not a measure of the degree of regurgitation and obstruction to the circulation, I readily admit; for in all cases the stronger be the force of the right ventricle which throws the blood back upon the veins, the stronger will be the pulsation propagated up them. Not so as regards the obstruction to the circulation, which depends on the relative proportion between the power of the two ventricles. If both be hypertrophied, the pulsation or shock will be great, and if both be

attenuated the pulsation will be weak, while the obstruction to the circulation is the same in each case; for the venous current and that of the regurgitating fluid, being both mainly derived from ventricular contraction, when the one is strong it is opposed by the other equally strong; and when one is weak, by the other equally weak: the obstruction is therefore the same in each case, and it is perfectly independent of the shock or pulsation. This is strikingly illustrated in Case XIII., where the pulsation of the jugular veins was most vehement, but the effect on the circulation slight.

If the proportion between the power of the two ventricles be altered, the effect on the circulation is altered; thus, if the right ventricle be hypertrophied and the left attenuated, a weak venous current is opposed by a powerful current of regurgitation, and the circulation is powerfully obstructed; if, on the other hand, the left ventricle be hypertrophied and the right attenuated, a strong venous current is opposed by a weak current of regurgitation, and a feeble resistance is offered to the circulation.

While, therefore, pulsations of the jugular veins may, in certain cases, be considered as indicative of regurgitation through the tricuspid foramen, their absence affords no proof of the non-existence of such regurgitation.

It has been proposed to test the completeness of the tricuspid valves after death by the manner in which they retain fluid in the right ventricle, which has either been forced or injected into it through the pulmonary artery. Mr. King has seldom found the valves to retain fluid when the experiment has been made with a healthy heart; and I have several times repeated the experiment with no better success, unless I pinched in the base of the right ventricle surrounding the foramen with my hand. This therefore would be an unfair test, as it would go to prove that almost all tricuspid valves were incomplete. Bouillaud has compared the measure of the circumference of the foramen with the height of the valves from their apex to the middle of their base. This latter plan gives a very insufficient idea of the area the valves can cover; because unless the circumference of the foramen be retained in its proper position, which is

very difficult to do, the valves will be stretched, and will measure more than they can do in action.

He gives three inches eleven lines as the mean circumference of an undilated tricuspid foramen.

I have been in the habit of removing the apex of the heart and laying open the right auricle, and then raising up the tricuspid vales into their plane of closure, by means of my fingers introduced into the right ventricle. By looking at them from the auricle when in this position, a tolerably accurate idea can be formed of the area they cover, and whether any and how much space is left uncovered by them, through which the blood can regurgitate. I have then usually gauged the tricuspid foramen by the introduction of the fingers; its ordinary size not quite admitting the three first fingers up to their first joint, and then measured the circumference after it has been laid open. I regret that the size of the heart should have been noted in such loose terms. I have lately measured its bulk and capacity, by ascertaining the quantity of fluid it displaces both when full and empty, and hope by this means to arrive at a more accurate standard of measurement.

In the cases which follow, and which I have condensed as much as possible, the heart presented a great variety of morbid appearances; but one peculiar state was common to them all, and that was the incompleteness of the closure of the tricuspid foramen. In two cases this arose from adhesion of one or more valves to the ventricle; in eleven from dilatation of the foramen to such an extent that the valves could not effect its closure. In most of them the dropsical effusion was preceded by turgescence of the jugular veins: in those of them where the heart was not softened or attenuated, it was preceded by pulsations in these vessels. I shall not in this place enter upon the causes which tended to produce dilatation, which will in most of the cases appear tolerably clear.

Being anxious to know how far the views in this paper would be borne out by cases recorded by others, I have consulted the work of Bouillaud, who is the only writer on diseases of the heart that has carefully measured its orifices. He gives thirty-four cases of cardiac dropsy, of which the following table gives the analysis:—

Analysis of 34 Cases of Cardiac Dropsy, detailed by Bouillaud.

Tricuspid foramen dilated and its closure incomplete, with the valves adherent in some	16
Tricuspid valve diseased so as to interfere with its action	4
Their chordæ tendiniæ shortened, producing the same effect	2
Concentric hypertrophy of right ventricle, with great diminution of cavity	2
Foramen dilated, and valves enlarged, so as to close it	1
Valves and foramen healthy	2
No mention of foramen, but both right auricle and ventricle dilated	7
	—
	34

In these last seven cases there appears to have been no measurement of the orifices of the heart, but as there was dilatation of both ventricle and auricle on the right side, we may conclude that the foramen between them was also dilated.

If this be allowed, it would follow that there were only three cases out of the thirty-four in which there was complete closure of the tricuspid foramen.

From this it might be supposed that Bouillaud was fully aware of the frequency of this pathological state of the heart, and of its intimate relation to dropsy as cause and effect. He simply remarks, however, "The dilatation of the orifices of the heart is not less common than that of the cavities themselves," and he recommends "diligent observers not to neglect examining the orifices of the heart in all its organic diseases, for if the dimensions are such that the valves cannot close the orifices, the circulation of the blood through the heart must be more or less seriously disturbed*."

Here he stops short, and does not seem to be aware of the extent of obstruction which is thus so frequently offered to the circulation, and that in a majority of cases dropsy does not come on till the tricuspid valves have become incompetent to close the dilated orifice to which they are attached. Overlooking the fact that contractions of the orifice of the left side are not uncommonly found unaccompanied either by extensive dilatation or by dropsy, he considers, throughout the whole of his work, that these contractions are the main cause of those disturbances to

* Paris, 1835, vol. ii. p. 526.

the circulation, of which dropsy is the most frequent and the most dangerous consequence. Nor does he seem to suspect that incomplete closure of the tricuspid orifice and consequent regurgitation through it, is the real and only impediment to the circulation in those cases where the heart is dilated and hypertrophied in an equable manner through its whole extent.

CASE I.—A married lady, æt. 29, had not enjoyed good health since an attack of rheumatic fever in early youth. She had for four months suffered from dyspnœa, during which time she constantly expectorated clear mucus, sometimes mixed with dark blood, and had occasional attacks of severe pain in both sides, with fever. She could not lie flat; her urine was not coagulable by heat. In about a month after this her legs began to swell, and a slight bellows sound was heard over the præcordial region, with the first sound of the heart, when she was hurried or excited. The veins of the neck, which had always been rather full, now became much swollen, and pulsated strongly. The œdema of the legs increased, ascites appeared, and she died, the subject of extensive anasarca, three months after my first visit.

Autopsy.—There was considerable serous effusion in both sides of the chest and in the abdomen. Some patches of circumscribed pulmonary apoplexy were seen in the lungs.

The heart was more than twice the size of the fist, being hypertrophied and dilated throughout. Two flaps of the tricuspid valve acted imperfectly, one being partially, and the other entirely adherent to the right ventricle. The tricuspid foramen was greatly dilated, and its circumference measured five inches and a half. An immense transparent coagulum, of the consistence of jelly, was lying in it, extending into both the auricle and the ventricle. The other organs were healthy.

CASE II.—Sarah Richards, æt. 14, pigeon-breasted; had rheumatic fever twelve months since, with very severe pain under the left breast, and distressing dyspnœa. When examined, complained of great dyspnœa, and occasional severe pain in the præcordial region; the urine was not coagulable by heat.

On the left side, the chest sounded dull from the second rib downwards. A purring thrill was felt under the left

mamma. At the same spot bellows sound was constantly heard, and for some distance around it.

In two months' time the veins of the neck were observed to fluctuate obscurely. In another fortnight the ankles and legs became œdematous, ascites soon followed, and in three months from my first visit she died extensively anasarcaous.

Autopsy.—A large quantity of clear lemon-coloured fluid was found in the abdomen, about a pint in each pleural sac, and three ounces in the pericardium. The lower part of each lung was slightly carnified; they were otherwise healthy.

The heart was dilated to nearly double its natural size; the walls retaining their usual thickness. The valves were not thickened, but a part of the tricuspid valves was glued down to the side of the right ventricle, thereby preventing the closure of the foramen, the circumference of which measured four inches and a quarter. The other organs were healthy.

REMARKS ON CASES I. and II.—The hypertrophy and dilatation of the heart in Case I., in all probability resulted from an attack of pericarditis and endocarditis during rheumatic fever—a result which Andral and others have shewn to be by no means uncommon. The intimate adhesion of one of the tricuspid valves to the right ventricle may also have taken place at the same time; but when we recollect that this fever took place in early youth, it is hardly possible to believe that incompleteness of the tricuspid valves could have existed for so long a period without giving rise to more serious consequences. The hypertrophy of both ventricles accounts for the strong pulsations observed in the jugular veins. The manner in which their appearance and increase was followed by dropsical effusion cannot but lead to the belief that it depended on the dilatation and consequent incompleteness and regurgitation through the tricuspid foramen.

In Case II. nearly the same phenomena were observable, with this difference, that there was only obscure fluctuation and not pulsation observable in the veins of the neck, and this because the ventricle was not hypertrophied.

I may here remark that Mr. Baynham has given me the particulars of a case

of general hypertrophy and dilatation of the heart, with incompleteness of the tricuspid valves from the enlargement of their orifice, unaccompanied by dropsy. The foramen ovale, however, was unclosed and spacious, so that the shock of regurgitation was divided between the veins of the general circulation and those of the lungs. As might be expected from this, there was much dyspnœa in this case, the foramen ovale acting as a kind of safety valve to the general circulation at the expense of that of the lungs.

CASE III.—Mary Newey, æt. 60, some years ago had an attack of acute rheumatism, since which time she has had a cough, with copious clear expectoration. During the last seven months she had suffered much from dyspnœa and palpitations; her pulse was feeble and irregular; the veins of the neck were much swollen, and presented a knotted appearance; they pulsated faintly. There was dulness for a large space around the præcordial region. The sounds of respiration and of the voice were natural. Bellows sound was heard over the heart. The heart's action was irregular; her urine was not coagulable by heat. The ankles were œdematous, and serous infiltration was becoming apparent in the thighs, when she was suddenly seized with apoplexy, and died in twelve hours.

Autopsy.—A great quantity of black coagulum was spread over the surface of each hemisphere of the brain, between the dura mater and the arachnoid membrane. There was a clot and laceration in the interior of each hemisphere. The heart was dilated to an enormous size, the walls being of the usual thickness, soft and flabby, of a yellow mottled colour, crackling under the fingers like a piece of lung. No valves were diseased.

The circumference of the tricuspid foramen measured nearly six inches, and its valves could not effect its closure. The other organs were healthy.

REMARKS ON CASE III.—Here again the abnormal changes in the heart had probably their origin in the inflammatory action which was set up during rheumatic fever. While the symptoms of venous obstruction and dropsy were developing themselves, the patient was suddenly cut off by apoplexy. There can be little doubt that this was produced by

the regurgitation of the venous blood through the dilated tricuspid opening, a similar instance of which came under my notice a short time since. A man had been under my care for twelve months, presenting all the symptoms of an hypertrophied heart succeeding rheumatic fever. Having one day stooped down much in the course of his employment, he was taken suddenly ill, and expired in ten minutes. The heart was found to be large and firm, being both hypertrophied and dilated. The tricuspid foramen measured five inches in circumference. The brain was gorged with dark blood, of which an immense quantity issued from the jugular veins.

It may be said that the soft and flabby state of the heart, by diminishing its muscular energy, and thus rendering it incompetent to propel the blood with sufficient force, would retard the circulation and give rise to the commencing anasarca, and would dispose to the apoplectic attack. Such a state of the heart might have this tendency; and yet I have several times found the heart in a similar state when no apoplexy and no trace of dropsy had been present.

CASE IV.—John Higginson, æt. 46, boot-maker, an old soldier, who had lost the right leg above the knee; had been accustomed to drink spirits to excess. Nine months since he felt a pain under the sternum, followed by dyspnœa, both of which have continued ever since. When I visited him the dyspnœa was most distressing; he was unable to lie flat, and had a strong sensation of constriction across his chest. There was œdema of the leg and stump, and some ascites. The jugular veins pulsated strongly. There was dulness on percussion for some inches around the præcordial region. The respiratory sound was natural. The action of the heart was heaving, and its sounds distant. The pulse was very full and hard, and its stroke remarkably quick and sharp, which character it retained till death took place, three weeks after my first visit, having been preceded by expectoration of dark clotted blood for two days.

Autopsy.—There was serous effusion in the chest and abdomen. A patch of pulmonary apoplexy as large as an orange existed about the middle of the concave portion of the right lung, near

the pericardium, and was continued gradually into the healthy portion of the lungs. The heart was enormously hypertrophied and dilated. On its surface was a patch of thick yellow lymph. The dilatation of the right cavities was very great; the circumference of the tricuspid foramen was five inches and three quarters; one of the valves had short thickened chordæ tendiniæ; they could not close the foramen.

The aortic valves were thickened, but acted well. There were many patches of atheroma under the lining membrane of the left auricle. The aorta was a mass of disease, being thickened and puckered, and, in many places, denuded of its lining membrane, and so throughout all the large arteries.

The liver was of a nutmeg colour. The kidneys and other organs healthy.

With this last case I would contrast the following, wherein, in addition to the disease of the arteries, and the hypertrophy and dilatation of the heart, with enlargement of the tricuspid foramen, the tricuspid valves were so increased in size as to effect its closure, and hence there was no dropsy:—

CASE V.—Priscilla Sturck, æt. 50: had a severe illness, with pain in left side, eight years ago, since which times she has occasionally felt beatings extending from her heart up to her head and down her arms, with giddiness. She has had latterly a constant cough, and when examined the pulse was small. Pulsations were felt above each clavicle. The action of the heart was heaving and tumultuous. The carotid arteries pulsated very strongly. Double saw sound was heard, its maximum being under the bottom of the sternum. Single saw sound under each clavicle, loudest under the right, was heard, without any second sound. Two years after this, being excited, she suddenly placed her hand over her heart, drooped her head, and died in half an hour.

Autopsy.—The heart was very much increased in size, being generally hypertrophied and dilated, more especially the left ventricle. The tricuspid foramen measured five inches in circumference; but the valves were hypertrophied in substance and extent, so that they effected the closure of the foramen perfectly. The aortic valves were slightly cartilaginous, but acted well. The lining membrane of all the arteries, particularly the aorta, was much dis-

eased, being puckered and having much atheroma under it, and being deficient in some places. The arteries of the arms were diminished in calibre by the deposition of atheroma in their middle coats. The other organs were healthy.

REMARKS ON CASES IV. and V.—In Case IV. the hypertrophy and dilatation of the heart, and the diseased state of the arteries, would seem to have been induced by that sub-acute inflammation of the surrounding and lining membrane of the heart and large arteries, to which spirit-drinkers are subject. The tricuspid valves retaining their usual size, while the orifice to which they were attached was enormously dilated, strong regurgitation would take place, and, as the heart was firm and hypertrophied, would be marked by strong venous pulsations.

In Case V. a similar state of the heart and arteries was probably the result of an acute attack of inflammation; but as in this case the valves had increased in size in the same proportion as the orifice to which they were attached, they were capable of closing it, and no dropsy or venous pulsation ensued.

Now if in Case IV., exactly parallel to Case V. in all but the state of the tricuspid valves, the dropsy depended on any other cause than regurgitation through the incompletely closed tricuspid foramen, why did it not also supervene in Case V., where the complaint had been protracted through a period of eight years?

A comparison of these two cases goes strongly to prove that venous pulsations are not produced without regurgitation through the tricuspid foramen, because in Case V. the heart was quite as firm and as much hypertrophied as in Case IV., and yet there were no venous pulsations.

CASE VI.—William Matthews, æt. 32, porter: was attacked with acute rheumatism twelve years ago, and again eight years since, when he had very severe pains in the left breast, troublesome cough, and dyspnœa. Eighteen weeks previous to my visiting him, his ankles began to swell; then his legs and abdomen. There was much dyspnœa. The urine was not coagulable by heat. Pulse hard, sharp, and vibratory. The jugular veins were swollen, and pulsated. There was dullness on percussion at the bottom of

each side of the chest. The first sound of the heart was accompanied by marked bellows sound. In a few days time intense pain at the præcordial region suddenly made its appearance, with most distressing dyspnœa. Pulse 120, very sharp and hard, with some fulness. Venesection being freely employed, with the administration of digitalis and mercurial frictions, the acute symptoms were removed; the anasarca and the pulsation of the jugular veins disappeared. The pulse remained very hard and vibratory. In a few weeks venous pulsation of the neck, rapidly followed by anasarca, reappeared, and he gradually sank.

Autopsy.—Much fluid was found in the abdomen and thorax, and four ounces in the pericardium. This membrane was thickened and opaque in patches. The heart was generally hypertrophied, and dilated to more than twice its natural size. The tricuspid valves were a little thicker and larger than usual, but could not nearly close their foramen, which would admit four fingers and the thumb nearly up to the knuckles, and the circumference of which measured nearly seven inches. The mitral valves were thickened, but free to act. The lining membrane of the left auricle was thickened and opaque, studded with several horn-like patches. On one of the aortic valves was a patch of soft, reddish, recently organized false membrane, of the size of a fourpenny-piece. The lining membrane of the aorta seemed thickened, and puckered unevenly. The other organs were healthy.

CASE VII.—John Wilson, æt. 62, wood-turner: had an inflammatory attack of the chest eighteen months ago, since which time he has felt a tightness of the chest; for the last nine months dyspnœa, and lately palpitations. Six weeks ago his legs swelled for a few days; this went off, and returned three weeks since. Dyspnœa was most urgent. The jugular veins were seen to pulsate, chiefly on the right side; but were not turgid. The urine was not coagulable by heat.

There was dulness, more extensive than usual, around the region of the heart. The respiratory sound was coarse in places, with slight cooing. The action of the heart was tumultuous, heaving, and irregular. Pulse irregular, but moderate. The sounds of the heart

were feeble and obscure, but on the right side of the sternum the first sound was louder, being slightly coarse and prolonged.

In two or three days time he began to expectorate dark blood, sometimes in clots. This, with increasing dyspnœa and anarcarsa, continued up to his death, which took place three weeks after my first visit.

Autopsy, forty hours after death.—The legs and thighs were anasarcaous; a little fluid was found in the abdomen. At the inner edge of the back and lower portion of the upper lobe of the right lung (both of which were otherwise healthy) was a mass of pulmonary apoplexy, of the size of an orange, with abrupt edges. The heart was immense, and measured fifteen inches around the outside of the base of the ventricles. It was generally hypertrophied and dilated, firm and red. Within the folds of one of the aortic valves was a patch of semi-osseous matter, which did not prevent the free action of the valves. The circumference of the tricuspid foramen measured full six inches. One valve was large, the other two small. They were roughed by small hard substances, rather less than split mustard-seeds, particularly on their free edges. The coronary vessels were large, but healthy; and the aorta was studded with numerous soft elevations, produced by atheromatous deposit. The other organs of the body were healthy.

REMARKS ON CASES VI. AND VII.—In these cases hypertrophy and dilatation of the heart seem also to have resulted from old attacks of inflammation; but in Case VI. an acute attack of endocarditis supervened; and this being met with active treatment, the dropsy disappeared for a time. The pulsations of the veins of the neck also ceased; but whether this arose from contraction of the tricuspid foramen when the heart was relieved from the pressure of blood by venesection, whereby regurgitation was prevented; or, which is more likely, by the diminished energy of the heart's action, produced by digitalis, cannot be positively determined. All the valves were slightly increased in size, yet the tricuspid orifice was so enormously dilated that they could not nearly close it; hence, in a short time, the old symptoms reappeared in an aggravated form.

I now pass on to cases wherein the dilatation of the heart may in whole,

or in part, be attributed to other causes than inflammatory action.

CASE VIII.—A lady, æt. 43, was married at the age of thirty. Previous to her marriage she was very pale, and was subject to palpitations, dyspnœa, and swelling of the ankles. Menstruation was, at that period, very scanty and irregular. Since her marriage a slight improvement had taken place in this respect, but at times she suffers in an exactly similar manner. Has no family. For the last four months she had menstruated but very slightly, and had suffered much from flatulence and other dyspeptic symptoms. In this state she applied for advice. The sounds of the heart were feeble, but clear, and were heard over every part of the chest. The veins of the neck were slightly swollen. She derived much benefit from tonic alterative medicines, which, in a great measure, removed her dyspepsia. In a few months time she returned, with œdema of the legs and thighs, and a trace of ascites. Dyspnœa and palpitation were oppressive. The most active diuretic medicines had been employed without the least benefit. The veins of the neck were now seen to be very much more swollen, and a fluctuation in them was manifest. The urine was not coagulable by heat.

A small quantity of blood was taken from the arm, and tonics were combined with diuretics. In less than three weeks the swelling and fluctuation of the veins subsided, and every trace of dropsy disappeared.

REMARKS ON CASE VIII.—There appears to be no reason why the heart should not partake of the general flaccidity which is found in all the other muscles of persons affected with chlorotic anemia. Hence, in Case VIII. such may be supposed to have been the case; and, as clear evidence of dilatation existed, we may conclude that it arose from the diminished tone of the parietes of the heart. Hence when depletion or diuretic medicines were employed, no good effect was produced; but the moment a tonic treatment was adopted, the heart seemed gradually to regain its power and usual dimensions; and the cause departing, its effect, dropsical effusion, departed also.

CASE IX.—James Brittain, æt. 55, had, during several winters, cough,

with expectoration of pearly mucus, wheezing, dyspnœa, and, last winter, œdema of the legs. In the spring all these symptoms, except the expectoration, disappeared. When visited at the commencement of another winter, he was found to have been ill for some weeks. There was great œdema of the legs, thighs, and scrotum. Urine was scanty and high coloured; not coagulable by heat. Respiration was hurried, and he had frequent palpitations. There was dulness on percussion around the heart. Sub-crepitant râle was heard all over the chest. The sounds of the heart were sharp and clear, and were heard over the whole thorax; but seemed distant when listened to over the præcordial region. He died in fourteen days.

Autopsy.—The lungs were gorged with serum. Large patches of emphysema existed at the summit of each lung; some cells were of the size of a horse-bean, with tough inelastic walls. The heart was dilated to three times the size of the fist, its parietes being of the usual thickness. The right ventricle and auricle were particularly large, and the tricuspid foramen measured six inches in circumference. Its valves were small and thin, and could not close the foramen. The other organs were in a healthy state, except that the lining membrane of the stomach was of an uniform bright scarlet colour, and rather mammelated at its great curvature.

REMARKS ON CASE IX.—There is no cause of dilatation of the heart to be detected in Case IX. arising out of any affection of the organ itself. The emphysematous state of the lungs was, however, probably of long standing. This must have greatly affected the passage of blood through them, and therefore it would have a tendency to stagnate and accumulate in the right side of the heart, and thus promote its dilatation. Somewhat similar to this was the case of a lady whom I attended with Mr. Wickenden. The dilatation and attenuation of the heart was evidenced by its sounds, sharp and clear, being distinctly heard over every portion of the chest. This had been perceived, more or less, ever since a protracted attack of bronchitis. She was much relieved by the administration of expectorant and tonic medicines.

CASE X.—Eliza Babington, æt. 14, had articular rheumatism, with some pain under the left breast, two years and a half ago. Six months after that she began to perceive palpitations, which have occasionally reappeared ever since, with depression, nausea, and great dyspnœa on exertion.

When visited she complained of constant nausea, long and violent attacks of palpitation, occasional headaches, and a dry cough. The pulse was feeble and small at times. The left side of the chest sounded dull from the second rib downwards; and over this space the respiratory sound was inaudible. A thrill or pulsation was felt between the fifth and sixth ribs, alternating with a similar one between the second and third ribs. A grating sound was heard with the first sound of the heart.

Six months after this, turgescence and strong pulsations of the jugular veins were seen. The feet, legs, and abdomen, became gradually and successively infiltrated. Urine became scanty; not coagulable by heat. A double grating sound was heard, which continued till her death, which took place nine months after she was first seen.

Autopsy.—There was much fluid in the thorax, pericardium, and abdomen. On either fold of the pericardium were several rough patches, feeling like a calf's tongue. The heart was hypertrophied, and dilated to more than double the size of the fist. The chordæ tendiniæ of one of the tricuspid valves were shortened and thickened. The tricuspid foramen measured nearly five and a half inches in circumference. The valves could not close it, a large space being left uncovered in the middle, not only from the inability of one of the valves to rise up to the plane of closure, but from the disproportion existing between the size of the valves and that of the foramen. The lining membrane of the left auricle was thickened and opaque, and near the mitral valves was granulated. These valves were thickened, and adhered to each other at their base, so that two fingers could not pass between them. The other organs were healthy.

CASE XI.—James Bull, æt. 13, had a severe attack of rheumatic fever when five years old. Four weeks since felt palpitations, and three days back first felt pain under his left breast. When

first seen by me he could not lie down, and dyspnœa was distressing. The pulse was small and vibratory; there were strong palpitations under the xyphoid cartilage. From the second down to the seventh rib of the left side the chest was bombed out an inch beyond the level of the other side. This space gave out a dull sound on percussion, and over it existed purring thrill. A loud coarse bellows sound was heard all over the chest.

In a month's time the projection of the left side had much decreased under the employment of iodine and mercurial frictions. A pulsation and thrill was then felt between the fifth and sixth rib, alternating in time with another between the second and third rib. In another month the veins of the neck became very turgid, the ankles œdematous, then the legs, and ascites followed. The urine was not coagulable by heat. In four months from my first visit he died, extensively anasarcaous; the whole of the left side of the chest sounding dull, and the respiratory sound being only heard on that side up the spine.

Autopsy.—The heart was enormous, extending up to the second rib, and upwards of two inches to the right of the sternum. The lining membrane of the left auricle was thickened and opaque, and was covered with patches of horn-like substance. This appearance was continued into the mitral valves, which were much thickened, and would not allow the passage of two fingers. The base of the aortic valves was thickened. The right ventricle and auricle were immensely dilated and attenuated. The tricuspid foramen measured five inches and a half in circumference. One of the tricuspid valves had its chordæ tendiniæ much thickened and shortened, so that it could not rise into the plane of closure; consequently the valve was very incomplete. Other organs healthy.

REMARKS ON CASES X. AND XI.—The causes of dilatation in Cases X. and XI. may have been double, because, although the obstruction at the mitral valves may have caused stagnation of the blood in the cavities of the heart behind it, and thereby have given rise to dilatation, yet it must be borne in mind that the original attack of endocarditis, in which the alteration of the mitral valves originated, may, at the

same time, as in other cases, have predisposed the organ to hypertrophy and dilatation.

It is instructive to remark that, although the dilatation of the left auricle had in each case existed for a long time, as evidenced by the dulness on percussion, and the pulsations alternating with those of the ventricle, which were perceived much higher in the chest than usual—in a position, indeed, where the dilated auricles were found after death—yet that dropsy did not supervene until there were evidences of incomplete closure of the tricuspid foramen. This additional obstacle to the venous circulation was necessary to produce serous infiltration.

CASE XII.—William Cashmore, æt. 61, iron-caster: had oppression of the chest and indigestion for twelve months. He complained of troublesome cough and distressing dyspnoea. The pulse was very small and thready. An obscure thrill was felt to the right of the upper part of the sternum. A single prolonged rasp sound was heard, its maximum being over the same spot where the thrill was felt.

Four months after this the pulse in each wrist was reduced to a mere flutter: nine months after this the intensity of the rasp sound was much diminished. The whole of the right side of the chest sounded dull, and the respiratory murmur of that side could only be heard up the spine. He could not lie on the left side. There was extensive dulness around the heart. In six months the veins of the neck became almost varicose; the ankles became œdematous, and in another month general anasarca appeared. The urine was not coagulable by heat. A little before his death, which took place nearly two years after my first visit, a slight fluctuation could be occasionally detected in the jugular veins.

Autopsy.—The right side of the chest contained a gallon, the left a pint of fluid; there was a large quantity in the abdomen. The size of the heart was truly astonishing; it projected for some distance under the sternum into the right side of the chest. All the cavities were uncommonly dilated, and the walls of the right ventricle were attenuated. There were only two aortic valves, which were converted into bone, and stretched tightly across the orifice of the vessel. The free edge of the

smaller valve was cartilaginous, so that it could slightly recede and allow a slit for the passage of the blood from the heart. These valves had long bony bases extending some distance into the ventricle. The tricuspid foramen measured six inches in circumference; its valves were small, and totally incompetent to close the foramen. There was atheroma in all the great arteries. The other organs were healthy.

CASE XIII.—Henry Wilson, æt. 66, gun-stocker: felt palpitations of the heart when in bed a year ago, which occasionally reappeared for six or seven months, and then ceased; great dyspnoea then came on. Had lost flesh for three weeks previous to my seeing him; and for three days had pain below the left breast. The veins of the neck pulsated most violently, and felt as firm and hard as arteries. The pulse was converted into a tremulous vibration. There was cough, with expectoration of much clear mucus, sometimes streaked with blood. The chest sounded dull about three inches around the heart. One coarse, hollow, and amazingly prolonged sound, was heard synchronous with the ventricular systole, and its maximum was to the right of the sternum. Cooing râles were heard over the chest. There was œdema of the ankles, and slight œdema of the legs. Five weeks after this he was carried off by an attack of acute bronchitis.

Autopsy.—The veins of the neck were very much distended with dark blood. The pericardium was opaque and thickened, and had on its cardiac portion a patch of soft yellow lymph as large as a crown piece. The heart was nearly three times the size of the fist, and very firm, from general hypertrophy and dilatation. The aortic valves were unfolded, grown together, and turned into bone, so that the passage for the blood was contracted into a small triangular opening which could barely admit a quill.* The aorta was studded with osseous scales. The arch was slightly dilated, and its middle coat easily torn. One of the tricuspid valves adhered to the right ventricle. The tricuspid foramen measured five inches in circumference. Other organs healthy.

REMARKS ON Cases XII. and XIII.—The extreme narrowing of the orifice of

* In the museum of King's College, London.

the aorta in Cases XII. and XIII. must have greatly tended to produce dilatation of the cavities of the heart, and it might be supposed hypertrophy of the left ventricle. While both hearts, however, were dilated, the walls of the left ventricle were hypertrophied in Case XIII. and attenuated in Case XII. Their history would lead us to conclude that the dilatation in Case XII. had been purely passive from obstructed circulation, while that in Case XIII. had been partly induced by pericarditis; traces of both ancient and recent attacks having been found on the pericardium.

From the nature of the morbid alterations of the aortic valves they must have been a long time in progress, and yet the health was not seriously affected until a comparatively short time before death. This is, in fact, explained by the prolonged sound heard during the systole of the ventricles, which proved that their contraction was continued much beyond the usual time, and that by this means the blood was gradually squeezed through the contracted orifice of the aorta, and was thus prevented from stagnating in the heart. But the circulation could not be forced on in this manner without injurious effects, and consequently in course of time dilatation of the heart situated behind the obstructed orifice of the aorta ensued, which was followed by congestion of the lungs and dyspnoea. The latter symptoms were doubtless much relieved by a copious secretion of mucus from the lining membrane of the bronchial tubes. Dilatation at length commenced on the right side of the heart, and the tricuspid foramen having been dilated to such a size that its valves could no longer close it, regurgitation, evidenced by jugular pulsations, ensued, and anasarca and death closed the scene. On this supposition the dilatation of the right side of the heart in Case XIII. must have been very rapid, as anasarca only appeared a little before death. It was probably hastened by the attack of bronchitis. A comparison of these cases offers a remarkable confirmation of the connection which exists between regurgitation and venous pulsations. In Case XIII. the heart was firm and hypertrophied, consequently the shock or pulsation was most vehement, but

the dilatation of the tricuspid foramen being carried only to a moderate degree, the obstruction to the circulation was slight, and the dropsy inconsiderable.

In Case XII. the venous pulsations were slight, because the heart was attenuated, but the obstruction to the circulation was great, and the dropsy considerable, because a vast quantity of blood must have regurgitated through a widely dilated tricuspid foramen.

CASE OF PROCIDENTIA UTERI,

CURED BY THE ACTUAL CAUTERY.

By J. ADAIR LAWRIE, M.D.
Surgeon, Glasgow Royal Infirmary.

(*For the Medical Gazette.*)

CHRISTIAN M'LEAN, æt. 18, servant; Scotch; unmarried; dark complexion; pale; habit firm and plump; admitted June 1, 1840.

Twenty-eight months ago, while carrying a heavy tub, part of the weight resting on the abdomen, felt something give way in the pelvis, and as if the vagina immediately afterwards were unusually distended. Some days after, while menstruating for the first time, a tumor projected through the orifice of the vagina, and has since rapidly increased. At present the uterus, dragging with it the posterior part of the bladder, and a large portion of the vagina, project externally to the distance of three inches, forming a tumor the size of the fist. At first it disappeared spontaneously in the recumbent position, but now it remains constantly prolapsed. It can be easily returned by pressure. The os externum is very unusually enlarged and relaxed; urine and fæces are passed with difficulty. A catheter can with ease be passed into the bladder, and its point made to project nearly as low as the apex of the tumor on its anterior aspect. Catamenia nearly regular; surface of abdomen and mammae as in the virgin state. Denies coition, either before or since the procidentia. Has derived no benefit from the free application of the nitrate of silver, astringent injections, pessaries, bandages, and the ordinary routine treatment.

On the 14th June, and again on the 2d August, strips of the mucous membrane were dissected off from the lateral and posterior parts of the vagina, without benefit.

On the 2d of October, the uterus being returned, Weiss's three-pronged speculum was introduced and dilated, and the actual cautery freely applied to both lateral surfaces of the vagina, nearly as high as the uterus. The operation gave considerable pain, and was followed by œdema of the external parts, retention of urine, some pain of abdomen, and hysterical symptoms, which readily yielded to mild treatment. She was kept in the recumbent posture till the 13th of November, and in hospital till the 8th of December. On the latter date the following report was entered in the journal. "Not the slightest tendency to renewal of the procidentia; she has no feeling of prolapsus. Uterus felt nearly in its natural position, with a circular contraction of the vagina a little below os uteri. The contraction firm, but quite elastic."

July 18, 1841.—She returned to the hospital, and the following report was taken. "Continued to do well till ten days ago, when, after having been employed for two days in the standing posture, used unwonted exertion, and afterwards taken a long walk, the prolapsus returned, and is gradually increasing; but the external tumor is not yet nearly as large as when she was first admitted."

July 25th.—The actual cautery was applied in the same manner as formerly, and was followed by the same train of symptoms, which readily subsided under one bleeding, aperients, fomentations, &c. She was confined to the recumbent posture till the middle of April, when she was allowed to walk, with a T bandage, and a compress on the perinæum. On the 25th of that month she was dismissed cured. I have heard of her within these few days. She remains free from prolapsus, or any tendency towards it.

REMARKS.—I am not aware that the actual cautery is in common use, in this country, as a cure for procidentia. The above case appears to me worthy of being recorded, as showing that it will succeed in extreme cases, after the failure of the more common operation with the knife. In future I should

prefer the cautery, as being rapid and easy of application, and so far as two applications can prove, not followed by any bad consequences, and successful.

CASES OF CYNANCHE TONSILLARIS.

To the Editor of the Medical Gazette.

SIR,

THE preceding volume of the GAZETTE contains a report of several cases of cynanche tonsillaris successfully treated by Mr. Bell, of Barrhead, with gum guaiacum; and it appears that the practice was first introduced by Dr. Hannay, of Glasgow. Two cases having lately occurred to me, I resolved to give the remedy a trial, and in both it realised my expectations.

CASE I.—W. Fenemore, aged thirty years, tall, and of a ruddy complexion, caught cold from being wet, felt a soreness of the throat and difficulty of swallowing, for which he applied a blister to the throat. I saw him on the third day: the right tonsil was enlarged, looked red, and very painful; face flushed; tongue covered with a white fur, &c.

I prescribed a mixture with Pulv. Guaiaci, in doses of half a drachm, which speedily relieved him.

CASE II.—Alfred Sewell, aged 22, of a strumous appearance; his occupation is that of a lath-cutter; had been confined to the house two days when I saw him. Both tonsils were enlarged and painful, rendering deglutition difficult: considerable fever; pulse quick; skin hot and dry; face flushed; tongue covered with white fur. Calomel and a purgative draught first prescribed, after which he was directed to take the guaiacum mixture as above. Hot bran poultices were ordered to be applied frequently to the throat. In a few days he was quite well, and able to work: the swelling of the tonsils rapidly subsided.

If you think the above cases possess sufficient interest, you will oblige by giving them a corner in your useful publication.—I am, sir,

Your obedient servant,

J. COLLIER, M.R.C.S.

Brackley, July 28, 1841.

ON SYPHILITIC IRITIS.

BY WILLIAM W. COOPER.

[For the *London Medical Gazette*.]

OF all the diseases to which the human frame is subjected, there are none of greater interest than those which affect the organs of vision; whether we consider the importance of these organs to the comfort and happiness of the individual—the extreme delicacy of their construction, which renders them peculiarly liable to derangement—the rapidity with which morbid changes are induced—and the painful consequences of diminished reputation and vexation of mind which would accrue to the surgeon, who, from ignorance or from error in diagnosis, permits the favourable moment to pass by, and thus may entail all the miseries of blindness upon the unfortunate sufferer.

There is scarcely a day elapses without patients presenting themselves at Moorfields, and other large ophthalmic institutions, whose sight has been irrecoverably lost from the effects of purulent ophthalmia: most of these are infants: and the number of instances is at once lamentable and surprising, in which we are informed that the practitioner who had either brought the child into the world, or who had been consulted on the first appearance of discharge from the eyes, had pronounced it to be “mere nothing—not worth notice.” And thus the favourable moment for subduing this most formidable disorder has been permitted to elapse, and its ravages to extend unchecked or controlled, until disorganization has taken place, and the organ is irrecoverably lost!

My purpose, therefore, is to bring forward, from time to time, cases which may come under my observation, and which are possessing of interest.

I do not pretend that any observations I shall offer may be attractive for their novelty; for I have no peculiar theory to adduce or to support. My object will be simply to bring forward cases, whether successful or otherwise, which may, either in their symptoms or their treatment, possess more than usual interest or instructiveness. I shall not hesitate to relate *unsuccessful* cases, for they are, in my opinion, especially valuable, as affording land-

marks for others to profit by; and it would be presumption in any man to assume that his judgment is infallible, or that disappointment did not at times attend his best exertions.

The cases which I have selected for the subject of the present communication are those of syphilitic iritis.

CASE I.—Jane Wilkinson, æt. 30, applied at the Royal Ophthalmic Hospital, Moorfields, March 29th, 1841. She stated that her right eye had become painful about a week previously, and that four days before the period of her application she had entirely lost the sight of it. There was much pain both in the globe and around the orbit, which was aggravated at night.

Appearances.—Congestion of the vessels of the sclerotic, and the zone around the margin of the iris exceedingly well marked; the iris altered in colour, and quite dull; pupil irregular, and the aqueous membrane hazy; there was congestion of the conjunctival membrane; not much intolerance of light; pulse 98, and feeble; tongue foul.

Had suffered from syphilis nine months previously, and the throat was sore at the time of examination.

R. Hydrarg. c. Conio, gr. v. ter die.
Liquor. Sarzæ (Battlei) gt. xx. ter die.
Extract. Belladonnæ supercil. applic.
Meat diet, but no stimulants.

April 1st.—The mouth not affected; no improvement in the symptoms, and she complains of excessive nocturnal pain.

To continue the medicines, with the addition of Mercurial Ointment with Opium (Opii 3j. to ʒvij.) to be applied to the temples.

5th.—Mouth now sore, and all the symptoms alleviated; inflammation greatly diminished, and the pain also. She was directed to continue the treatment. On the 15th the inflammatory action had entirely subsided, but she complained of general weakness. To go on with the Liq. Sarzæ simply. She was dismissed well on the first of May.

CASE II.—R. Thomas, æt. 36, applied April 1st. The eyes have been affected three weeks; in both iritis is well marked, but in the right particularly so; the iris is dull and discoloured; the pupil irregular; inflammatory zone very distinct; the sclerotic and conjunctival tunics both inflamed; and the aqueous membrane is hazy. The iris of the left eye is dull and discoloured, and the zone visible; the other symptoms are but slight: pain considerable, especially at night, around both orbits and in the globe of the right eye: tongue foul; pulse moderate. He denies any venereal taint, but a suspicious eruption exists upon his face. Ordered

Blue Pill, gr. v. night and morning ; a blister behind the right ear, and six leeches to the right inferior palpebra ; meat diet, but no stimuli.

April 5th.—The mouth has become very sore, and a surprising change for the better has taken place already ; indeed the inflammatory symptoms have nearly disappeared. He states that the eyes began to improve as soon as the mouth became affected.

To repeat Blister and Blue Pill.

13th.—He is now convalescent ; the iris and aqueous membrane have recovered their natural brilliancy and appearance ; and a slight irregularity of the right pupil, from synechia posterior, is the only indication of disease having existed.

CASE III.—J. Walden, æt. 21, applied April 13th. He states that he suffered from syphilis seven months ago, but allowed it to take its chance without having medical assistance. At present, independent of the affection of the eye, he suffers greatly from pains in the bones, especially the tibiæ. His throat is ulcerated, and a syphilitic eruption is visible upon the face and other parts of the body. He complains of great prostration of strength. The pulse weak and frequent ; tongue white and dry ; the left eye has iritis strongly marked : it has been affected three weeks : the sclerotic is intensely inflamed ; the inflammation being most severe around the margin of the iris. The conjunctival vessels are also considerably injected, their tortuous course and brilliant scarlet colour contrasting with the pink hue and straight direction of the sclerotic vessels beneath ; the iris is very dull, and much altered in colour ; there is a deposit of fibrin of a dull red colour around the margin of the pupil ; aqueous membrane very turbid ; there is very great pain in the globe and eyebrow, extending to the temple and inner side of the nose, greatly increased at night, and the globe is very tender. Ordered

Calomel grs. ii., and a third of a grain of Opium, every six hours ; Belladonna to be freely applied to the eyebrow, and the Unguent. Hydrarg. c. Opii. to be rubbed into the left temple. Meat diet.

16th.—Mouth not yet affected ; the inflammation rather increased, the aqueous membrane being more turbid, and the deposit of fibrin, together with the irregularity of the pupil, decidedly greater ; pain at night very severe.

To go on with the medicines.

20th.—The mouth has become exceedingly sore, and a manifest improvement in the symptoms has taken place ; the cornea is now nearly clear ; inflammation much less,

and deposit of fibrin diminished ; pain also more moderate, and he gets some rest at night ; the pupil, however, is very angular and irregular.

To repeat the Calomel and Opium every night. Continue the Belladonna and Ointment to the temple.

May 1st.—The appearances of inflammation have entirely subsided ; the iris has resumed its normal aspect, and the pupil has nearly recovered its circular form.

To continue the Belladonna alone.

The patient did not return after this date, so that we may conclude he has quite recovered.

CASE IV.—Anna Bartlett, æt. 30, applied June 15th. Iritis has existed in the left eye for the last three weeks, and at present there is very little vision : the iris has entirely lost its brilliancy, and has been changed from a clear blue colour to a reddish grey : on the upper and outer portion a distinct patch of congested vessels is visible, and the margin of the pupil exhibits a rusty-coloured deposit of lymph : the capsule of the aqueous membrane is also affected, and very turbid towards its inferior part : the red inflammatory zone is well defined, as is also the grey circle around the margin of the cornea, although there could not be a doubt, from the result of inquiry, of the syphilitic origin of the affection : there is excessive pain in the eye, the temple, and brow, which is much worse at night : pulse weak and small ; tongue tolerably clean ; and bowels regular.

She was ordered to take two grains of calomel, and half a grain of opium, night and morning. To rub the blue ointment with opium upon the temple, and to apply belladonna to the brow night and morning : meat diet.

18th.—The mercury has not as yet produced any effect upon the mouth ; and although the disease has not made progress, yet it has certainly not decreased. She complains of great weakness and exhaustion, and is directed to take half a pint of ale daily, with the meat diet.

To continue the calomel and opium three times a day.

22d.—Here, as in the other cases, a surprising alteration for the better has taken place upon the system coming under the influence of mercury. The mouth is now very sore, and all the symptoms are alleviated. The red zone has nearly vanished. The iris has recovered its brilliancy and natural colour in a great degree. The pupil is much more regular, and the deposit of fibrin less. The aqueous membrane is also nearly free from inflammation.

To repeat the Cal. and Opium twice a week, and to continue the Belladonna.

29th.—The iris has quite resumed its natural aspect, the pupil its regularity, and the vascular zone has so completely disappeared, that it would be almost difficult to believe that the eye had so recently been affected with such a severe inflammation. Discharged.

CASE V.—Henry Mills, æt. 21, a seaman, applied June 18th. His forehead and face were covered with a copper-coloured eruption, he having contracted syphilis six months previously, for which he had been severely salivated. The effects of the salivation continued until very recently, and have left him in a state of great debility. The left eye had first become uncomfortable six weeks previous to his application, and had been getting worse ever since. The iris has quite lost its brilliancy, and the colour is greatly changed; the aqueous membrane is hazy, and the inflammatory iritic zone very well marked; the grey corneal zone is also very clearly defined, as in the last case; pupil exceedingly irregular, and the margin coated with reddish lymph. There is a small quantity of pus in the anterior chamber. Sclerotic much injected, and the pain in the globe and around the orbit very acute, especially at night. Ordered one-sixteenth of a grain of oxymuriate of mercury in three ounce decoction of sarsaparilla three times a day; two leeches to the lower palpebra; a blister behind the left ear, and belladonna to be applied to the eyebrow night and morning: meat diet, without stimuli.

22d.—The disease has not been controlled by the treatment, all the symptoms being aggravated. There is more pus in the anterior chamber; the vascular zone is greater, and the nocturnal pains more severe: pupil very irregular, and the deposit of lymph increased. Vision much impaired. Tongue foul, and the bowels confined. He was now ordered two grains of calomel, with a third of a grain of opium, three times a day, with the blue ointment and opium to the temple, and belladonna to the eyebrow night and morning.

25th.—The mouth has become rather sore, and an improvement is visible in the eye; the hypopyon is much less, and he is more free from pain; but the iris is exceedingly dull, and the pupil very irregular: the inflammation of the sclerotic diminished. Ordered to go on with the same treatment.

29th.—Much better; vascular zone has nearly disappeared; the pupil and iris are much clearer, although the irregularity of the pupil is little altered: neither hypopyon nor aquo-capsulitis remains.

July 2d.—With the exception of the irregularity of the pupil, all the symptoms have nearly disappeared; the inflammation of the sclerotic is gone, and the brilliancy of the iris has returned.

The Cal. and Opium twice a week, and half a pint of ale daily.

I have not seen him since.

These cases afford good illustrations of the beneficial effects of mercury in controlling and subduing iritis. It will be observed that it was not until its effects upon the system became manifest that benefit was derived; until then, indeed, the symptoms generally rather increased than diminished. As soon, however, as the mouth bore evidence of the constitution being under its influence, the symptoms in each case gave way, and usually with a rapidity proportioned to the extent to which the system was affected.

The exacerbation of pain at night was a very characteristic symptom throughout, and seemed to be proportioned to the degree to which the sclerotic was involved. The unguentum hydrarg. cum opio afforded great relief.

The irregularity of the pupil, which was in some cases very considerable, gradually gave way under the mercurial treatment, and the continued use of belladonna.

The white or ash-coloured zone around the margin of the cornea, which some authors have described as peculiar to arthritic iritis, was beautifully shown in two of these cases; but the syphilitic origin of the disease could be most satisfactorily traced in each: indeed, I believe that this symptom cannot by any means be considered as diagnostic of the disorder alluded to.

The bodily powers of all the patients were feeble and depressed, and accordingly the diet ordered was nutritious without being stimulating. The satisfactory progress of the cases would fairly lead us to infer that a depleting treatment, as still further tending to lower the *vis vitæ*, would have been injurious; indeed, in Bartlett's case, half a pint of ale daily was ordered with much advantage, although the local symptoms were severe.

I believe that I have touched upon the main points of interest relating to this subject, and will not further extend this communication. My next will have for its object—Purulent Ophthalmia as it appears in the Infant.

6, Park Cottages, East Village,
Regent's Park, July 1841.

MEDICAL GAZETTE.

Friday, August 6, 1841.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recusó.”

CICERO.

ANIMAL MAGNETISM.

WE have attended one of the exhibitions by M. Lafontaine, at the Hanover Square Rooms, and should have given reports of his proceedings, had not that task been already performed by those for whose pages things having but little connection with science are better suited — we mean, the newspapers. Through their medium the magnetism of M. Lafontaine has already become a matter of notoriety; and in the *Times*, the accounts, though written with more respect for the marvellous than one accustomed to observe in medicine would have shown, are so correct, that we can refer our readers to them for the apparent facts of the proceedings. We shall only say that the impression conveyed to our minds was, that M. Lafontaine is but a poor magnetizer, or rather that his patients are of an inferior class. The phenomena which they exhibit are of the most ordinary kind: they are merely thrown either into a state like that of a natural somnambulist, or into a deep cataleptic sleep, in which they are insensible to such impressions as the pricking of pins, or a slight galvanic shock, and in which some of their limbs are rigidly fixed. But there is neither *clairvoyance* nor *prevoyance*, nor the development of any extraordinary power of the muscles, or of the imagination, or of the invention. The phenomena are far less striking than those produced in the patients of either Dr. Elliotson or the Baron Dupotet, and of course far less marvellous than those to which we recently referred as forming the staple of

the modern magnetizers in France. There is nothing, therefore, in these exhibitions, from which any member of our profession would derive either instruction or surprise; he would find in them no phenomena with whose existence he is not familiar, nor any that would give him a deeper insight into the nature of the influence called magnetic, than he probably already possesses.

With the public, however, the case is different; to them the phenomena are striking, and being novel, are of course thought to be the results of some marvellous and newly-discovered influence. Nor would it be easy, till more stirring events than are now occurring engage their attention, to convince them of the contrary, or to prevent their being, for a time, cheated out of their common sense. Till some more diverting amusement is invented for the loungers about town (of whom the great majority of M. Lafontaine's spectators are composed), till Parliament meets, or till some fearful murder is committed, or some new genius in music or in acting appears, animal magnetism will, in all probability, be the leading topic of the day; and will be discussed with all the curiosity and all the credulity for which unemployed Englishmen have ever been remarkable.

To make that part of the public by whom such exhibitions are frequented judge rightly in a matter of science, or to make them believe any thing to be less marvellous than it seems, when their discernment is flattered if they disbelieve the truth, is what no reasonable man would undertake. If it were desirable, it would not be difficult to show the fallacy which lies at the foundation of a belief in animal magnetism—that is, in the possession by any person of a power, peculiar and else unknown, by which he can produce in others conditions in which

their senses are displaced, or any of their faculties marvellously exalted. We believe that every rational man, accustomed to observe diseases, finds in the real facts of magnetism no more than the results of diseased conditions, whose natural occurrence is well known, though rare, and which are produced in those who are unfortunately subject enough to such influences, by various impressions on the mind.

The question so incessantly put, "Is there any thing in animal magnetism?" cannot be answered so indefinitely as it is asked. If it be meant to ask whether, in the exercise of animal magnetism, there be employed any force different from those which are in operation every day, or deserving to be called by any other name than the force of imagination, or of imitation, or of fear, or of some commonly employed impression on the mind, no one need hesitate to answer, No. There are in the facts of animal magnetism no other things than were known before it was invented; no others than have been often seen, both before and since, in diseases over which no pretended magnetic influence had ever been exercised; no others than exist in the ordinary phenomena of catalepsy and somnambulism, as they occur in those diseased from natural causes.

On this too much stress cannot be laid,—the *facts* of animal magnetism are not new, nor peculiar to it; and the novel effects to which it pretends to give rise, the seeing without eyes, the discernment of internal diseases in the patients themselves or in others, the power of prophesying deaths and other events, the peculiar discernment of metals, the magnetizing of inanimate objects, the cure of organic diseases—all these, and all like them, are fallacies. For those who believe or profess them, there is no alternative but to regard them as either gross deceivers

or themselves deceived; of folly or dishonesty, each in its highest degree, they, by the very fact of their profession, convict themselves; for the effects, in which they declare their belief, have repeatedly, and in spite of them, failed of being produced before competent witnesses. To adduce but one example—the great majority of magnetizers believe that patients can see by other means than eye-sight; yet though M. Burdin has, for the last two years, offered a handsome premium to any one who will bring him a somnambulist that can read after her eyes are blindfolded, to his and others' satisfaction, none of the many who have tried to gain the reward have ever succeeded. In every case the failure has been signal, and has manifestly been due, not to an accidental defect of magnetic power, or any such thing, but to the difficulty of accomplishing the trick of displacing the bandage from over the eyes, when all the movements necessary for that purpose were accurately watched. These facts admit of no dispute; and if, after knowing them, any person can still put faith in the assertions of these extraordinary results being produced by magnetic influence, he must surely be insane in the love of error.

But if the same question, "Is there any thing in animal magnetism?" only means, "Do the persons professing to magnetize possess any power of throwing others into states of sleep, or catalepsy, or somnambulism?" it would be unwise to answer it with an unconditional negative. There are undoubtedly some unfortunate persons, weak and excitable girls especially, in whom these conditions may be produced by any thing that strongly affects their minds, and more particularly by any thing which they have known to produce the same effects in others or in themselves before. Such a patient

once affected by what she has deemed an extraordinary or even a supernatural influence, is afterwards completely in the power of her magnetizer; each time that she is acted on, the mind becomes weaker and less capable of resistance, if there be any desire to make it; till at last the phenomena of the fit may even surpass, though they will not differ in kind from, those known in the strangest natural catalepsy or somnambulism. Yet in all this there is no new power employed or engendered: in all ages effects similar in kind have been produced confessedly by mental excitement, and none know better than the magnetizers themselves that their manipulations are only essential, because the patients believe them to be so, and regard them as the sources of the strange influence.

The influence of impressions on the mind, as the sole and sufficient means by which are produced all the magnetic effects that are true, is abundantly proved. In the times which the magnetizers speak of as the earliest of the practice of their art, these were the only agents employed, and they produced effects which modern manipulations have not surpassed: the history of fanaticism has facts far more marvellous than M. Lafontaine's. By the first commission of the Academy of Sciences in Paris also, the energetic influence of the imagination was proved beyond a doubt: patients exhibited the same effects whether they were magnetized or not, provided they believed they were so. In subsequent years, and especially by M. Bertrand, himself for a long time a believer in magnetism, the same influence has been repeatedly shewn; and if there be any change in the character of modern magnetism, it is only that now there is much more intentional deception. The late proceedings in France have sufficiently proved the readiness of magnetizers there to resort to any attempt at a juggle, when their ex-

periments are in danger of failing; and although we fully admit the possibility of phenomena, such as insensibility, somnambulism, and so on, being produced at will in certain persons, we should not, without close inquiry and examination, believe in the genuineness of even these ordinary phenomena, in persons influenced by any professor of the art, who came without good credentials of his respectability.

It is a very bad sign of the character of any thing that pretends to be a science, when its professors call upon the public to be judges of it;—to a man with no more caution than is essential to one looking after truth, such a proceeding is at once sufficient to brand it with a strong suspicion of imposture. And this suspicion is increased almost to certainty when any professed science has been repeatedly before the public, and repeatedly examined and discussed by competent judges, and has yet made no permanent progress either in the certainty of its evidence or in the number and importance of its supporters. This want of progress has been peculiarly the lot of animal magnetism; nor would a person of ordinary discernment in such matters need more than the knowledge of its history to convince him that there must be some great fallacy in it. It has fewer disciples now than in the time of Mesmer; and among them there are fewer than ever of men who can lay any pretension to the possession of sound judgment: the facts in its favour are not more numerous nor more cogent: it has led to no general rules, no useful applications; it has been found to exercise no more general influence, and to have no more connection with any other science, than it was supposed to have at its first invention. In a word, as a science, it has not made in the last sixty years one step.

Now it is ridiculous to refer this to the incredulity of the public, or to the

bigotry of men of science, or the prejudices of the interested. In the same period during which animal magnetism has made no progress, nearly every true science has had its field enlarged more than in any preceding period of double the extent, and those only have not made such progress which before were nearly perfected. In this same period, chemistry and electricity, and magnetism proper, have risen from being hardly more than collections of strange facts to the rank of certain sciences; and why, if there be any truth in it, has animal magnetism alone stood still? As for the prejudices of the public, has not the same period of years just passed seen more customs willingly abolished, more improvements, more reforms, more applications of science to the every-day affairs of life, than ten times the number of years in any preceding age? For the willingness of medical men to admit improvements during the same period, need we do more than point to the establishment of vaccination after a few years' partial opposition; to the use of the stethoscope, now so general; to a thorough and repeated reformation of nearly every part of physiology. There can be no doubt that the predominant character of the last half century has been the love of novelty, the avidity for discovery; no period could be imagined better for the promulgation of a new science; for nothing but inherent error, and that of the most obvious kind, can in these times long hinder the acceptance of a new doctrine.

It is as absurd, therefore, as it is impudent, for magnetizers to rank themselves with the persecuted, and to place themselves in the same list with Harvey, and Galileo, and Jenner: the opposition that these met with was speedily beaten down, and served only to demonstrate the force of their truth. If magnetizers

dare to assimilate themselves to these great men in being opposed, why have they not long since, with every advantage of modern times and circumstances, proved themselves yet more like them in triumphing? For this simple reason—that in animal magnetism there is no discovery to be made, no fact of which the like has not been long known, no force which has not been long exerted. It is now earnestly to be hoped that its practice having passed into the hands of those who make money by the public exhibition of its unfortunate victims, it will soon cease even to pretend to be a science. It may then be turned to the only use of which it is capable; its facts, if they can be winnowed from its falsehoods, will afford an interesting volume of evidence on the effects of impression on the mind.

LIQUOR HYDRIODATIS ARSENICI ET HYDRARGYRI.

THE efficacy of the *Liquor Hydriodatis Arsenici et Hydrargyri* in the treatment of psoriasis, lepra, and lupus, has lately been brought under the notice of the profession, by Mr. Donovan, in the Dublin Journal of Medical Science. The process recommended by Mr. Donovan for the preparation of this compound, and the directions for its use, are as follows:—“Triturate 6·08 grains of finely levigated metallic arsenic, 15·38 grains of mercury, and 50 grains of iodine, with one drachm measure of alcohol, until the mass has become dry, and, from being deep brown, has become pale red. Pour on eight ounces of distilled water; and after trituration for a few moments, transfer the whole to a flask; add half a drachm of hydriodic acid, prepared by the acidification of two grains of iodine, and boil for a few moments. When the solution is cold, if there be any deficiency of the original eight ounces, make it up exactly to that measure with distilled water; finally, filter. The theory of this process need scarcely be adverted to. By the long-continued trituration of arsenic, mercury, iodine, and alcohol, the metals are converted into iodides, which combine. The mass, by solution in water, is converted into an hydriodate of arsenic and mercury. The quantities of the two metals are so adjusted, that when converted into protoxides by decomposition of a portion of the water in

which they are dissolved, there will be eight grains of protoxide of arsenic, and sixteen of protoxide of mercury. The quantity of water is such that each drachm measure of the solution will contain exactly one-eighth of a grain of protoxide of mercury. I conceive that the quantity of mercury ought to be double that of the arsenic, in order to ensure a slow and moderate, yet adequate mercurial action, along with the proper effect of the arsenic. Of this *liquor hydriodatis arsenici et hydrargyri*, each drachm measure consists of—water, one drachm; protoxide of arsenic, one-eighth of a grain; protoxide of mercury, one-fourth of a grain; and iodine (converted into hydriodic acid) four-fifths of a grain. The colour of the solution is yellow, with a pale tinge of green; its taste is slightly styptic. It cannot be properly conjoined with tincture of opium, or with sulphate, muriate, or acetate of morphia; for all these produce immediate and copious precipitates in it. Hence, if opiates are to be used during the exhibition of this arsenico-mercurial compound, they must be taken at different periods of the day. Tincture of ginger produces no bad effect. The following formula is proper:—

Liquoris Hydriodatis Arsenici et Hydrargyri, ℥ij.; Aquæ Distillatæ, ℥iiss.; Syrupi Zingiberis, ℥ss. Misce. Divide in haustus quatuor. Sumatur unus mane nocteque.

Thus, one-sixteenth of a grain of protoxide of mercury would be taken in each dose, along with two-fifths of a grain of iodine, which, being in the state of combined hydriodic acid, will be much diminished in energy of medical effect. This is no doubt the proper dose to begin the exhibition of arsenic with, but it will be soon necessary to increase it. The division into draughts is here necessary: first, to insure accuracy in the dose, so essential in the case of this active medicine; and, next, to prevent injury to the ingredients by the use of a metallic spoon as a measure,—the general way in which, unfortunately, the dose of a medicine is determined." (*Dublin Jour. of Med. Science*, vol. xvi. p. 277. Nov. 1839.) Since the publication of the memoir from which we have now quoted, Mr. Donovan has published the opinions of various professional friends, regarding the importance and efficacy of this new remedy. After giving their communications at length, he makes the following observations:—"In several of the reported cases, it may be observed that slight ptyalism was induced. This is not to be wondered at, when it is recollected that the mercury is exhibited in solution, and that arsenic itself is capable of acting on the mouth. Of the latter fact an instance has

been lately recorded. I hope the profession will favour me with other statements, according as the effects of this medicine become more known. They shall be published from time to time. And as the employment of various processes might lead to dangerous consequences, I entreat medical men to be certain that the formula I have given shall be rigidly followed in its preparation. It would be a pity that the character of the remedy should suffer through any error in its mode of production. The test of its perfection is, that the whole of the iodine, arsenic, and mercury, shall disappear and be dissolved during the process of preparing it. This cannot happen unless the three elements employed had been chemically pure, and unless the trituration had been sufficiently long continued. The colour, in a short time after the liquid is prepared, should be golden yellow, and the sp. gr. 1.02. I have known one instance in which this medicine had been attempted to be produced by substituting common white arsenic of commerce in place of re-sublimed metallic arsenic; it is almost needless to observe that the former substance does not answer the purpose. If any remains after the boiling, part of it will be arsenic, and where so small a quantity is made use of, not a particle can be spared to go to waste."—*Edinb. Jour. of Med. Science*.

TREATMENT OF URINARY FISTULÆ.

By M. RICORD.

At the meeting of the Académie de Médecine on the 20th of April, M. Ricord presented a patient whom he had cured of a urethral fistula, situated at the inferior part of the penis, by means of an opening made into the membranous portion of the urethra, through which he introduced a catheter into the bladder. This catheter he allowed to remain; and when the urine had ceased to flow through the primary fistula (which was not till after two months had elapsed), M. Ricord revived its edges, and then united them with sutures. When the union was completed, the urine again took its normal course, and the opening made in the perineum soon after cicatrized.—*L'Expérience*, Avril 22, 1841.

ON THE DEAF AND DUMB IN THE DUCHY OF MEININGEN.

By DR. JAHN.

In some statistical researches respecting the deaf and dumb in his country, the author has published the relations which exist be-

tween the population of some countries and the number of deaf and dumb among them.

The following is the interesting table relating to this subject :—

Countries.	Year of Census.	Deaf and Dumb.	Population.	Proportion of Deaf and Dumb to Population.
Switzerland	8005	2,200,000	1 : 275
Weimar	1834	334	238,672	1 : 715
Sardinia	1834	4862	3,675,327	1 : 756
Altenburg	1836	103	120,514	1 : 1170
Lower Canada	1831	408	511,917	1 : 1255
Prussia	1834	10239	13,509,927	1 : 1319
Hanover	1833	1111	1,642,807	1 : 1479
Saxony	1836	1094	1,626,000	1 : 1486
Denmark	1834	630	1,223,807	1 : 1943
Brunswick	1835	125	253,232	1 : 2026
United States (New York excepted) .	1830	5223	10,952,094	1 : 2097
Belgium	1835	1900	4,154,922	1 : 2187
Holland	1098	2,400,000	1 : 2204

It appears from this table that the countries with high mountains and low valleys have more deaf and dumb than the low and level countries; and this rule is confirmed when we examine particular provinces. According to some, moisture also contributes much to the development of this disease. This assertion may be true when the moisture is united with other circumstances, such as the elevation of mountains, &c.; but it is not true of level countries, such as Belgium and Holland. According to Schubler, countries in which the waters are saline and selenitic have many deaf and dumb, and gôitres and cretins; and the tendency of these several circumstances in favour of this condition is confirmed, according to Dr. Jahn, by the topography of the duchy of Meiningen.

As to the relation between males and females among the deaf and dumb, it has been determined that it is as 10 to 9 in Meiningen; but from the following table by M. Schmalz, made on a large scale, it is as 4 to 3.

Countries.	Population.	General Proportion of Deaf and Dumb Males to Females.
Saxony .	1 ² / ₃ mill.	100 : 74
Altenburg .	¹ / ₈	100 : 87
Prussia .	13 ¹ / ₂	100 : 76
Hanover .	1 ² / ₃	100 : 76
Holland .	2 ² / ₃	100 : 75
Belgium .	4 ¹ / ₈	100 : 80
Canton of Zurich	¹ / ₅	100 : 91
Denmark .	1 ¹ / ₄	100 : 87
Sardinia .	3 ² / ₃	100 : 69
Rome .	³ / ₂₀	100 : 75

174 were so from birth, and 18 had contracted the disease at a subsequent period. M. Schmalz gives the following table :—

Countries.	Deaf and Dumb.	From Birth.	From a later period.
Saxony . .	151	77	74
Altenburg .	103	82	21
Bavaria . .	135	79	56
Baden . .	533	240	293
Bohemia . .	165	77	88
Hamburgh .	11	5	6
Rhine, Prussia	29	16	13
Belgium . .	1891	1484	407
France . .	238	171	67
Denmark . .	88	54	34
Sardinia . .	48	27	21
England . .	590	498	92
United States	1443	855	588
	5425	3665	1760

M. Jahn has also confirmed from the cases in his own country the facts that the majority of the deaf and dumb occur in poor families; that it is only very rarely that we see children become deaf and dumb after their seventh year; and that this never happens after the tenth.—*Gazette Médicale*, Mai 20, 1841, and *Haeser's Archiv für die Gesämmte Medicin*.

ON THE FREQUENCY OF THE PULSE IN INFANTS.

IN a recent work by M. Jacquemier, it is stated that the minimum of frequency in the pulse of new-born infants is about 97, and the maximum about 156. Haller fixed it at 140, and Soemmering at 130. Every physician is well aware how easily the pulse of infants is rapidly quickened by restlessness,

Of the 192 deaf and dumb in Meiningen,

crying, &c., and that the only accurate way to determine it is to feel it during sleep. In the act of sucking, the breathing is hurried, and the circulation is therefore necessarily quickened at the same time. Valleiz, who seems to have used the greatest precautions to avoid all sources of mistake, states, as the results of his examinations, that in infants from two to twenty days old, the minimum of frequency is about 76, and the maximum about 104. We may therefore consider the medium 87 as the average frequency of the pulse at this period of life. It has been generally believed that the frequency of the pulse diminishes as the age of the infant advances. The very reverse, however, seems to be the case, according to the experience of this author; for he found that, at seven months, the pulse is much more frequent than during the first week after birth, and that from that period it progressively diminishes in rapidity to about the sixth year.—*Clinique des Maladies des nouveaux nés, par F. Valleiz, 1833; and Edinburgh Monthly Journal of Medical Science.*

SENECIO JACOBÆA.

To the Editor of the Medical Gazette.

SIR,
THE last number of the MEDICAL GAZETTE contained a letter from Mr. Gardner on the efficacy of the Senecio Jacobœa in Gonorrhœa, from which it would appear that the preparations of that plant are novel additions to the Materia Medica. I shall feel obliged by your informing the profession, through the medium of your journal, that I have, for some time past, prepared both the Succus and Extractum Jacobœæ, and that this remedy has been, to my knowledge, tried in cases of gonorrhœa with but doubtful success; and that, in order to facilitate further experiments as to its efficacy, and having a considerable quantity of these preparations by me, I shall be most happy to forward a quantity of either to any professional gentleman who may be induced to make trial of them.—I remain, sir,

Your obedient servant,
EDWARD BENTLEY.

41, Moorgate Street, Finsbury.

THE FLEXIBLE STETHOSCOPE.

To the Editor of the Medical Gazette.

SIR,
DR. G. BIRD having declined to answer the questions addressed to him, I close this correspondence by contradicting, with the concurrence of my estimable friend, Dr. Stroud, the statement that he ever presented me with a flexible stethoscope.—Your obedt. servant,

JOHN BURNE.

24, Lower Brook St., Aug. 2, 1841.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, July 29, 1841.

J. W. Turner, Banstead, Surrey.—J. R. Withecombe.—F. M. Peck, Newmarket.—W. S. Cortis, Hull.—R. G. Wollaston, Bishop's Castle.—W. E. Johnson.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 24th July, 1841.

Small Pox	14
Measles	15
Scarlatina	23
Hooping Cough	26
Croup	3
Thrush	5
Diarrhœa	14
Dysentery	1
Cholera	0
Influenza	0
Typhus	19
Erysipelas	3
Syphilis	1
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	133
Diseases of the Lungs, and other Organs of Respiration	220
Diseases of the Heart and Blood-vessels	18
Diseases of the Stomach, Liver, and other Organs of Digestion	70
Diseases of the Kidneys, &c.....	2
Childbed	6
Ovarian Dropsy	2
Diseases of Uterus, &c.....	4
Rheumatism	2
Diseases of Joints, &c.....	4
Ulcer	0
Fistula	2
Diseases of Skin, &c.....	1
Diseases of Uncertain Seat	93
Old Age or Natural Decay.....	56
Deaths by Violence, Privation, or Intemperance	13
Causes not specified	6
Deaths from all Causes	756

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

	THERMOMETER.		BAROMETER.	
July.	from 54 to 70		29·79 to 29·76	
Wednesday 28	48	67	29·72	29·68
Thursday . 29	49	63	29·65	29·62
Friday . . 30	47	64	29·55	29·58
Saturday . 31				
August.				
Sunday . . 1	46	65	29·70	29·88
Monday . . 2	53	67	29·88	29·76
Tuesday . . 3	57	69	29·66	29·44

Winds, S.W. and N.W.
On the 28th, and two following days, generally clear. The 31st ult. morning clear, otherwise cloudy; rain in the afternoon. The 1st inst. generally clear, rain in the morning and afternoon. The 2d, morning clear, otherwise overcast; raining heavily during the evening. The 3d, a general overcast, raining from 6 till nearly midnight.

Rain fallen, '56 of an inch.
CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, AUGUST 13, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XLV.

Acute Laryngitis. Symptoms. Treatment: blood-letting, bronchotomy, mercury, antimony; anatomical characters of the disease. Causes. Secondary Laryngitis. Œdema of the Glottis. Chronic affections of the Larynx.

THE disease of which I have next to speak is of far more serious character than those which were considered in the last lecture. *Cynanche laryngea*, or acute laryngitis, has proved rapidly fatal in a large proportion of the instances in which it has been known to occur. Yet, when the patient is seen tolerably early, and the nature of the malady is clearly perceived, and the source of peril thoroughly understood, I believe that our art is sufficient, in most cases, to rescue the sufferer from the fate that hangs over him. It is of the greatest importance, therefore, that you should be able to recognize laryngitis when you meet with it, and that you should comprehend the principles according to which it requires to be treated.

What is laryngitis? It consists, as that term implies, in inflammation of the parts composing the larynx; and especially of the mucous membrane that covers the laryngeal cartilages, including the epiglottis. The inflammation may be, and sometimes is, exactly limited to the larynx; but frequently it extends also to the posterior fauces, the velum palati, and the tonsils.

Symptoms of laryngitis.—The symptoms of acute inflammation of the larynx are these.

The patient complains of *sore-throat*. If you look into his throat you will commonly perceive some redness of the velum and uvula, and of the fauces generally. But there is a degree of restlessness and anxiety about the patient more than proportionate to the apparent inflammation. Among the earliest of the symptoms that bespeak danger, and ought to excite alarm, is *difficulty of deglutition*, for which no adequate cause is visible in the fauces; and to this is presently added *difficulty of breathing*, for which no adequate cause can be discovered in the thorax. The mode and character of the respiration are peculiar; it is attended with a throttling noise; the act of inspiring is protracted and wheezing, as though the air was drawn in through a dry narrow reed. If you ask the patient what is the *seat* of his distress, *where* the disease is situated, he points with his finger to the *pomum adami*. If he coughs, he coughs with a peculiarly harsh, stridulous, husky, abortive sound. He either speaks quite hoarsely, or (what is more common) all power of audible voice in the larynx is lost, and he speaks by means of his lips and tongue only, in a whisper. There is tenderness of the laryngeal cartilages; they are painful when pressed externally. The face is flushed; the skin hot and dry; the pulse hard. As the disorder advances, the patient's general distress increases; but some of the symptoms alter: his countenance becomes pale or livid, anxious and ghastly; his eyes protrude; he is miserably unquiet, impatient for some relief, declares or makes signs that he wants air, and begs that the windows may be opened; and if he does not *obtain* timely relief, he perishes—he dies strangled.

The pathology of this terrible disease is extremely simple. The membrane covering the interior surface of the instrument of the voice suffers inflammation. One effect of inflammation in mucous membranes is a thickening of those membranes; they become

turgid and swollen. Another frequent effect is the effusion of serous fluid in the subjacent cellular tissue. By this tumid thickening of its lining membrane, the chink called the *rima glottidis* is narrowed: it is still farther diminished in breadth whenever the membrane is lifted and protruded by infiltration of the tissue beneath it: it is so nearly closed up, that air cannot pass inwards in sufficient quantity to sustain the vital functions: a small portion only of the blood returned to the lungs from the right side of the heart undergoes the requisite change from venous to arterial. The miserable patient grows drowsy and delirious, and dies by a slow process of strangulation. If the *rima glottidis* becomes *quite* shut up, his sufferings, and his life, are quickly at an end.

This disease affords a good instance of the truth which was announced in a former part of this course of lectures; viz. that the *danger* of a morbid change may depend entirely upon its situation. It is so, eminently, with laryngitis. The inflammation is sometimes limited to a spot of membrane not bigger than a square inch. If a square inch and no more of the same membrane, a little lower down in the trachea, were inflamed in the same manner and degree, the complaint would be quite unimportant. Cynanche laryngea derives all its peril from the circumstance that the inflammation tends to close up what may well be called the *janua vitæ*. The part affected subserves two purposes: it is the organ of speech; and it forms a portion of the channel through which air is conveyed from without into the lungs. Both of these purposes are impeded in laryngitis. Now the animal function of speech may be entirely and permanently suspended without any danger to life. The function of respiration, which, though under the influence of the will, is an organic function, will not bear to be *suspended* even for a few minutes; and life cannot be *long* sustained when it is *much impeded*.

The difficulty of swallowing is a remarkable symptom, and almost always present. Yet it is not absolutely universal; for Mr. Lawrence describes a case in which it did not occur. It appears to depend, in some measure, upon the tumid and tender condition of the whole membrane which is common to the larynx and pharynx, and which is pressed upon as the larynx rises in the act of deglutition: but this symptom depends also, and in a greater measure, upon the state of the epiglottis, which is often enlarged, and fixed by the swelling in an erect position, and unable to execute its natural valvular office: so that when the patient makes efforts to swallow, a portion of the food or drink gets into the larynx, and a paroxysm of choaking dyspnoea ensues. By pressing down the back part of the patient's tongue, and getting

him at the same moment to make a coughing effort, you may sometimes obtain a sight of the tumid, red, and upright valve.

The dyspnoea is constant: yet there are pauses of comparative ease and quiet; and there are, commonly, periods of severe aggravation and urgent distress. It is probable that the permanent narrowing of the chink by the inflammation and its consequences is from time to time increased by spasmodic contraction of the muscles that close the glottis.

This is the first disease that has come before us, in which the respiration has been *primarily* impeded. If you call to mind what was stated in one of the early lectures respecting death by apnoea, you will be at no loss to understand the manner in which life is destroyed in laryngitis.

This formidable malady has always existed; for you may trace examples of it, under various names, even in the writings of the ancients. But it is only in recent times that it has been singled out from the rest of the anginae, and made a separate object of study. It has numbered some distinguished medical men among its victims: Dr. David Pitcairn, Sir John Macnamara Hayes, Sir George Tuthill. The celebrated General Washington died of it. When it has caused death it has generally run a rapid course, and proved fatal before the fifth day. It *has* carried the patient off in less than twelve hours.

It is of the utmost consequence to make an accurate diagnosis. Laryngitis is easily distinguished from cynanche tonsillaris by the extreme and peculiar dyspnoea which attends it. There may be difficulty of breathing in the latter disease, from enormous swelling of the tonsils: but then such swelling will mostly be *visible*. In laryngitis the marks of inflammation to be *seen* on inspection of the fauces are generally slight and trifling, and quite inadequate to explain the difficulty of swallowing. Do not, however, forget that laryngitis may *supervene* upon cynanche tonsillaris. Again, cynanche laryngea is readily discriminated from cynanche pharyngea; in which complaint there is great pain and difficulty in deglutition; but the breathing is quite free. In cynanche trachealis, or croup, which I shall next describe, the breathing *is* affected, and the swallowing *is not*.

Treatment: Blood-letting.—What is to be done for a patient labouring under acute laryngitis? How and when are we to employ the great remedy for acute inflammation—blood-letting? or are we to employ it at all? These are points concerning which it is quite necessary that your minds should be prepared and prompt to decide. If you look merely to the results of the recorded cases of this fearful complaint, you will scarcely find an answer to the question. In

some of them copious bleeding appeared to save the patients: in others, it was of no service, but rather seemed to accelerate their death. Sir John Macnamara Hayes suffered two attacks of cynanche laryngea. In the first he was freely bled. Dr. Roberts, of Bishop Stortford, informs us that the first bleeding was attended with *considerable relief*; the second also with *manifest advantage*; and by the third, *his safety appeared to be ensured*. Fifteen years afterwards he died of the same disorder, for which he was again bled and leeches, under the care of the late Dr. Baillie. Washington was largely bled, and died. Again, a Dr. Francis, of New York, recovered from acute laryngitis after copious venesection. It is evidently needful to consider and determine the circumstances under which we are to use, or to withhold, the lancet.

Bleeding, to be serviceable, or safe, must be performed *early*. There is, perhaps, no disease in which the *καιρος οξυς*, the fleeting opportunity, is more conspicuous than in this. When I say that you must bleed early if at all, I do not mean that you are to reckon so many days or hours from the commencement of the disorder; but you must ascertain what progress it has made; for it travels sometimes at a railroad-pace. You must look to your patient's actual condition: and I apprehend that your practice, in respect to blood-letting, may be safely guided by the following rules. If there be high inflammatory fever present, and the skin is hot, the pulse firm and full, and the cheeks are red, and the lips florid, you may bleed your patient with decision and advantage. But if his powers are beginning to sink under the poisonous influence of imperfectly aerated blood, if his skin be cold, or even cool, his face pale or leaden, his lips blue, his pulse small and feeble, his mind wavering—you will do no good by blood-letting: nay, you will increase the debility which already exists, and hasten the fatal catastrophe.

With regard to *local* blood-letting, and to counter-irritation, there is one remark make by Dr. Farre of much practical importance. It is a common practice, in affections of the throat, to apply leeches over or near the laryngeal cartilages, and afterwards to place a blister there. Now serous infiltration of the neighbouring parts often follows leech-bites; and the effect of a blister in producing serous effusion often extends beyond the skin; and the cartilages of the throat lie very near the surface; and it is possible that œdema of the glottis might be produced, or augmented, in consequence of these topical remedies. It will be better, therefore, when we wish to take blood locally, to take it by cupping from the back part of the neck: and when we desire to produce counter-irritation, it will be prudent to lay a blister on the

upper part of the sternum, rather than to the front of the throat.

In the advanced stage of the disease, *medicine*, I fear, can effect but little.

But *surgery* may be more successful.

Bronchotomy.—The danger arises from the *mechanical* obstacle to the entrance and exit of air into and from the lungs; and this state of peril admits of a *mechanical remedy*. If an artificial opening be made between the obstructed part and the lungs, the air is again freely inhaled and freely expelled; the blood undergoes the vital change from purple to scarlet; and the patient is placed in a condition of safety. He continues to respire through the hole thus drilled in the trachea, until the inflammation of the larynx has subsided; the thickening of the membrane disappeared; the submucous infiltration been re-absorbed; and the vocal instrument restored to its natural integrity; and then the aperture in the wind-pipe may be suffered to heal, and the patient will again draw his breath through its natural channels.

This is one of the triumphs of the healing art. It requires a knowledge of the general *pathology* of the disease, *i. e.* an acquaintance with the facts that acute inflammation may affect the larynx almost exclusively, and that its tendency is to narrow the fissure of the glottis, and destroy life by suffocation. It requires a knowledge of the *symptoms* of such inflammation: and it requires an accurate knowledge of all the essential circumstances of the particular case. For it is not every case in which the transit of air through the slit in the larynx is hindered, that is a fit case for the operation of tracheotomy. Some years ago there was brought to me by a surgeon a man breathing with considerable labour and constraint, the air passing through the larynx with an audible hissing noise. The surgeon wished to know my opinion of the propriety of opening the patient's wind-pipe. He had come to the conclusion that there was ulceration of the membrane lining the larynx, with thickening; that the cause of the sibilous respiration was partly mechanical, partly spasmodic; the little muscles that close the glottis acting with injurious energy in consequence of the neighbouring irritation: and he thought that this mischief in the larynx would have a better chance of being repaired, if the functions of the organ could be for a time suspended. He was aware, however, of the necessity of ascertaining what was the condition of the *lungs*; and he had not studied auscultation long enough to trust his own ear in that matter. The patient was pale and thin, and emaciated: and three minutes sufficed to convince me that his lungs were extensively disorganized. His respiration was not so difficult as to threaten suffocation; he was not dying of the laryngeal obstruction; and I recommended

that he should not be subjected to an operation which might curtail his existence, but could not effect a cure. The man died soon after; and we examined his body together. There was, as my friend had supposed, ulceration of the membrane near the chordæ vocales, and the lungs were full of suppurating or softening tubercles. I mention this case to shew you that it is necessary to ascertain the condition of the thorax generally before we perform or sanction such an operation as tracheotomy. Not that there is any thing very formidable, or painful, or dangerous, in the operation itself. But if we cut a hole in a patient's throat, who is sure to die soon after of some other incurable complaint, we shall incur the risk of being charged with having killed him. Do not misunderstand me, however. If a patient's *life* be threatened by acute laryngitis, or by laryngeal oedema, and we are *sure* of that, and if at the same time we are sure that he carries a mortal disease about him, we are not for that reason to let him die, if we can help it, of the *laryngitis*: any more than it would be lawful for us to administer a drachm of prussic acid to a man condemned to be hanged the next morning. But we must state the whole of the case plainly to the patient's friends, and propose the operation as the means, not of effecting an absolute cure, but of staving off the *immediate* danger.

And here let me repair an omission of which I was guilty when speaking just now of the diagnosis. My object was to guard you against mistaking laryngitis for some other malady: but I must also warn you against the converse error, that of mistaking some other malady for acute laryngitis. I can assure you that such a mistake has been made; and tracheotomy has been performed, too, when there was no disease in the larynx; and the practitioners by whose authority it was performed have been ungenerously reproached for their error, although no harm beyond the slight pain and inconvenience of the operation resulted from it. The cases in which this blunder has been committed have nearly all, I believe, been cases of *aneurism of the thoracic aorta*, which, by its pressure on the first divisions of the air-passages, or on the nerves thereabouts distributed, had caused that kind of laboured and stridulous breathing which is characteristic of laryngitis. I may venture to say that no person who has had opportunities of educating his ear for the purposes of auscultation, and has made a proper use of those opportunities, could ever overlook such a complication as this. I have myself seen a woman (I mentioned her case before) whose trachea was laid open by a surgeon while she was suffering under mere hysteria; so closely did that disease mimic laryngitis.

When you have good evidence that a mechanical obstruction to the passage of the air exists in the larynx, and that the tubes *beyond the larynx* are pervious and free; there are two things which I would recommend to you. First, I would most earnestly advise you not to *wait too long* before you propose or perform tracheotomy; and secondly, never to omit performing it *merely* because it may appear to be then *too late*. If, in the acute and limited disease, an artificial opening be made while the patient's strength is yet entire, and before his whole system is poisoned with venous blood, or his lungs are overwhelmed with sanguine congestion and serous effusion, it will almost infallibly save his life. But if the sinking of the vital power has got beyond a certain point, tracheotomy will not, in that case, rescue him. It is bad and foolish practice to wait, and try other methods, and postpone the operation as a *last resource*, when the circulation is evidently loaded with unarterialized blood. In my own case I should choose to be operated on early; the moment that I found early blood-letting was not *telling* upon the local distress, and that any shade of duskiness became perceptible in the skin; just as I should choose to be operated upon at once for strangulated hernia, after one fair attempt had been made by a skilful hand to return the bowel, without waiting till inflammation set in, or had been *caused* by the taxis. On the other hand, if you do not see your patient until his powers are nearly exhausted, do not abstain from the operation even though you may feel convinced that it will be unsuccessful; for if it does not save life, it will disarm death of its agony. A patient will lie sometimes for hours, painfully labouring for breath in deep and strong catches, at considerable intervals from each other: in fact, he is just in the condition of a man with a cord round his neck, not pulled quite tight enough to suffocate him at once. Besides, it is not always easy to say whether the period of possible recovery *is* yet gone by. I had a female patient in the hospital who had suffered one or two attacks of frightful dyspnoea, in which the main difficulty was referred to the larynx; but she had rallied from them before any steps could be taken for performing tracheotomy. On the next occasion, however, the seizure was so sudden and rapid, that although Mr. Arnott was luckily in the hospital at the time, before he could be found and brought to her bedside, the woman was, to all appearance, dead. Respiration had entirely ceased. This quietude of the larynx rendered the operation more easy. Mr. Arnott speedily made an opening into the trachea; some air was blown in through the aperture, and then pressed out again; and presently the natu-

ral respiration was renewed. The woman recovered; the orifice healed up, and she left the hospital. Three or four months afterwards word was brought that she had died at her own home after a short attack, and when there was no one at hand to open her windpipe. We got permission to examine the body, and found a large ulcer in the trachea, near the larynx; which ulcer by its irritation had occasioned, as we presumed, the spasmodic closure of the glottis. The preparation exhibiting the diseased parts is on the table before you. You see that there was enlargement of the thyreoid gland. This had probably nothing to do with the symptoms. There was also a large ulcer in the left bronchus.

Mr. Goodeve, surgeon to the Clifton Dispensary, operated on a patient in whom "no pulse could be found at the wrist; his face was suffused with blood, and his lips livid; and it was hard to say whether he breathed or not:" yet he recovered.

It so happens that there is at present (December 1838) under Dr. Wilson's care, in the hospital, a woman named Slack, who was rescued when almost *in articulo mortis*, by the same expedient. She had chronic disease of the larynx; but a sudden aggravation of the symptoms occurred; she became stupid and comatose, her countenance was cadaverous, her skin covered with a cold clammy sweat, and her breathing, which had been stridulous and laryngeal, had almost, if not quite, stopped. For a little while she had been making efforts to respire, not more than twice in a minute. Her pulse was intermittent, and extremely feeble. In this state the house-surgeon (Dr. William Merriman) made a small incision in the skin over the cricoid cartilage, and then thrust a large trocar into the tube. Air rushed through the opening, the respiration returned, the pulse revived, and the stupor passed away. This happened on the 10th of October. She is still in the ward; the aperture has closed up; and though she is not well, she is *living*.

What is the reason, you may ask, of these different and inconsistent results? How is it that bronchotomy shall reanimate one patient, whose last breath, but for its help, was already drawn, who was already motionless in apparent death; and yet shall fail to save another patient, who is still alive and sensible of his danger, and struggling with his disease? The difference depends, I make no doubt, upon the time that elapses between the commencement of extreme dyspnoea, and the performance of the operation; upon the slow or the speedy completion of the strangling process. And this, again, obviously depends upon the manner and degree in which the passage is narrowed. When the obstruction, though considerable,

is incomplete, and does not rapidly augment, the respiration continues to be performed, however imperfectly. Meanwhile the brain gets oppressed, the circulation tends to stagnate, and, above all, the lungs become gorged with black blood, and clogged up by effusion into their cells and substance. Secondary causes of apnoea are thus established, which do not cease when the primary cause is at length removed, by unbarring the main channel for the admission of air. Whereas, when the access of the atmosphere is suddenly or soon shut out, the lungs are not thus mortally injured, but remain capable of resuming their functions when they are again supplied with air.

Tracheotomy, then, will be the most likely to succeed, while the patient is still lively and strong; and after that the chance of success will be worse in those cases in which the apnoea has been *slow* in its progress, than in those in which it has been *rapid*. I repeat that, in threatening circumstances, the operation should be done *early*; but that it should not be withheld, through despair, at *any* period of the disease.

The effect produced upon the condition of the patient by the timely formation of an artificial glottis, is very striking. The moment that the scalpel penetrates the rings of the trachea, air begins to hiss through the incision; and when a fair opening is established, and a full inspiration is drawn in through the wound, several forcible expirations generally succeed, whereby a considerable quantity of mucus is expelled, which could not pass the contracted aperture of the natural glottis. Then the breathing soon becomes easy, the anxiety and distress are followed by a perfect calm, and usually the exhausted sufferer sinks into a tranquil slumber. This sleep is apt to be from time to time interrupted by the clogging up of the orifice with frothy mucus. It is requisite that some intelligent person should remain by the patient, to assist him in these emergencies, or he may still be throttled, notwithstanding the apparent prosperity of the operation.

When a sufficient hole has been made in the instrument of the voice below the glottis, the voice of course becomes extinct, or nearly so; and the patient is as unable to utter a cough as he is to use vocal language. Now this it is of some importance to notice, for he often wants to cough, in order to clear the air passages of mucus, or of blood, by which they may be embarrassed; and he may be helped to do so, or taught to help himself. First he should draw in a full breath, and then stop the orifice for a moment with his finger, while he makes the expiratory effort. And as the parts within the larynx recover, the patient, by a similar manœuvre, may enable himself to speak aloud.

As actual examples are more interesting and often more instructive than an abstract of results, I will tell you in a summary manner the history of a case of laryngitis, which occurred in one of my hospital patients, in the latter part of the year 1832. He was an old man, about 60. His name was Kent. He was brought to the hospital bloated with anasarca, which was most conspicuous in his legs and thighs. His breathing was laborious and difficult, and attended with a wheezing noise, audible at some distance. He could not lie down: he had a hard, but not full pulse. The dropsical swelling had come on suddenly five or six days before; and in the outset his face (he said) was so puffed up that he could scarcely see. He had been bled to the amount of a pint and a half, according to his own account, on the previous evening. I had a vein opened immediately, and 24 ounces of blood were drawn; and 8 ounces more were taken from the chest by cupping. He was thoroughly purged with calomel and senna. The bleeding gave him very little relief, so far as the respiration was concerned; but the next morning the anasarca had totally disappeared. I found him sitting up in bed, breathing with great effort, and with a loud stridulous noise which accompanied both inspiration and expiration. He referred all his uneasiness to two points; one of these was the larynx, the other the ensiform cartilage. He swallowed with great pain and difficulty; and every attempt to do so excited a fit of choking cough. There was no morbid appearance visible in the fauces; every part of his chest sounded well on percussion, and the murmur of healthy respiration could every where be heard in the lungs, almost drowned, however, in the louder laryngeal noise. As his strength was entire I had him again cupped, to twelve ounces, at the back of the neck; and prescribed three grains of calomel every three hours. He also inhaled the steam of hot water.

Upon visiting him again the same evening, I found the dyspnoea increased. Each act of respiration was attended with a loud croupy noise. His countenance was beginning to be anxious and ghastly. He was restless; and his pulse was less firm. I was so convinced that the operation of tracheotomy was the only thing that could save him, and that it could not be safely delayed, that I sent to request that Sir Charles Bell would come and perform it. By the time he arrived the restlessness had increased. The patient was shifting perpetually from one side of the bed to the other, as if seeking some new point of support: his face had become pale; and his lips were turning livid. He spoke with sudden, and as it were convulsive efforts; stating earnestly how thankful he should be to have the obstacle to

his breathing removed; and pointing to the larynx as the seat of his distress.

The operation, under such circumstances, is by no means an easy one to perform. Its difficulties were well exemplified in this patient. In the first place he was sitting up; he could not bear to be placed in a horizontal position. Then the dyspnoea caused him instinctively to elevate his shoulders, and sternum, and clavicles, to the utmost, so that the trachea was sunk deeply into the thorax; and the larynx was in constant and rapid movement up and down with a plunging motion, like that of the piston of a steam engine. Sir Charles, after some trouble, succeeded in cutting out a piece of the cartilage; for a mere slit did not suffice, it closed tightly during every inspiration, although it was open enough during expiration. At length, when the air was freely admitted, the breathing became gradually easy. I shall never forget the whole spectacle: there sat the poor man gasping and fighting for breath; his face covered with sweat, and wearing the most anxious expression. By and by what I have called an artificial glottis is opened for him; and presently afterwards, though half a dozen candles (as Sir Charles has himself painted the scene) are held close to his face, to throw light upon the wound, and though the surgeons, their hands covered with blood, are still busy about his throat, making arrangements to ensure the patency of the orifice, the patient falls fast asleep. It was necessary to place an assistant behind him to prevent his head from nodding forwards, and deranging the apparatus in the wound. Nothing can express more strongly than this fact the great distress and fatigue which had previously existed, and the perfect relief afforded by the operation.

This man ultimately got quite well: and he has since shown himself occasionally at the Hospital, in excellent health. There were two or three points about the case which I am unwilling to pass over without notice. It was evident that after the opening was made in his windpipe, he still breathed in part through the rima glottidis also; for the stridulous sound did not wholly cease. The aperture was formed as low as the circumstances of the case appeared to permit: the tube was perforated in the membranous space between the thyreoid and cricoid cartilages. Strictly speaking, *laryngotomy* was the operation performed. I do not enter into the consideration of the best place for making the opening: that point you will be taught by the professor of surgery: but it was observed in the case in question, that the slightest touch of the irritable mucous membrane, with a hook or a probe—especially if the touching instrument was turned

upwards towards the glottis—produced a fit of coughing, and a paroxysm of still more laborious breathing. For some days after the operation, it was noticed that a part of whatever he swallowed *appeared immediately at the wound*. Now this proved as plainly as if we could have seen the parts, that the epiglottis was thickened, and erected, and incapable of performing its protective function to the larynx: and it accounted for the paroxysms of choking cough produced by efforts at deglutition. At first the lining membrane of the larynx and trachea was so irritable, that the patient could not bear to have a metallic tube inserted; and an ingenious contrivance was adopted for keeping the orifice from being covered over by the lips of the wound. They were held apart by two bent wires, which were tied together at the back of the neck. After twenty-four hours had elapsed the irritation of the mucous membrane had so far abated that he was able to breathe through a canula.

There cannot be a doubt that this man was snatched from the very jaws of death by the intervention of the surgeon. A function indispensable to life was nearly suspended; and a substitute for the faulty organ was provided by art, until the interrupting cause was removed. Scarcely a year passes over our heads without the occurrence of one or two such events in the Hospital. When lecturing upon this subject last season, I was able to shew you a female patient whose life had been saved in a similar way. And there is now also (Dec. 11, 1839), in Pepys' ward, a rescued man, with the tube still in his windpipe. The operation was done on the spur of necessity by Mr. Tomes, the present house-surgeon, with a trocar. The patient, who was previously in a state of extreme distress, said, in a faint whisper, as soon as the opening was effected, "It's all right now."

He had been exposed to rain and cold about a week before; and had suffered pain and tenderness of the larynx. Prior to his admission he had been bled, *and salivated*, and had a *blister on the throat, which embarrassed the operation*.

Within the last eight years the operation of bronchotomy has been performed in the Middlesex Hospital fourteen times. Seven of the patients recovered; seven died. In two of the seven fatal cases, the condition of the patients was hopeless at the time of the operation. In four at least of the five others much relief from suffering was afforded by it, and life apparently prolonged. One of the patients was a young child: the opening was made by a trocar: much blood got into the air-passages, and the child, which seemed to be sinking previously, died within the hour. Life might, I think, have been preserved in this case, by a *timely* operation,

properly done. For a trachea so small, the scalpel is preferable to the trocar.

Mercury.—I have said nothing hitherto about the use of mercury in this acute disorder, because I hold it to be of very secondary importance, and because I have been anxious not to divert your attention from the two great practical points, *bleeding* and *tracheotomy*. Mercury may very fitly be given in those cases and circumstances in which blood-letting appears proper; but we cannot depend upon it: we cannot reckon upon its influencing the system *in time*; nor upon any marked improvement of the symptoms when it does produce its specific effects. After the operation, it is, for the most part, unnecessary.

Tartarized antimony.—Nor do I recommend the employment of tartar emetic, powerful as that drug is known to be in subduing inflammation of the mucous tissues. In the swollen and unpliant state of the epiglottis it would not be prudent to excite, or to hazard, vomiting. The contents of the stomach passing upwards would be apt to enter the unprotected larynx, and to cause hurtful, and distressing, and perilous attacks of suffocative cough.

Anatomical characters.—In the examination of fatal cases, sometimes the thickened membrane forming the edges of the rima glottidis is found covered with viscid mucus, which had formed an additional impediment to the passage of air towards and from the lungs: sometimes pus is discovered, lying in the sacculi laryngis, or scattered among the cartilages and surrounding muscles: and sometimes the chief morbid condition is the infiltration of the submucous cellular tissue. The effect in all cases is the same, that of closing up, wholly or partially, the narrow fissure between the arytenoid cartilages. The state of the epiglottis I have several times described.

This very serious disease is a disease of adult age: it is not often known to occur in children. They again are almost exclusively liable to *croup*: and *cynanche laryngea* has been called the croup of adults. But as the part occupied by croup, and the event of the inflammation, are both different from those of laryngitis, this name, croup of adults, is objectionable. I may remark, however, that sometimes in true croup, the inflammation, besides specially affecting the membrane of the trachea, extends to that of the larynx also.

Causes.—The main exciting cause of laryngitis is exposure to cold, or to cold and wet. My hospital patient, Kent, was a seller of small wares in the streets, and must therefore have been habitually in the way of such causes. The first attack of the disease in Sir J. M. Hayes was brought on by exposure at an open window to the night air

for some time, while he was undressed, and in a profuse state of perspiration, with a strong breeze blowing upon him. Dr. Craigie states that young persons from tropical climates, from the West Indies for example, are apt to be attacked by laryngitis soon after their arrival in Europe.

The disease is liable to be produced also by mechanical violence, or chemical injury done to the larynx. It has been caused, on several occasions, in children, by their attempting to swallow boiling water from the spout of a tea-kettle; and life has been saved in such cases by the performance of tracheotomy. The mineral acids, taken as poisons, have excited the disease. Fatal laryngitis has followed the incautious application of ammonia to the nostrils, in cases of hysteria, and of suspended animation: and I once knew a man nearly killed by the inhalation of the fumes evolved from cinnabar thrown upon a hot iron, in what is called *fumigation* of the throat for venereal ulceration of that part. I am afraid that I must confess also to have once seen acute laryngitis produced by a bungling attempt to introduce the stomach-pump, in a case of poisoning.

Secondary laryngitis.—In all these cases the laryngitis is primitive. But laryngeal inflammation, and especially laryngeal *œdema*, not unfrequently takes place, and proves suddenly fatal, in the course of other diseases. I have apprised you that in cynanche tonsillaris, the inflammation sometimes steals onward to the larynx. I have seen two or three cases of erysipelas of the head, attended, as it almost always is, with sore throat, wherein death took place suddenly and unexpectedly, and where the epiglottis, and the edges of the fissure of the glottis, were found to be œdematous: the inflammation of the throat had extended to the cellular tissue beneath the mucous membrane of those parts, and had led to the effusion of serous fluid there. The very same thing is apt to happen in other forms of exanthematous disease attended with sore throat, and especially in small-pox, measles, and scarlet fever. I have known a similar condition of inflammatory œdema arise from a *mercurial* sore throat in a broken down constitution. In these cases the laryngeal affection is consecutive; and in all of them the great remedy is the formation of a sufficient aperture beneath the obstructed glottis. In all of them also the essential symptoms, warranting and demanding the operation of tracheotomy, are the same.

œdema of the glottis.—A distinction has been made between *laryngitis* and *œdema of the glottis*; and it is a just and real distinction. œdema of the loose cellular tissue subjacent to the mucous membrane of the glottis is indeed one common consequence of inflammation of that membrane: but it may

occur independently of inflammation. The lips of the glottis become tumid and *drop-sical*; sometimes (as I have just pointed out) in consequence of a low inflammatory action in the throat, but sometimes also from obstruction of the veins leading from that part. When laryngeal dyspnoea accompanies aneurism of the thoracic aorta, it may, in some instances, result from local dropsy thus produced; and then tracheotomy is fully justifiable, and indeed demanded.

The main practical difference between mere œdema glottidis and acute laryngitis, is this: that in the former, there being no fever or inflammation, blood-letting is not requisite; and the operation of bronchotomy becomes the sole resource to which, in the extremity of danger, we can look for help. Mere œdema glottidis is seldomer attended with dysphagia too, than is laryngitis: yet, if the epiglottis be involved in the œdematous swelling, and unable to shut over the glottis, the act of swallowing will be followed by strangling cough, and increased dyspnoea.

Chronic affections of the larynx.—Besides the affections which I have now described or referred to, the larynx is liable to *chronic* disease: to chronic *inflammation*; chronic *thickening* of the membrane; slow *ulceration*; necrosis of its cartilages. Chronic inflammation and ulceration of that part is very common in *consumptive* patients. It is attended first with hoarseness, then with aphonia, a barking or stridulous cough, and all the melancholy accompaniments of tubercular phthisis. There has accordingly been a species of phthisis spoken of as *phthisis laryngea*. But in most, if not in all cases, this laryngeal affection is only a part of the complaint under which the patient labours; and what I have farther to observe respecting it, I shall postpone until we come to the consideration of tubercular consumption.

Again, the membrane lining the laryngeal cartilages is not unfrequently thickened and ulcerated in *secondary syphilis*: giving rise to a hoarse croaking voice, and a noisy and painful breathing. In such cases, or in chronic thickening of the same parts from common inflammation, you may do great good by *gently* introducing mercury into the system, until the gums rise. I have again and again seen the uneasiness about the throat, the noisy respiration, the rough or whispering voice, all cease, as if by enchantment, as soon as the specific influence of the mercury became manifest. There was a woman who used to apply at the Middlesex Hospital for an affection of this kind: whether it was syphilitic or not I could not well determine, but she lost it under the employment of mercury, two or three times: the complaint returning again after the interval of a few months, upon the re-application of

some irritating cause. In another female patient, who was long under my care in the hospital with similar symptoms, every thing failed to give her permanent relief, till I began to leech the neighbourhood of the larynx repeatedly. She had four leeches applied, I think, every night, and then every other night, for a fortnight or three weeks; the hoarseness or difficulty of respiration gradually diminishing all the time, until at length the perfect use of the instrument of the voice was restored. It is often necessary in these cases, while using local depletion, or mercury, to uphold the strength of the patient by nourishing but unstimulating diet: and it is always expedient that the organ should be kept, as much as possible, in a state of repose.

It is said that a little practice will enable a person to pass his finger into a patient's throat, and to familiarize his sense of touch with the ordinary condition of the upper part of the respiratory apparatus, so as to be able to detect swelling, or irregularity, or thickening about the chink of the glottis. And great advantage is said to have been obtained from applying remedies directly to the diseased or irritable part. This practice was much followed by the late Mr. Vance, who had been for many years a naval surgeon; and he called it, in naval phrase, *swabbing* the affected organ. A small piece of sponge, secured with a string, or fastened to the end of the finger of a glove, is dipped in a strong solution of nitrate of silver, and then carried down into the throat, as far as the spasmodic state of the muscles which the attempt induces will permit, and pressed downwards against the superior surface of the larynx. I believe other stimulating applications are sometimes employed in the place of the nitrate of silver. Now of this method of cure I do not know much, except by report. I have heard that many cases of chronic hoarseness and cough have speedily been cured by it. But I have more than once had what seemed satisfactory evidence of the beneficial effect of this expedient. The man Kent, whose case I have related, gradually regained the power of easy breathing through the natural passage, and the opening, which Sir Charles Bell had made, closed up perfectly. About a week after this took place, he began again to respire with a wheeze almost as audible as that which existed at the time of his admission; and to speak in a hoarse voice; and a night or two after the return of the wheezing, he had a paroxysm of extreme dyspnoea. I began to be afraid that the whole process of laryngotomy and the metallic tube would be again requisite. However, I got Sir Charles Bell to examine the interior of the throat, and we agreed that it would be advisable to swab the epiglottis and upper part of the air-passages with a strong

solution of lunar caustic. For he had no fever, and we thought it probable that the membrane might have been left lax, and in a state to be benefited by astringents. Sir Charles applied the sponge with very little difficulty; and the next day the breathing was greatly improved, and the hoarseness almost gone: and he never had from that time any recurrence of troublesome dyspnoea.

Mr. Arnott has twice or thrice, at my request, swabbed the upper part of the larynx for intractable hoarseness and aphonia: but with no good, nor any bad consequence.

The lining membrane of the larynx is liable also to warty growths, which impede the entrance and exit of air, and ultimately destroy life. There are several examples of that kind on record. I extract the following from my note-book:—

George Tenon la Font, aged 11, admitted March 4, 1828. He speaks in a whisper; complains of difficult breathing, and of cough. Inspires with a loud wheeze. Coughs with a sort of whistling sound, as through a narrow tube. The cough is most troublesome at night. Expectoration mucous, and inconsiderable in quantity.

Has been ill, in this way, all the winter—having had hooping-cough in the preceding autumn. There are marks of cupping on his throat. Little can be heard in the chest, the loud wheeze of his respiration obscuring all other sounds. In about a fortnight his gums were brought under the influence of mercury. No perceptible improvement ensued. A careful examination was again made of the thorax, and the conclusion arrived at was, that the obstacle to his respiration lay in the larynx, or upper part of the trachea, and that the lungs themselves were not concerned. After this, a blister to the throat, a seton near the thyroid cartilage, small doses of ipecacuan, emetics, and iodine were successively tried—but in vain. Towards the end of the month he began to suffer occasional, very violent, and apparently spasmodic attacks of extreme dyspnoea. He died during the night, two months after his admission. For some days before he had been manifestly worse than usual, was more feeble, wandered somewhat, and complained that his vision was imperfect. No noticeable increase, however, had taken place in the difficulty of breathing, except during the paroxysms of aggravation already mentioned. The death was sudden, and probably took place in one of these paroxysms.

When the body was examined the lungs were found sound as to structure, but copiously infiltrated, especially on the left side of the thorax, with serous fluid. At the very top of the larynx, involving the base of the epiglottis and the vocal cords, was a considerable warty growth, closing the rima glottidis almost entirely. The excrescences

sprung *chiefly* from one continuous base, and branched out precisely after the manner of what is vulgarly called a seedy wart. There were, however, several distinct smaller growths or warts: the main excrescence, having several heads, passed upwards from and through the rima, and so came to act partly as a valve during inspiration, which was always sensibly more difficult than expiration.

Ought tracheotomy to have been performed in this case? I now think so. But supposing it to have been done, and to have been successful, the boy would have been under the necessity of breathing through a tube for the remainder of his life.

There are two excellent specimens of warty growths in the larynx upon the table before you.

I might have referred, when speaking of chronic enlargement of the tonsils, in the last lecture, to the case of a little boy at present in the hospital under the care of one of my colleagues. He was brought to the hospital on account of great dyspnoea, and a hissing respiration, produced apparently by two enormous tonsils. With some difficulty (arising from his unmanageable age) a large part of one of the tonsils was cut off with scissars: but after the operation, (whether from any fresh swelling of the parts, or from pressure made by the remaining tonsil, which grew downwards, I understand, into the throat), his difficulty of breathing became extreme; and it was thought necessary to perform tracheotomy, which afforded him signal relief. He breathed for some time through the artificial opening in his windpipe. At length the other tonsil was partly removed: and the child is now well, and about to be discharged.

LECTURES
ON THE
FUNCTIONS OF THE NERVOUS
SYSTEM.

BY W. B. CARPENTER, M.D.

LECTURE IX.

Functions of the cephalic nerves, continued.

Nerves of the orbit. Emotional and instinctive actions.

THE *hypoglossal* nerve, or, *motor linguae*, is the only one which, in the regular order, now remains to be considered. That the distribution of this nerve is restricted to the muscles of the tongue, is a point very easily established by anatomical research; and accordingly we find that, long before the time of Sir C. Bell, Willis spoke of it as the nerve of the motions of articulation, whilst to the lingual branch of the fifth pair he attri-

buted the power of exercising the sense of taste; and he distinctly stated, that the reason of this organ being supplied with two nerves is its double function. The inference that it is chiefly, if not entirely, a motor nerve, which has been founded on its anatomical distribution, is supported also by the nature of its origin, which is usually from a single root, corresponding to the anterior root of the spinal nerves. Experiment shows that, when the trunk of the nerve is stretched, pinched, or galvanised, violent motions of the whole tongue, even to its tip, are occasioned; and also, that similar movements take place after division of the nerve, when the cut end most distant from the brain is irritated. In regard to the degree in which this nerve possesses sensory properties, there is some difference of opinion amongst physiologists; founded, as it would seem, on a variation in this respect between different animals. Indications of pain are usually given, when the trunk is irritated after its exit from the cranium; but these may proceed from its free anastomosis with the cervical nerves, which not improbably imparts sensory fibres to it. But in some Mammalia, the hypoglossal nerve has been found to possess a small posterior root with a ganglion: this is the case in the ox, and also in the rabbit; and in the latter animal, Valentin states that the two trunks pass out from the cranium through separate orifices, and that, after their exit, one may be shown to be sensory, and the other to be motor. Hence this nerve, which is the lowest of those that originate in the cephalic prolongation of the spinal cord generally known as the medulla oblongata, approaches very closely in some animals to the regular type of the spinal nerves; and though in man it still manifests an irregularity, in having only a single root, yet this irregularity is often shared by the first cervical nerve, which also has sometimes an anterior root only.

The hypoglossal nerve is distributed not merely to the tongue, but to the muscles of the neck which are concerned in the movements of the larynx; and the purpose of this distribution is probably to associate them in those actions which are necessary for articulate speech. It is generally stated that *all* the motions of the tongue are performed through the medium of this nerve. Perhaps this assertion requires some additional proof. It is quite certain that, in many cases of paralysis, the masticatory movements of the tongue are but little affected, when the power of articulation is much injured or totally destroyed; and I have myself seen such a case, in which the tongue was the only part in which paralysis manifested itself, and in which it seemed likely that the cause of paralysis was in the course of the nerve, and not in the medulla oblongata. If the hypoglossal

excites those motions only of the tongue which are concerned in articulation, we must look in the lingual branch of the fifth pair, or in the glosso-pharyngeal, for some motor filaments, through which the movements required for mastication and deglutition are occasioned. When this nerve is paralysed on one side, in hemiplegia, you will generally observe that the tongue, when you direct the patient to put it out, is projected *towards* the palsied side of the face : this is due to the want of action of the lingual muscles of that side, which do not aid in pushing forwards the tip ; the point is consequently directed only by the muscles of the other side, which will not act in a straight direction, when unantagonized by their fellows. It is a curious fact, however,—which was first publicly noticed, I believe, by Dr. Alison,—that the hypoglossal nerve is not always palsied on the same side with the facial, but sometimes on the other. This is perhaps due to the origination of the roots of this nerve from near the point at which the pyramids of the medulla oblongata decussate ; so that some of its fibres come off, like those of the spinal nerves, without crossing ; whilst others are transmitted to the opposite side, like those of the higher cerebral nerves ; and the cause of paralysis may affect one or other of these sets of roots more particularly. Whatever may be the validity of this explanation, the fact is an interesting one, and I would direct your attention to it as one not unlikely to present itself to your notice.

I shall next offer you a few general observations on the character of the cephalic nerves as distinguished from the spinal,—a point of much interest when considered in relation to comparative anatomy, and to embryology. It appears, from what has been already stated, that the par vagum, spinal accessory, glosso-pharyngeal, and hypoglossal nerves, may be considered nearly in the light of ordinary spinal nerves. They all take their origin exclusively in the medulla oblongata ; and the want of correspondence in position between their roots and those of the spinal nerves is readily accounted for, by the alteration in the direction of the columns of the spinal cord, which,—as long since pointed out by Rosenthal, and lately stated prominently by Dr. Reid,—not only decussate laterally, but, as it were, from behind forwards. The hypoglossal, as just stated, not unfrequently possesses a sensory in addition to its motor root. The glosso-pharyngeal, which is principally an afferent nerve, is stated by Arnold and others to have a small motor root ; at any rate, the motor fibres which belong to it are to be found in the par vagum. That the par vagum and a portion of the spinal accessory together make up a spinal nerve, has been already stated as probable. Leaving these nerves out

of the question, therefore, we proceed to the rest. Comparative anatomy, and the study of embryonic development, alike show that the spinal cord and medulla oblongata constitute the most essential part of the nervous system in Vertebrata ; and that the cerebral hemispheres are superadded, as it were, to this. At an early period of development, the encephalon consists chiefly of three vesicles, which are the rudiments of the ganglia, from which the three pairs of nerves of special sensation,—the olfactory, optic, and auditory,—take their origin. These ganglia correspond with those of the nervous cord of the Articulata, and mark three divisions of the cerebro-spinal axis ; and, in accordance with this view, the osteologist is able to trace in the bones of the cranium the same elements which would form three vertebræ, in a much expanded and altered condition. However improbable such an idea might seem, when the cranium of the higher Vertebrata alone is examined, it at once reconciles itself to our reason, when we direct our attention to that of Reptiles and Fishes ; in which classes the size of the cerebral or hemispheric ganglia is very small in comparison with that of the ganglia of special sensation, and in which the latter evidently form but a continuation of the spinal cord, modified in its function ; so that when we trace upwards the cavity of the spinal column into that of the cranium, we encounter no material change, either in its size or direction. The three pairs of nerves of special sensation, then, may be considered as making their way out, through the three cranial vertebræ which enclose their ganglia. At a later period of development, other nerves are interposed amongst these, which are *inter-vertebral*, and are evidently more analogous to the spinal nerves, both in situation and function. A separation of the primitive fibres of these takes place, however, during the progress of development, so that their distribution appears irregular. Thus the greater part of the sensory fibres are contained in the large division of the trigeminus ; whilst, of the motor fibres, the anterior ones chiefly pass forwards as the oculo-motor and patheticus ; and of the posterior, some form the small division of the trigeminus, and others unite with the first pair from the medulla oblongata, to form the facial. This last fact explains the close union of this nerve with those proceeding more directly from the medulla oblongata, which we find in Fishes and in some Amphibia. According to Valentin, the glosso-pharyngeal is the sensory portion of the first pair from the medulla oblongata, of which the motor part is chiefly comprehended in the facial nerve. It is very interesting to trace this gradual metamorphosis from the character of the spinal nerves, which is exhibited in the cephalic,

when they are traced upwards from the medulla oblongata; and this is shown, as formerly pointed out, as much in the nerves of special sensation as in the rest. Although we are accustomed to consider the fifth pair as *par excellence* the spinal nerve of the head, the foregoing statements, founded upon the history of its development, show that the nerves of the orbit really belong to its motor portion; they may consequently be regarded as altogether forming the first of the *intervertebral* or spinal nerves of the cranium. The facial and glosso-pharyngeal appear to constitute the second; whilst the par vagum and spinal accessory intervene between this and the true spinal, of which the hypoglossal may be considered as the first.

Motor nerves of the orbit.—We now return to consider the functions of the *third*, *fourth*, and *sixth* pairs of nerves, together constituting the entire channel of the movements of the eyeball. Their particular functions are but ill understood; and the movements which they govern offer so many peculiarities, that the inquiry becomes a very complex one. It is of peculiar interest, however, at the present time, in consequence of the assistance which a correct knowledge of these functions may afford us, in the treatment of strabismus by the operation which has now been so extensively and (when excuted with care and judgment) so successfully performed. I shall, on this account, dwell on the subject at a length which might otherwise appear disproportionate.

It will be recollected that, in the human orbit, six muscles for the movements of the eyeball are found,—the four recti, and the two oblique muscles. The precise actions of these are not easily established by experiment on the lower animals; for in all those which ordinarily maintain the horizontal position, there is an additional muscle, termed the *retractor*, which embraces the whole posterior portion of the globe, and passes backwards to be attached to the bottom of the orbit. This muscle is most developed in ruminating animals, which, during their whole time of feeding, carry their heads in a dependent position. In most carnivorous animals, instead of the complete hollow muscular cone,—of which the base encloses the eyeball, whilst the apex surrounds the optic nerve,—which we find in the Ruminants, there are four distinct strips, almost resembling a second set of recti muscles, but deep-seated, and inserted into the posterior instead of the anterior portion of the globe. It is obvious that the actions of these must greatly affect the results of any operations we may perform upon the other muscles of the orbit; and, as it is impossible to divide the former, without completely separating the eye from its attachments, we have no means of cor-

recting such results, but by reason alone. Experiments upon animals of the order *Quadrumana*, most nearly allied to man, would be more satisfactory; as in them, the retractor muscle is almost or entirely absent. If the origin and insertion of the four recti muscles be examined, however, no doubt can remain, that each of them, acting singly, is capable of causing the globe to revolve in its own direction,—the superior rectus causing the pupil to turn upwards,—the internal rectus causing it to roll towards the nose,—and so on. A very easy and direct application of the laws of mechanics will further make it evident to us, that the combined action of any two of the recti muscles will cause the pupil to turn in a direction intermediate between the lines of their single action; and that *any* intermediate position may thus be given to the eyeball by these muscles alone. The fact has not received the attention it deserves; it leads us to perceive that the oblique muscles must have some supplementary function. It may be objected that this is a theoretical statement only; and that there may be some practical obstacle to the performance of diagonal movements by the recti muscles, which renders the assistance of the obliques essential for this purpose. But to this it may be replied, that *no single* muscle can direct the ball either downwards and inwards, or upwards and outwards; that, as we have good reason to believe *these* movements to be effected by the combination of the recti muscles, there is no reason why the other diagonal movements should not also be due to them.

The action of the *superior oblique* muscle has been a matter of dispute. Unlike the other muscles which arise from the back of the orbit, its tendon is not inserted into the front hemisphere of the eye, but into a point behind its vertical axis; and we should, therefore, be led to suppose, that its operation is to move the pupil in a direction contrary to that in which its tendon is inserted;—that is to say, as its tendon passes, from its insertion towards the trochlea, upwards and somewhat inwards, we should suppose that, in shortening, it would draw the back of the eyeball in that direction, and turn the pupil in the contrary one,—namely, downwards and a little outwards. This theory of its action is borne out by experiments both upon the muscle, and the nerve which supplies it; for by laying bare the muscle without disturbing the eyeball or the neighbouring parts, and then exercising gentle traction upon it, so as to draw the tendon in the same manner as ordinary contraction of the muscle would have done, the eyeball is turned downwards and somewhat outwards. The same effect is produced when the fourth pair of nerves is

irritated, either mechanically or by galvanism, after it has been separated from the brain. On the other hand, the inferior oblique muscle may be shown, by experiments upon itself, to roll the eye upwards and inwards; the inward movement is much greater than the outward movement caused by the superior oblique; so that these two muscles are not exactly antagonists of each other.

The distribution of nerves to these muscles is very peculiar. The superior oblique has a nerve for itself alone, namely, the *fourth* pair; this was formerly called the *patheticus* nerve, from its being supposed to govern that rotation of the ball upwards and inwards, which gives a pathetic expression to the countenance; but, as just shown, its real action is the reverse. By Sir C. Bell, this nerve was considered as belonging to his respiratory system; and he endeavoured to show that the sudden movement of the pupil upwards and inwards, which takes place in coughing and sneezing, and the fixation of the ball in a similar position during sleep, is due to the operation of this nerve. The ascertained action of the muscle, however, constrained him to suppose, that the operation of the nerve was not to cause contraction but relaxation of the superior oblique muscle; by which the antagonist muscles might be free to occasion the movement. This idea affords a remarkable exemplification of the degree in which *theory* may, in some minds, usurp the place of observation. There is, as we have formerly seen, no ground for the assumption of a system of respiratory nerves distinct from those forming the general excito-motor system, from which a part of every motor trunk in the body is derived; and the supposition that the *action* of a nerve is ever to cause relaxation in a muscle, is at variance with all sound physiological induction. In this particular instance, it is at once refuted, by such experiments on the trunk of the nerve as those just adverted to. It may further be added, in regard to this nerve, that there is no decided reason to believe that it contains any sensory fibres. Its distribution is entirely restricted to the superior oblique muscle; but, since in this, as in other muscles of the orbit, there is certainly a degree of sensibility, as is experienced by the fatigue to which the long fixation or violent straining of them gives rise, it may be questioned whether the fourth pair of nerves is entirely motor. Its course within the cranium renders it very unlikely that this point can be satisfactorily determined by experiment. Müller states that a connexion exists between this nerve and the ophthalmic branch of the fifth pair; so that it is not improbable that, as in other instances, its sensory endowments are derived from this source.

The same may be said of the *sixth* pair,

which is termed *nervus abducens*, from its being solely distributed to the rectus externus muscle. There is no reason to believe that the actions of either of the two last mentioned nerves are ever involuntary; on the contrary, there will appear reason to suppose that they are, with a branch of the third pair, the sources of the voluntary movements of the eyes. Cases occasionally present themselves in which this nerve alone is paralysed; and the outward motion of the ball is then almost entirely lost.

The three other recti muscles, together with the levator palpebræ, and inferior oblique, are supplied by the third pair, commonly termed *oculo-motor*. The general question, how far this nerve is to be regarded as *exclusively motor*, was considered in the last lecture; that it is *chiefly* so, there can be no doubt. But we have now to inquire whether there is any ground for believing that different branches of the nerve are subservient to motions of a different character—some, for example, being more connected with the reflex function of the spinal cord; others with those instinctive tendencies which cause opposite muscular actions to take place in the two orbits by one effort of the will; and others being immediately directed and controlled by the will. It will be remembered that this nerve subdivides into two principal branches; of which one supplies the levator palpebræ and superior rectus; whilst the other is distributed to the internal and inferior recti, and to the inferior oblique. Now the action of the former appears to be of a purely voluntary character. We have no instance of the upper lid being elevated by any other than an effort of the will; and, if this be suspended, the orbicularis may be made to depress it by the reflexion of a stimulus applied to the edge of the tarsi. Moreover, when a strong light causes the lids to contract involuntarily, we feel conscious that a voluntary effort is required to keep them apart. The same may be said of the directly upward movement of the eyeball, which is caused by the superior rectus alone. It is never, so far as I know, any thing but a voluntary act; for the upward and inward movement adverted to by Sir C. Bell, is evidently occasioned by the inferior oblique acting alone. On the other hand, it is certain that some, at least, of the actions of the second branch are of a simply reflex nature, and that others cannot be said to be voluntary, but are rather of an instinctive character. It is from this branch that the twigs proceed, which enter the ciliary ganglion, and which govern the movements of the pupil; movements which have been already shown to be of a simply reflex character. Some have attempted to show that the actions of the iris are in a slight degree voluntary, because, by an effort of

the will, they could occasion contraction of the pupil; but this so-called voluntary contraction is always connected with a change in the place of the eyeball itself, occasioned by an action of some of its muscles. It is principally noticed under these two conditions. When an object is brought very near the eye, and we steadily fix our attention upon it, the axes of the two eyes are made to converge; and if this convergence be carried to a considerable extent, so that the pupils of both eyes are sensibly directed towards the inner canthus, a contraction of the pupil takes place. The final cause or purpose of this contraction is very evident. When an object is brought near the eye, the rays proceeding from it would enter the pupil (if it remained of its usual size) at an angle of divergence so much greater than that which would allow them to be properly refracted to a focus, that indistinct vision would necessarily result. By the contraction of the pupil, however, the extreme or most divergent rays are cut off, and the pencil is reduced within the proper angle. The principle is precisely the same as that on which the optician applies a *stop* behind his lenses, which reduces their aperture in proportion to the shortness of their focal distance. Contraction of the pupil is also noticed when the eyeball is performing that rotation upwards and inwards which has been already spoken of as occasioned by the contraction of the inferior oblique muscle; and which, when performed along with violent respiratory actions, or during sleep, must be regarded as involuntary. This movement also takes place, to a slight degree, when the eyelid is depressed, as in ordinary winking; and it is obvious that in this manner the surface of the eye is more effectually swept free from impurities which may have gathered upon it, than it would be by the downward motion of the lid alone. But the pupil is *not* contracted when the eyeball is *voluntarily* rotated upwards or inwards—an action which is principally affected by the superior rectus, some fibres of which are sufficiently far removed from the central axis of the globe, to give it an internal direction. There is good reason to believe, therefore, that the actions of the second branch of the third nerve are in great part automatic; and Valentin has founded upon this reasoning a very ingenious theory of the consensual movements of the eyeball, which I shall now endeavour to explain. The rectus superior, obliquus superior, and rectus externus, which are supplied from the first branch of the third pair, and by the fourth and sixth pairs, are all, in his estimation, purely voluntary muscles; and he considers them analogous to the extensors of the limbs, spine, &c., which are for the most part distinguished by the same character. By the

actions of these three muscles, singly or combined, the eyeball may be moved in nearly all directions. On the other hand, the inferior and internal recti, and the inferior oblique, supplied by the inferior branch of the third pair, are more or less automatic in their action; and these are compared, by Valentin, to the flexors. By the single or combined actions of these muscles also, the eyeball may be moved towards almost any point, except in an upward and outward direction.

OBSERVATIONS
ON THE
TREATMENT OF PHTHISIS.

To the Editor of the Medical Gazette.

SIR,

WITHOUT attempting to discuss the merits either of the extensive variety of general modes of treatment, or of the vast multitude of single remedies which now constitute the therapeutics of pulmonary consumption, I shall merely presume, in the following remarks, to adduce a series of arguments which, as they are intended to lead to a tolerably accurate appreciation of the causes which, in a very large proportion of cases, tend immediately to excite this disease into confirmed activity, may also induce the adoption of such a rationale of treatment as promises, in numerous instances, to be followed by beneficial results. I must, however, admit that I cannot attach much originality to the facts which I have advanced, as it has merely been my intention to apply a series of principles, most of which are commonly recognised by pathologists, to a system of treatment which, although it may not improbably have been long employed by several practitioners, and has already received some attention from writers upon phthisis, is not at present generally adopted by the profession.

It cannot long remain unknown to any careful pathological inquirer that cases of this, often so called, irreparable disease, are, in its earlier stages, continually undergoing an almost spontaneous cure. From rather extensive opportunities afforded me during several years of examining, post-mortem, the bodies of patients at a large metropolitan hospital, I think it may be safely stated that in at least one-eighth of the cases of those who die of various

diseases, appearances of phthisis, cured in its incipient stages, will be found commonly at the apices of the lungs, in the form of depressed puckerings of the pleural surfaces covering well-marked cicatrices, altered tubercles, or collections of strumous atheroma, together with calcareous masses, evidently of tubercular origin, completely enclosed within the pulmonary tissue. Again, we by no means unfrequently observe phthisical cavities of considerable size, but evidently undergoing a process of contraction, perfectly emptied of tubercular matter, and lined with a thick false membrane which, during life, poured out a copious secretion of healthy looking yellow pus*. These circumscribed cavities cannot be confounded with dilated bronchi, if examined with due attention, and their phthisical character is usually established by the presence of tubercles scattered in the lung immediately surrounding them. Indeed, it will usually be found, in examining the bodies of patients who have died of long-protracted phthisis, that the walls of the vomicæ first formed in the disease are nearly entirely free from ulcerating tubercular matter, being lined throughout with a tolerably even "pyogenic" membrane. This, then, indicates the mode which nature sometimes adopts for the remedy of phthisis in its advanced stages. Pulmonary tubercular matter, once in a state of suppuration, certainly possesses, in common with several other morbid products of strumous origin, the interesting property of undergoing a series of actions which would, without doubt, either isolate it eventually, or lead ultimately to its complete removal by ulceration, and to the patient's consequent recovery, providing (the cause which immediately excited ulceration having been removed) the sufferer's strength could be sufficiently maintained under the continual drain of pus from the abscesses; or if it were possible to prevent nature's sanative process from being fatally interrupted by the development of extensive disease in other organs, or, as is more frequently the case, by the occurrence of acute pulmonic attacks, to which phthisical persons are so liable as to render it a matter of common observation that patients (in hospitals) rarely die of the ultimate effects of the

disease, but are usually taken off by the sudden accession of extensive pneumonia, pleurisy, or pericarditis, appearing either singly or in conjunction, and generally assuming a highly asthenic character.

There is good reason to believe that tubercular matter deposited in the lungs, or elsewhere, is disposed to undergo but little change so long as it is permitted to remain in a state of rest. It can scarcely be doubted that many persons continue for years the subjects of slight strumous infiltration of the lungs without manifesting any of the symptoms of confirmed phthisis: and if there be truth in the supposition, maintained by several able pathologists, that this deposit is often congenital, it must, of course, frequently happen that those affected by it, passing through the numerous vicissitudes of disease incidental to childhood, either attain, in adolescence, a strength of constitution which defies the accession of phthisis, or, at any period of adult or declining age, become exposed to some unusually violent cause of disturbance to the respiratory functions, which, causing the tubercles to suppurate, develops the disease in its most confirmed severity. The cause of disturbance which, in a great majority of cases, appears to be mainly instrumental in setting up ulcerative action around pulmonary tubercles is irritation, of the bronchial membrane, and its necessary result, cough. There seems to be great fallacy in the generally received opinion that the cough in phthisis is commonly the *result* of the presence of tubercular matter either in the parenchyma or the air-cells of the lungs; for if it be, how shall we account for the long residence of bullets, shreds of cloth, and other extraneous bodies, of old and partially absorbed apoplectic clots, cerebriform and very extensive hæmatoid tumors, scrofulous matter converted into calcareous substance; and lastly, of miliary tubercles in the lungs of persons in whom their existence is not indicated by cough during life, and is often detected only by autopsical examination? Another argument, practically acknowledged by most writers upon the subject, of the frequent dependence of phthisis* imme-

* Laennec. Dr. Abercrombie, in Edin. Med. and Surg. Journal, vol. xvii.

* It will be perceived that, throughout these remarks, I make use of the term "phthisis" to signify pulmonary consumption in its developed and obvious form.

diately upon the irritation produced by the action of coughing, is derivable from the fact that many persons, members of highly consumptive families, who have suffered from repeated attacks of hæmoptysis, and present, in a striking degree, all those outward signs which are allowed to indicate the existence of tubercles in the lungs, continue for years, and perhaps to the extremity of a long life, to enjoy immunity from symptoms of confirmed phthisis, having fortunately chanced to escape attacks of bronchitis in this climate, or having always adopted the precaution of removing to a mild unirritating atmosphere abroad at the first apprehension of danger. As a converse to this argument, it will be sufficient to allude to the great mortality from phthisis which prevails among persons occupied in those trades where certain mineral or vegetable molecules are continually inspired; such as needle-pointing, flax-dressing, &c. It has been proved that but few of the workmen so employed survive the age of thirty or thirty-five years, unless they are careful to adopt some of the ingenious contrivances suggested to prevent the particles from being inhaled. So frequently is the ulceration of tubercular matter dependent upon irritation of the membrane lining the pulmonary air-tubes, that, upon making inquiry of patients as to the manner in which this disease first attacked them, we rarely fail to ascertain that, at the commencement, having caught cold, they suffered from bronchitis, perhaps complicated with pleurisy or pneumonia; that the severer symptoms abated under treatment, or wore off of themselves, but that the cough has continued, with more or less severity, ever since. I may mention, as an illustration of this, that the great characteristic of the influenza which passed through our metropolis like a pestilence, in the winter of 1836, was the intense and obstinate bronchitis from which nearly every person attacked suffered in a greater or less degree; and which appeared to give its peculiar fatality to that epidemic. For a long period after it had ceased to be prevalent a very unusual number of patients continued to be carried off by phthisis: many of these individuals, whom I had the opportunity of examining at Guy's Hospital, stated that they had never suffered from phthisical

symptoms previously to the time at which the epidemic seized them, and invariably traced back the commencement of their fatal malady to the period of that attack. And, lastly, it may be observed that, in nearly all the cases of patients who die during the earlier stages of developed phthisis, the linings of the bronchial tubes are found to present traces of inflammation, usually of an acute or subacute kind, evidenced by vascular injection and thickening of the membranes, with the effusion of a glairy and tenacious, or occasionally semi-purulent secretion. It must not, however, be supposed I would attempt to prove that the softening of pulmonary tubercle *invariably* depends upon the irritation of bronchitis; as it is unquestionable that instances occur in which this process must be attributed to causes purely constitutional. Take, for examples, the cases of those who, after suffering, for very long periods, from extensive glandular or arthritic suppurations of a strumous character, at last perish from the breaking down of tubercles in the lungs. Or, again, the instances, too commonly met with in this town, of individuals who, after enduring for years the extremes of famine, exposure to cold, excess in the use of spirituous liquors, unceasing toil, and the almost constant inhalation of a noxious atmosphere, gradually sink under the ravages of phthisis. It would be useless, in these and in several other parallel cases which might be adduced, to seek for accessions of bronchitis as the immediate excitants of the fatal attacks: but it does appear that, except in the most unhealthy districts of overpopulated cities, the occurrence of cases of this description is, if not absolutely, comparatively rare. For it is probable that pulmonary tubercles seldom begin to ulcerate*, until submitted to the influence of some direct cause of irritation, such as bronchitis; unless the patients are suffering under a diseased condition of the system far more severe even than that which has been termed "tubercular cachexia."

These remarks have not been offered either as new observations or as necessary confirmations of the even popularly understood, but often discussed, fact that pulmonary consumption may be called into activity by bronchial ir-

* Or rather to be surrounded by ulceration.

ritation; but with a view, in the first place, of directing the attention of your readers to the necessity of invariably inquiring minutely into the state of the bronchi at the commencement of developed phthisis; at that period when it is to be inferred, from the physical and rational symptoms, that the tubercular matter has ceased to remain in a quiescent state, and is beginning to soften and produce ulceration of its nidus. And, secondly, of urging the propriety of treating a proportion of the cases of phthisis, met with in this stage, simply as instances of acute or sub-acute bronchitis; by very carefully managed general and local depletion, counter-irritation, and the administration of mercurials and antimonials, with the other routine remedies for active inflammation of the pulmonary air-tubes; wherever the character of the cough, expectorated matter, or physical signs, indicates the recent induction of bronchial irritation; providing the patient's general health will admit of such remedies; as will be the case in a great majority of instances where a state of marked cachexia, the result of preceding disease or vicissitudes, has not been previously established. But, unhappily, these patients seldom become aware of the true nature of their malady, or think of applying for medical assistance, until some of the outward symptoms of confirmed phthisis begin to be superadded to those of bronchitis; when these, having taken on a more chronic character, will scarcely admit the employment of that very strict antiphlogistic system which would have been required a little earlier in the disease. Still it must, of course, be evident that (providing the views, already submitted, relative to the causes which appear to be usually productive of the suppuration of pulmonary tubercles be correct) an adoption of the old plan of tonic or stimulating treatment, during the earlier stages of the morbid process, is strongly contraindicated; as tending to generate a state of pulmonary congestion which is so likely to cause hæmoptysis, to aid the already commencing ulcerative action, and to render the patients more than usually liable to attacks of that fearful species of general pneumonia to which the subjects of phthisis are so fatally disposed. Whereas the pursuit of an opposite course, aiming at the

removal of the immediately exciting cause of the disease, must tend to leave the tubercle to undergo partial absorption or the ossific change; and to give any small vomicæ which may have formed a disposition to heal upon a natural principle, analogous to that which causes the sinuses extending from a scrofulous joint to close when the carious portions of the articulating surfaces, and with them the sources of irritation, are removed; and in the same manner as obstinate swellings and suppurations of the gums and cheeks often subside after the extraction of diseased teeth. For it must be borne in mind that the lung is probably as well disposed to heal after either injury or disease as any organ in the body; as is shown by the numerous cases upon record of rapid recovery from extensively penetrating gun-shot, bayonet, and rapier wounds of the chest; by the facility with which the pulmonary tissue is known to close over various foreign bodies which have been violently introduced; as well as by the obliteration of pneumonic abscess, and the absorption of large clots effused into the parenchyma from ruptured pulmonary vessels. It cannot, however, be assumed that the removal of the immediately exciting cause of the disease will be sufficient to put an end, at once, to its progress or to the patient's danger: it will, at best (if the plan of treatment has been so judiciously carried out as to effect the prime design of removing the bronchitis, without materially diminishing the constitutional power, which, if once lost in this malady, is so difficultly regained) place the diseased parts in a condition, the most favourable possible, to undergo a process always of necessity slow, and ever liable to be interrupted by fresh attacks of bronchitis, or to be terminated prematurely by some fatal accident, such as the opening of a vomica into the pleural cavity, or the sudden ulceration of an unclosed pulmonary vessel. It will be the practitioner's constant study to avert these and other casualties; as he may, very frequently, succeed in doing, by guarding the individual from injurious vicissitudes, and by taking, as a chief guide for his plan of constitutional treatment, the actual power (not the mere activity) of the patient's circulation; at the same time continuing in readiness to apply his remedies to

the obviation of local symptoms immediately they appear; as most of the cross-accidents which are apt to occur suddenly in this disease may often be recognised at their earliest approach, and checked, *in limine*, by an attentive and prompt observer. Successfully aided by such precautions, nature will seldom fail to proceed gradually towards the accomplishment of a curative process. We may now advert to the more advanced stages of phthisis—to the cases in which, extensive vomicae having formed, the patients are slowly becoming exhausted from the constant drain of pus and puriform mucus from these cavities and the bronchial membrane. It cannot be doubted that in all cases of confirmed phthisis the important functions of the lining of the air-tubes are seriously interrupted; the secretion of this membrane becomes profuse and morbid, and its power of throwing out a halitus is frequently in a great measure lost; to which last circumstance the occurrence of the colliquative sweats and diarrhoea, which add so much debility and anguish to the latter days of consumptive patients, appear to be often mainly attributable. It has already been submitted that acute bronchitis very commonly precedes the ulceration of pulmonary tubercles: it may, with equal truth, be alleged that the same disease, degenerated into its chronic form; is, although probably not the sole instrument, certainly one of the principal agents in keeping up the irritation upon which the extension of disease in the phthisical cavities depends. It cannot be argued successfully that the discharge from these abscesses gives rise to the inflammation of the bronchial membrane, as we seldom find the marks of active inflammation upon this surface (excepting where the patient has died of an acute attack superadded to the chronic), but merely those well-known characters by which bronchitis of very long-standing is commonly recognised. It is far more reasonable to assume that the contrary is the case, as the discharge from the abscesses is usually bland and albuminous, seldom becoming fetid until the disease is very far advanced*; whereas the muco-purulent secretion from the tubes contains a large proportion of saline

matter, and must therefore have a tendency to keep up irritation in the diseased surfaces upon which it gravitates. These arguments appear naturally to lead to the proposition that the advanced stages of phthisis may frequently be advantageously treated by means similar to those usually adopted in simple cases of chronic bronchitis—a practice which is doubtless very frequently followed by medical men, but, not being founded upon the principles above advanced, is seldom persisted in to the proper extent. The great primary objects of this plan of treatment are the diminution of the morbid discharge and the re-establishment of the natural exhalation from the bronchial membranes. A remark made by Dr. Rainy, the late distinguished Professor of the Theory of Medicine at the University of Glasgow, upon the great power which belladonna has in restraining too copious secretion from mucous membranes, led me to believe that this medicine might be advantageously employed in those cases of chronic bronchitis where there is evident relaxation of the air-tubes, accompanied by a very copious expectoration of thin sputa; and I found that it certainly appeared, although given in very small doses, to have the effect of diminishing the secretion with great rapidity. I am strongly impressed with the idea that we might very advantageously avail ourselves of it, with the intention of checking the bronchitis, in a certain class of cases of phthisis in the advanced stages; giving it, for about a week, in very small doses, combined with gentle diaphoretics and diuretics, to avoid the occurrence of diarrhoea; gradually increasing it until its action becomes evident; discontinuing its use immediately it shall have produced its effect in diminishing the quantity of the expectoration, and following it up by the employment of those means which are best calculated to promote healthy transpiration from the bronchial membrane*. The results to be expected from the adoption of such a plan of treatment are these. That the vomicae, having in all proba-

* When it does probably react upon the mucous membrane, which also, at this period, has a tendency to become aphthous.

* One of the best preparations of belladonna for internal use is the Tinctura Belladonnæ of the Pharmacopœia of Guy's Hospital. The following is the formula:—

“R Belladonnæ, unc. ijss.; Spiritus Tenuioris, Oct. j.; macera per dies quatuordecim et cola Dosis, a min. v. ad xv. bis terve quotidie.”

bility been already emptied of tubercular matter, would be left, on the subsidence of the bronchitis, in a state of comparative rest; and, as it has long been observed that the inner surfaces of chronic strumous abscesses bear a very close resemblance, in function, to the true mucous membranes, it is fair to presume that even the phthisical cavities themselves, having their secretion checked by the action of the belladonna, would after a time gain a tendency to contract: while deposits of tubercular matter in other parts of the lungs would be rendered less liable to take on suppurative action, as the frequency of the cough, and, with it, the main cause of irritation, became diminished. It would scarcely be judicious to prescribe the belladonna either in the earlier stages of the disease, or in cases where there is much hectic, or an evident tendency to pulmonary congestion; or, again, where portions of tubercular matter continue to be voided with the sputa. It can only be applicable to that class of cases in which the patients have been long struggling with the disease; where it is probable that the walls of the larger cavities are nearly free from tubercles, and death is to be feared from the slow drain which the constitution suffers in the profuse and long-continued expectoration. The very chronic cases of phthisis appear to be, in every respect, those which offer the fairest scope for medical treatment, as it will generally be found that the constitutions of these patients have been originally good; and it is usually fair to suppose (even if stethoscopic examination fails to acquaint us with the fact) that, although the cavities may be large, considerable portions of the lungs still remain free from tubercular infiltration; as in those cases where the deposition of tubercles has gone on to an extreme degree before symptoms of confirmed phthisis occur, the patients generally sink very rapidly from the spreading of suppurative action from mass to mass of the morbid deposit, or from the occurrence of extensive and intractable pneumonia. I would, in conclusion, again strongly urge the propriety of directing our main plan of treatment in most cases of phthisis, during the two stages which I have attempted to describe, to the relief of the bronchitis which so very frequently attends them; making use, at the same time, of any

of those local or constitutional remedies which have been found beneficial in phthisis, and may not, in any way, interfere with the carrying out of this principal intention.—I am, sir,

Your obedient servant,
NORMAN CHEVERS,
M.D. M.R.C.S.

5, Haymarket, July 23, 1841.

HÆMORRHAGE FROM CONSTIPATION.

To the Editor of the Medical Gazette.

SIR,

As I consider the following case highly interesting and instructive, I feel it my duty to transmit it to you for insertion in your journal. I send it as recorded in the notes and remarks made during the time of my attendance.

I remain, sir,
Your obedient servant,
ALFRED LORD.

Islington, July 10th, 1841.

April 13th, 1841.—I was called this night, at half-past eleven, to see W. F. L., æt. 19, who had just arrived at Staples Inn from the country, where he had held the situation of assistant in a classical academy. His most urgent symptom was violent spasm of the muscles of the upper part of the trunk, for which I prescribed a sedative mixture, leaving his case for more full investigation next morning.

14th.—I found him this morning up and dressed. He had passed a tolerable night. His countenance was florid, but not flushed; eye bright; pupil contracted; face indicating no emaciation; the body and limbs muscular, and even fat; chest pigeon-shaped; ribs flattened on left side over region of the heart. He still suffered, though in a less degree, from spasmodic action of the diaphragm and muscles of the back and chest. Pulse 70 to 80, regular; tongue moist, and slightly coated with a brown fur: dyspeptic. He has constantly vomited blood in large quantities, and also passed the same by stool and with his urine, and been subject to frequent epistaxis; has slight cough, with expectoration tinged with blood; breath very short; there exists great tenderness to the touch under both clavicles, and over

the region of the heart; respiratory murmur at upper part of left lung dull; perspiration at night occasional, but not profuse. The bowels previous to this illness were habitually constipated. He informs me that he has frequently had diarrhœa during his illness, which has been checked by anodynes; and when he has taken purgatives his motions have been relaxed, but mixed with lumps of fæcal matter. He complains of a sense of fulness, and can bear no pressure over the abdomen, which is tumid and very hard: appetite slight; takes only arrow-root.

I remember that I attended him three years since, when, from his symptoms, and the fact that his mother died from phthisis, I came to the conclusion that he would very probably, at some remote period, also prove consumptive. I remarked at that time the peculiar development of the ends of the bones, and all those symptoms which denote the absence of a due portion of earthy particles in the osseous system, as well as a want of firmness and tonicity in the other solids. He informed me this morning that he has been, during the last several months, under the treatment of three surgeons, successively, of whom the last in attendance called in a physician resident in the town, who attended him during the three weeks preceding his visit to London. I also received this morning a letter from this physician, conveying to me, in a very courteous and unreserved manner, an account of the case, and the treatment to which his patient had been subjected. He spoke of the case as one of "great peculiarity," and was inclined to think that there existed "serious organic lesion both in the stomach and one of the large intestines," as well as "some morbid affection of the phrenic nerve and par vagum." His treatment consisted in the occasional administration of purgatives; in the use of the Plumb. Acet. with Ext. Opii, and the application of leeches and blisters. I must in justice beg particular attention to one part of his letter, where he stated that owing to the shy and eccentric habits of his patient, neither himself nor his usual medical attendant could ever see any of the discharges.

I give the following remarks as they stand in my original notes. Here the most striking symptom is the hæmor-

rhagic tendency, blood being constantly discharged from five outlets—the stomach, bowels, nose, lungs, and kidneys, or bladder. This may form a part of that general laxity of system to which I have alluded as having existed three years since; still there must be some other circumstance to bring this tendency into operation, and one must be found consistent with the other symptoms. It cannot be phthisis, as all other symptoms of an advanced stage of this disease are absent. The hæmorrhage is too general to be the result of an organic lesion. Is it not obstruction? Then, where the seat, and what the nature of that obstruction? Does it not consist in constipation of the bowels, and do not the tumid state and tenderness of the abdomen, the frequent diarrhœa, and the passing of solid fæces after purgatives, favour this opinion? Is not the proper secretion from the exhalents of the mucous surface thus prevented, and the circulation interrupted—every organ gorged? And does not nature seek to remedy this by the occasional exudation of blood throughout the whole mucous track? And may not the spasmodic action be caused by the consequent irritation of the sympathetic, pneumogastric, and phrenic nerves? It appears to me to be not only probable, but almost certain. I have, therefore, to feel my way, ordered a table-spoonful of castor oil to be taken, and repeated if necessary, and the evacuations to be kept for my inspection, determining to be guided by them as to the propriety of administering further purgatives.

15th.—Passed a good night; suffered little from pain or spasms: took yesterday ʒiiss. of castor oil, which caused the expulsion of a great quantity of fæcal matter, of which there was at least lbj. of scybala, of the circumference of a small orange, and three or four inches in length, of a dark colour, and most offensive smell, and mixed with a small quantity of blood. The abdomen is considerably less tumid; considerable pain and distension at the commencement of the descending colon; breath not so short. To take the same quantity of castor oil, and throw a quart of warm water up the rectum.

16th.—Feels to-day much better; has lost the sense of fulness, of which he so much complained; pain and distension of colon less; breathing im-

proved; has expectorated a little blood; convulsive action only occasional and slight; appetite good; pulse and tongue as before. Has used the injection, and taken the oil, and discharged about a pint of fæcal matter, semifluid, evidently scybala broken up by the injection and oil, a considerable portion consisting of the inner skins of oranges and seeds of figs, which he supposes were eaten a month since; not so dark or offensive as yesterday.

Oil and Injection to be repeated.

17th.—Has passed about the same quantity of fæcal matter as yesterday, and of a similar quality, though of a lighter colour; abdomen generally much less tumid, especially in the region of the descending colon; has had very slight return of the convulsive action; no discharge of blood; sleeps well; appetite good.

To continue Oil and Injection.

8th.—Still improving; very little pain and spasm; bowels acted several times; motions watery and light coloured, containing no blood; slight expectoration of blood; urine turbid; tenderness over the chest still exists; walked a mile yesterday.

R Tinct. Digital. ʒj.; Pot. Nitrat. ʒi.; Acid Sulph. Dil. ʒss.; Inf. Rosar. ʒvj. M. A sixth part three times a day, with a tea-spoonful of salts as often as required.

19th.—Complains more to day of pain and spasm, perhaps induced by walking four miles yesterday, and eating too full a meal. He informs me that he has not been able to walk a mile for the last six weeks till yesterday; and that, till he came to town, he could not eat more than two or three mouthfuls without stopping, the food appearing to go no further than the chest. Bowels acted freely; motions watery. Pulse 60; tongue cleaner. I went with him to-day to see him take a vapour bath. The temperature was very gradually increased, lest hæmoptysis should be produced. Whilst in the bath he perspired profusely, and when he left it was free from pain and spasm.

20th.—Bowels acted three times; last motion copious, consisting of very dark green fæculent matter, taking the form of the rectum, but very hard. Has had slight expectoration of blood this morning. Says he has spit blood for the last three years almost every morning.

Urine copious, and less turbid; abdomen much reduced in bulk, and softened. Slight tenderness, and considerable fulness, in situation of arch of colon, and in the right hypochondriac region. To continue the mixture, with a little gentian, and one of the following pills with each dose.

R Hyd. Chlorid. gr. $\frac{1}{2}$; Ext. Colocynth, gr. iij.; Ol. Menth. Pip. ʒj. ft. Pil. Mitte xij.

21st.—Improving in every respect. No hæmoptysis. Passed a remarkable quantity of dark fæculent matter, partly scybalous. Urine clear. To continue pills and mixture.

22d.—Has had several motions of dark fæculent matter. Last night two or three table-spoonsful of blood were suddenly voided by the mouth, but not in the act of coughing. Has directed my attention to a pulsation in the epigastric region very sensible both to the touch and sight. To take two pills three times a day.

23d.—Slight hæmoptysis. Pulsation less distinct. Has passed an almost incredible quantity of dark fæces, mixed with the skin of pears, which he said he ate a month since. To continue pills, with blue-pill instead of calomel.

24th.—Has passed a great number of watery evacuations perfectly free from scybala, but mixed with fæcal matter, and containing, for the first time, bile. Has very severe pain in right hypochondriac and iliac regions. Fearing the effects of hypercatharsis, I have directed six leeches to be applied, to be followed by an opium poultice.

25th.—Quite free from pain. Bowels acted four times without medicine; motions relaxed, not watery, and of good colour.

26th.—No pain, spasm, or hæmoptysis. Bowels acted freely without medicine. Apparently quite well.

28th.—Each day, since I saw him, has passed five motions without medicine, caused, I imagine, by the free secretion of bile, to the stimulus of which the bowels appear to have been so long unaccustomed. I this day discontinued my attendance. In the course of a fortnight he passed about one hundred motions.

On the 29th he called upon me, and said he had that day walked nine miles. A short time after, he returned to the country. The last report received from him bore date 27th of May, when

he stated that he was in possession of better health than he had had for some years past.

I think my professional brethren will consider me justified in concluding that the hæmorrhage was produced by obstruction consequent upon constipation; the convulsive action, by irritation of the sympathetic, pneumogastric, and phrenic nerves; and the extreme shortness of breath, by the thrusting up of the diaphragm, thereby diminishing the cavity of the chest.

I communicated to the physician lately in attendance the result of the case, and my opinions thereon, who agreed with me in every respect, excepting that he could not but imagine there must be some structural lesion from which the hæmorrhage had occurred, not only whilst the patient was under his care, but also on other and frequent occasions.

ON THE
PRODUCTION OF MALARIA AFTER
INUNDATIONS FROM THE SEA.

To the Editor of the Medical Gazette.

SIR,

AFTER perusing in your journal Prof. Daniell's interesting paper on the Spontaneous Evolution of Sulphuretted Hydrogen Gas in the Waters of Western Africa, and his quotation from Signor Giorgini's paper in the *Annales de Chimie*, it occurs to me that the scientific inductions of those gentlemen have been long since anticipated by one of our most distinguished provincial physicians. To Dr. Robert Hamilton, of Lynn Regis, the author of the invaluable monograph on the uses of mercury and opium, in Duncan's Medical Commentaries, we also owe a pamphlet of great practical utility, designated "Observations on the Marsh Remittent Fever, more particularly in regard to its appearance and return every autumn after the inundation from the sea, on the 1st of January, 1795, and the five succeeding years at Lynn, and its environs."

As Dr. Hamilton's pamphlet is not accessible probably to the great majority of the profession, some quotations therefrom may not be inapposite. He says, "The remittent fever is the same

disease every where in the neighbourhood of low swampy or marshy grounds, which are subject to be overflowed by the freshes after great rains, or to be *covered occasionally by inundations from the sea.*" In allusion to the undrained state of marsh land and the Bedford level at that period, he says, "The banks in those places near the sea are subject to be broken by the impelling force of high tides, accompanied with strong gales of wind; and those in the inland parts of the country are liable to breaches from the weight of water after great rains. The inundations from the sea are generally followed by worse consequences, in respect to health, than those from the fresh water. If they extend far, they cover much low ground under cultivation, and fill many ditches, which, in many situations, cannot be drained by any other means than evaporation by the heat of the sun. Dead fish, left upon the overflowed land, become putrid, and animal and vegetable life destroyed by the salt water, in the various insects, reptiles, and smaller order of animals, and the different vegetable productions which happen to be covered by it; and the miasmata which must arise from such an accumulation of putrefaction produce the worst kind of remittent fevers." After one of the irruptions of the sea, he observes, "The stench of the exhalations arising from the corrupted stagnant water, and putrid half-dried mud or ooze, was most intolerably offensive; the effluvia was almost suffocating; and it was almost impossible to avoid a nausea, a languor, and a constant spitting, whilst within their influence."

The memorable new-year's gale, 1779, was attended by an inundation of the sea; the effects of which, developed by the extreme heat of the five successive summers and autumns, were seen in the fevers of those years, which were more violent, universally epidemic, and more fatal, than in any period within Dr. Hamilton's remembrance since he had resided at Lynn, which was then forty years.

The devastating epidemics so graphically delineated in the above pamphlet have long ceased; and the practitioner of the olden time (some sixty years since) would no longer identify the prevalent diseases of this district, as they now present themselves. One of the

most unhealthy portions of Britain has become one of the most salubrious, thanks to the spirit of enterprise which originated and accomplished the drainage of this portion of the Bedford Level. The river Nene, one of the principal outlets of this system of drainage, now ebbs out ten feet below its former level. Through the Nene Outfall and the Eau-Brink Cut the science of Telford and Rennie have secured an almost complete natural drainage.

The observation of Signor Giorgini that "the mixture of fresh and salt water thus formed, and which, in summer, was rarely changed, became corrupt, and spread infection over the neighbourhood of the most destructive kind," is equally applicable to the state of marsh land in the time of Dr. Hamilton.

The pamphlet was published with other papers by the doctor's widow after his decease, which took place in 1793; hence there is an error in the title, and for 1795 read 1779.

The production of one form of malaria; by the decomposition of the sulphates in sea water, seems the most ingenious theory yet promulgated. To the professional chemist may be left the discussion of the subject.—I am, sir,

Your obedient servant,

WILLIAM ENGLAND, M.D.

Wisbech, July 31, 1841.

MALFORMATION OF THE GENITALS.

To the Editor of the Medical Gazette.

SIR,

THE present case of malformation of the genitals has come under my observation, similar to the one related by Mr. Earle (in the *Lancet*, vol. ii., 1832 and 1833, p. 797); and what renders these cases more singular than other instances of *lusus naturæ*, is the semblance of health they present. This child is in every way, humanly speaking, calculated to have an average duration of life. On examination of the case, with my friend, Mr. D. Culhane, of Dartford, June 30th, 1841, the following appearances presented themselves. The child was in excellent health, with a good cranial development; chest well formed; the arms and legs of full size;

the scrotum and testes natural. Immediately above the scrotum there was a fold of integument attached to what might be termed a penis; there was the glandula preputium, with the inferior surface placed superior, and grooved along the urethral part of the penis; between the umbilicus, pubes and recti muscles intervened the posterior part of the bladder, with a termination of the ureters and vas deferens, the internal surface of the bladder being exposed to the air. On again examining the case, July 8th, I was surprised to see two streams of urine passing out through the ureters, as if they had been expelled by muscular power. Without doubt the pelves of the kidneys were filled, and by the act of undressing the child, pressure was made by the intestines over the anterior part of the kidneys, so as to expel the water in the form of a stream, muscular fibre having never been demonstrated either in the glands or their ducts. The correct definition of the erectile tissue is still involved in mystery. Professor Müller thought he had demonstrated a set of helicine vessels, but was successfully refuted by M. Valentin, who subsequently showed the peristaltic action of the ureters through the influence of the sympathetic nerve, by irritating any of the abdominal ganglia. In Mr. Earle's case, which was in a child seven years of age, he contrived an apparatus to prevent the constant dribbling of water over the skin, by having a silver bowl attached to an elastic tube communicating with a bladder, and secured round the thigh. It would be desirable to know whether the plan pursued answered the purpose, as it is well known to every practical man how great the difficulty is in affording even temporary means of relief in any case where there is a deficiency of tone in the bladder.—I am, sir,

Your obedient servant,

JONH GRANTHAM.

Crayford, Kent, July 26th, 1841.

Dr. Vernon, of Bury, in Lincolnshire, has reported a similar case in the *Edinburgh Medical and Surgical Journal*, vol. xxvii., 1827, p. 81.

MEDICAL GAZETTE.

Friday, August 13, 1841.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE COLLEGIATE SYSTEM IN MEDICAL SCHOOLS.

It is with sincere pleasure that we find the opinions which we expressed last April, on the propriety of adopting some measure of the system of Colleges for the more regular discipline and education of medical students, entirely concurred in by some of the most influential members of our profession. Nor are the assurances of ready co-operation confined to them. We have lately received a pamphlet* by the Rev. J. H. North, the estimable chaplain of St. George's Hospital, in which the advantages of the system are urged, though briefly, yet with an admirable force and clearness; and we have good reason to believe, that what he has expressed is entirely in accordance with the feelings of several of the members of his profession, and, indeed, of all who are in a position to judge of the necessity of such a system as that we have advocated.

Mr. North's letter is addressed to Sir Benjamin Brodie, than whom, if this indicates his readiness to give his aid to the furtherance of the system, a more important coadjutor could not be found. It advocates the plan solely on the ground of the moral, and, if we may so speak, the domestic advantages which it would afford to the medical student. And this it does by presenting the same comparison which we drew; that, namely, between the respective conditions of a young medical student and of a freshman going up

to one of our universities. The comparison is extremely well drawn, and we think that no honest man can read it without being convinced of the great benefit that will result, if the metropolitan student can be afforded only a part of the advantages of a collegiate system. In a domestic point of view, he would exchange his rooms, now chosen by hazard, with every danger of fraud, and with certainty of discomfort, for others set apart for him either in the college itself, or in some adjacent appointed house, and arranged with every regard for economy and comfort: instead of taking his meals alone and irregularly, in all the noise and annoyance of a public room, he would have them in the company of his fellow students, in their common hall, with all possible attention to his comfort, his health, and his respectability: and in a moral view, instead of being so completely his own master (as it is called), that he can scarcely avoid becoming the slave of his own momentary inclination for folly, he would have the prudent superintendence of his seniors, and (which would have a much stronger hold upon him) the desire of the esteem of his fellow students, amongst whom he would be continually placed, and whose general assent to the rules of the College would make them *their* rules, which no one among them could disregard without incurring the displeasure of the rest. In a word, instead of being placed as he now is, in a position such that, if he does well, it is only by great labour and self-control, and in spite of his external circumstances, the medical student would find himself in one where every circumstance around him would favour his advancement towards a good result. He might in such a case do ill (for if there are some whom no circumstances can prevent from making way to good, there are others whom none can benefit), but if he did,

* A Letter to Sir B. C. Brodie on the Application of the Collegiate System to the Medical Schools of the Metropolis. By the Rev. J. H. North, M.A. Chaplain to St. George's Hospital. Churchill, and Hatchard and Son.

it would be utterly his own fault: it would be in spite of circumstances that he did ill, as much as it is now in spite of them that any one does well.

Mr. North has taken care to prove the error of thinking that there can be no need of a change in a system under which such men as have ornamented our profession have been trained. Such a change might not be necessary for the great and eminent, and yet might be most useful for the ordinary class of students. "That the resolutely industrious will make good progress under any system, while the incorrigibly idle will be mended by none, is no good argument against such an improvement in the mode of education as shall provide suitable aids for those who, being neither resolutely industrious, nor incorrigibly idle, are likely to be assisted or hindered accordingly as the circumstances by which they are surrounded are or not favourable to their comfort and discipline." We need not say, that among medical, not less than in any other class of students, those who are of the kind to receive such benefit are a great majority of the whole. If there be any peculiarity among the students of our profession, it is, that, from the nature of their occupation and their associations, they, more than others, need that circumstances should be rendered favourable to their progress towards good.

For all that relates to the moral and domestic advantages of the collegiate system, we are content to refer to Mr. North's pamphlet; it will not fail to produce a strong feeling in favour of the measure that it advocates; and we cannot yet wish any thing better for the cause, than that it may have a very wide circulation both among and beyond our own profession. But we are very anxious to have it clearly established that the evils of the present absence of system in medical schools

are not confined to the domestic arrangements and comforts of the student; but exist as palpably and as mischievously in all that regards his professional education. If "the life of a medical student may be well described by the word desultory," there is certainly no part of his life to which that term is more completely applicable than it is to the mode in which he commonly pursues his studies. He receives no more advice or guidance as to what books or even what subjects he should work at, than he does as to what rooms or what associates he should choose; nor is his own judgment in the matter more likely to lead him aright in one than in the other case. The only plan, if it deserves the name, that is set before him, is the curriculum of education required by the Apothecaries' Company; and this he cannot attempt to follow without absolute mischief. The subjects which it gives him to study at once and all together are so numerous and so wide, that not even the most resolutely industrious, though he may possess more than the average amount of abilities, can hope to master them all. Chemistry, Materia Medica and Therapeutics, Practical and Physiological Anatomy, and, in general, Surgery both by lectures and practice, are expected to be studied, and in great measure learned, in seven months, by a man who, it may be, has previously been in utter ignorance of them all. The result of such a requirement is, that either all the subjects are totally neglected, or, which is not better, they are all studied, but no one of them learned to any profitable degree. They are rare exceptions, in which a student has the discernment to find out that he is misguided by the curriculum, and must altogether neglect the majority of the subjects set him, for the sake of studying profitably some one or two of them.

And even when such a subject of study is determined on, as with proper management might be learned well in the given period, still, for want of due guidance, every student throws away much both of time and of labour. The same desultory plan which he followed in attempting at first to study every thing, now mars his study of any one subject; he generally knows neither what he had best read, nor how he may best unite his reading with personal observation, nor, as he proceeds in reading, can he form any just idea of what progress he is making in knowledge. And so he often goes on through all his time in error; and after having worked, it may be hard and with much well-meant industry, he finds himself, when his examination comes, lamentably deficient; his labour, expended at wrong times and in wrong directions, has been expended to no profit, and he is intellectually not better than the idler. This is no imaginary picture; it is drawn from examples that we see occurring almost daily; and the evil is the necessary and unavoidable result of the present systemless schemes of medical teaching.

Now, to correct these grievous errors, the Collegiate system would be a sovereign remedy. What is wanted is, that each student should have at all times his work definitely apportioned to him, and should be instructed in the best mode of learning it. The progress he makes should be ascertained by regular examinations; not such examinations as those are which have been lately introduced into most of the courses of lectures, in which questions are asked at large to the whole class, and answered by not more than one in ten or twenty of them, but close and personal examinations, by the result of which it might be determined what amount and course of work should be set to each several student. In a word, every student should be not merely

lectured to, but *tutorized*; the personal instruction which, concentrated in the form of a short *grinding* before examination, is now mischievous, should be made useful by being distributed over the whole period of study, and by being made the guide of all the student's progress.

It is hardly to be expected that this mode of instruction, essential as we believe it to be, should be undertaken by the several lecturers. Nor, were it possible, is it certain that such a plan would be advisable: for the lecturers are of necessity guided by the schemes of education required by the diplomating bodies; and these, as we have already said, if strictly followed, must always produce evil. It would be far better that in each school there should be officers analogous to the tutors of Colleges, on whom the general management of the education of all the students should devolve; whose business it should be to ascertain the state of knowledge of each, and thereby to decide for him what should be his future course and method of study. The same persons, in the full establishment of the Collegiate system, should have the immediate superintendence of the College, both in respect of discipline and of domestic arrangements; in a word, they should hold as nearly as might be the same offices as College tutors, and to them the students should be taught to look as to their immediate preceptors.

That some of our medical schools will soon become Colleges we can scarcely doubt. It will be no small honour to the one in which the system is first fully adopted; and it is very certain that, if adopted in one, the same course must be speedily followed by the rest, if they desire to retain their pupils; for the advantages will be such that it will not be easy to find any thing that will compensate for their absence.

CLINICAL LECTURE,

Delivered at University College Hospital,

BY SAMUEL COOPER, ESQ.

Senior Surgeon to the Hospital, &c.

GENTLEMEN,—I invite your attention to two examples of injury of the elbow at present in the hospital; one, attended with circumstances perhaps absolutely preventive of a complete cure; the other, generally sure to have a favourable termination under proper treatment.

CASE I.—*Dislocation of the ulna from the humerus upwards and outwards, with fracture or separation of the coronoid process. The case of three months' standing. Division of the tendon of the triceps.*

Lucy Clayton, æt. 14, admitted March 16, 1841. Last Christmas-day she slipped and fell down on the ice, and hurt her elbow, though her hand first came against the ice, in the attempt, which she had made, to save herself from harm. Directly after the accident she was unable to bend the arm, which was fixed almost in the extended position. At the same time, a good deal of pain was complained of in the shoulder, and the forearm was benumbed. The elbow, which was much deformed, soon swelled considerably. Five hours after the accident, a surgeon, who saw the case, pronounced it to be a dislocation, and, after making extension and counter-extension, he applied a lotion of vinegar and water.

On her admission into this hospital, about twelve weeks after the occurrence, the joint presented a very deformed appearance, and its mobility was but slight. The inner condyle made a remarkable prominence, between which and the displaced ulna, an unusual depression was perceptible. The radius rotated freely. The point of the olecranon, which projected upwards and backwards, was about half an inch further from the inner condyle, than the corresponding point of bone in the other arm was from the same part of the humerus in that limb. Whenever an attempt was made to bend the arm, the triceps was rendered exceedingly tense. In front, a firm substance, which could be moved about, was felt; and concluded to be the coronoid process detached from the rest of the ulna.

March 18.—Attempt made to bring the ulna into a better position. After extension and counter-extension had been maintained for some time, I forcibly bent the arm over my knee. Mr. Quain then repeated the same proceedings, with the view of bringing the ulna into its proper position, and immediately afterwards the girl was able to put

her hand up to her forehead, which she had not been able to do since the accident. This amendment, however, did not continue in the degree here described. Hence, for several weeks, a mechanical apparatus has been employed for the purpose of maintaining and gradually increasing the flexion of the elbow. Together with the use of this instrument, we have now begun to combine the exercise of passive motion for a certain time almost every day. At present the girl undoubtedly has more use of her arm, than when she was first admitted; yet, her own power of bending the elbow, it must be confessed, is very limited, and, if passive motion of it were not still rigorously followed up for some time, I should be apprehensive of an ankylosis between the ulna and the humerus.

When the ulna is displaced backwards, and the coronoid process is broken off, or detached in a young subject from the rest of the bone, you know perfectly well that the maintenance of the reduction is much more difficult, than when no such complication exists.

In Sir Astley Cooper's work on Dislocations, I find the following observations under the head of Fracture of the Coronoid Process of the Ulna; and they are interesting in relation to the accident now engaging our attention. A gentleman, after falling upon his hand, found himself unable to bend the elbow; nor could he entirely straighten it. The same things happened in Lucy Clayton.

"He applied to his surgeon, who, upon examination, found that the ulna projected considerably backwards; but that, as soon as he bent the arm, it resumed its natural form. He immediately confined the limb in a splint, and kept it in a sling." When Sir Astley Cooper saw this patient in town, several months had elapsed since the accident; yet the same appearances, which the surgeon described as presenting themselves when he first saw the injury, still remained; namely, the ulna projected backwards whilst the arm was extended; but it could be drawn forwards and bent, without much difficulty, and the deformity was then removed.

Sir Astley Cooper had been for some years accustomed to mention this case in his lectures, when a subject was brought to the dissecting room, who had met with the same accident. The coronoid process, which had been broken off within the joint, had united only by ligament, so as to move readily upon the ulna, and thus the sigmoid cavity was so altered, that in extension the ulna glided backwards on the humerus. Sir Astley Cooper doubted, whether any treatment of such an accident would perfectly succeed, because the coronoid process does not admit of bony union. He recommended, however, the arm to be kept steadily in the bent position for three weeks after the injury, so as

to render the ligamentous union as short as possible.

As in our case the triceps was remarked to become exceedingly tense, whenever an attempt was made to bend the arm, Mr. Quain decided to try what benefit would result from dividing the tendon of that muscle, which operation was performed about three weeks before the girl left the hospital, during most of which time the instrument was also worn a part of the day for the purpose of keeping the limb bent. This treatment was certainly productive of some good, though the power of bending the elbow is yet very limited. She has been directed to continue the use of the splint for some hours every day, in order gradually to increase the flexion of the limb by means of the screw; and to let the joint be bent and extended by other persons for twenty minutes or half an hour daily, when the instrument is not on the limb. I think that, in this manner, she will ultimately have a good use of the arm.

CASE II.—*Fracture of the internal condyle of the humerus.*

Robert Fisher, æt. 12, admitted March 25th. In running across the street, his foot touched the curb-stone, and he fell with his elbow against the pavement.

On his arrival at the hospital the elbow was found to be exceedingly painful and much swollen—flexion and extension could be performed, but not without severe pain. The olecranon did not project backwards more than natural. Pronation and supination could be performed without assistance; and scarcely any deformity was observable except the swelling. On making pressure on the internal condyle, a crepitus was felt. This process was drawn downwards, and was not on the same level with the external condyle. The styloid processes of the radius and ulna bore their normal relative position to each other.

Fomentations were at first applied.

29th.—The swelling and pain are diminished; but considerable ecchymosis is seen.

31st.—The ecchymosis yet continues; pain is felt in the hand, and in the ring and little finger, especially when the arm is extended.

April 1st.—This pain is yet experienced. Two lateral angular splints have been applied to the limb, which is kept bent at right angles.

10th.—Splints taken off and reapplied. Fractured condyle in good position. The swelling and ecchymosis have subsided. Flexion of the arm causes pain.

22d.—Less pain felt on moving the joint. Still some pain in the hand, the ring, and little finger.

May 4th.—Splints readjusted. Patient discharged.

The particulars of this case make us acquainted with one symptom of a fracture of the inner condyle, which, though from anatomical considerations it might be expected generally to attend such an accident, has not, I believe, been noticed by surgical writers. I allude to the pain felt by the patient in the ring and little fingers, and in the palm of the hand, evidently arising from dilatation or disturbance of that part of the ulnar nerve which lies between the olecranon and internal condyle. You know that when a nerve is irritated, or disturbed in its course, the principal pain is usually felt in the ultimate divisions of it. Thus, in a case of popliteal aneurism, the pressure of the tumor on the popliteal nerve usually causes the greatest degree of pain in the parts to which the external and internal plantar nerves are distributed; and in a fracture of the inner condyle of the humerus, if the displacement of the fragment irritated or disturbed the trunk of the ulnar nerve behind it, the pain would be chiefly experienced in the ring and little fingers, and in the palm, where its deeper branch is distributed, to some of the smaller muscles of the hand.

From these accidents I will proceed to notice two cases of a different nature.

CASE III.—*Syphilitic Iritis.*

About two years ago, William Tomsett, aged 22, the subject of this disease, contracted some venereal complaints, which were treated with mercury. From the report it seems that, in 1839, he had a chancre and warts. The chancre was followed in about a fortnight by a bubo in the right groin, and, after this had been cured, another swelling formed in the left groin. The patient was in the Lock Hospital seven weeks, and when he left it, all his complaints had yielded to mercury, with the exception of the warts, which grew again after having been cut off.

He was attended at University College Hospital first as an out-patient for a glandular enlargement and an abscess under the lower jaw, from which a table-spoonful of matter had been discharged by puncture. The part was then poulticed, and the iodide of potassium prescribed.

This complaint was shortly afterwards followed by an attack of iritis, for which he was admitted into the hospital.

At this period the pink vessels of iritis, always observable in the early stage, were perceived running forwards in the sclerotic coat, parallel to each other, and also enlarged vessels in the conjunctiva, containing blood of a darker colour, and presenting a reticulated arrangement. The red zone, one of the most prominent symptoms of iritis, was conspicuous, around the margin of the cornea. The pupil was somewhat contracted, irregular, and drawn towards the inner can-

thus. The iris was moveable, but chiefly at its upper portion. Fibrine of a brown colour was seen deposited in irregular masses on the lower part of the iris, which had become deprived of its natural colour and brilliancy. The patient was experiencing deep-seated throbbing pain in the eye and around the orbit, which was particularly severe in the night. The patient betrayed great intolerance of light. Two small opaque specks on the cornea were noticed—the effect of inflammation of the eye in his infancy from small-pox. On the penis no sore was perceptible; merely a few warts behind the corona glandis.

The pulse did not exceed 64; the skin was dry; tongue rather furred; patient thirsty, but his appetite good.

March 25th.—The treatment was commenced with the application of leeches to the right temple; the exhibition of three grains of calomel and half a grain of Pulv. Opii every four hours; and the application of the moistened extract of belladonna to the integuments of the temple, eyebrow, and eyelids.

26th.—The calomel pills and the belladonna were continued, with a draught of the Liq. Ammon. Acet. ʒij., Aq. ʒij. et Antim. Potassio-tartratis gr. ¼, tertia quaque hora.

27th.—Still a good deal of conjunctival and sclerotic inflammation: the red zone around the cornea has not disappeared; the pupil is very irregular, but the angular deformity of it is diminished; the margin of the iris is thick; the deposition of fibrine, however, is not increased. Intolerance of light greater, and the nocturnal pain in the eye and around the orbit, and at the back of the head, is worse, sleep being entirely prevented by its nightly exacerbations. The mixture causes sickness and purging; the gums are very sore; the skin rather dry; the pulse 80, and sharp; the tongue furred.

Belladonna applied again. C. C. tempori dextro ad ʒx. Mixture, pills, and lotion, continued.

28th.—The following points of improvement may now be remarked:—Intolerance of light less; objects more plainly discerned; circumorbital and occipital pain quite gone; pain in eye itself felt only when the light first strikes it.

Tongue foul; bowels open; pulse 88, compressible; mixture no longer produces sickness; gums very sore.

Pills to be discontinued, and mouth washed with the alum and myrrh gargle. Mixture and Belladonna to be continued.

29th.—Cornea clearer. The deposition of fibrine not increased. The iris becoming more of its natural colour. The angular deformity

has quite disappeared. The pupil is contractile, especially at the upper part. Pain less; though the conjunctiva and sclerotica are still far from being free from inflammation; pulse 96.

Mixture, gargle, and lotion, continued; Belladonna to be re-applied.

30th.—Vast improvement; scarcely any intolerance of light; vessels less injected; the iris assuming its natural colour; vision clearer; pupil obedient to the light. The red zone of vessels around the cornea, still perceptible; tongue covered with a yellow fur; pulse 92; some nausea, and pain in the epigastrium, occasioned by the mixture, which was therefore omitted.

Hydrarg. c. Creta, gr. v. o. n. Belladonna to be reapplied, and the Lotion and Gargle used.

31st.—Red zone around the cornea is of a paler colour, and some of the fibrine on the margin of the iris has been absorbed. The remaining portion of it, when examined with a pocket lens, appears vascular. A little pain felt in the right temple; but none in the eye. Vision improved, and inflammation abated. Some tenderness in the epigastrium. Pulse 88, and soft. Same means continued.

April 1st.—Circumorbital pain quite gone. Still a little intolerance of light. Red zone disappearing. Inflammation of conjunctiva and sclerotica diminished. Bowels confined. Gums very sore. Pulse 100.

Dose of Senna Mixture prescribed. Powder, lotion, gargle, and belladonna, continued.

2d.—Effused fibrine nearly all absorbed. No intolerance of light. No pain in the eye, nor around the orbit. Bowels have been freely opened. Pulse 88, soft.

Same plan continued.

3d.—The pupil greatly dilated by the belladonna, and its margin irregular. Vision yet imperfect. Pulse 68. Bowels open.

Belladonna discontinued.

4th.—Pupil still dilated. Vision much improved.

5th.—Gradual amendment. Gums yet very sore.

Pulv. Hydr. c. Creta gr. ijss., instead of v., o. n.

Pulse 96.

6th.—Patient much better. Still some discolouration at inner margin of the iris; some redness of the conjunctiva; and a degree of pain around the orbit, especially at night. Slight aversion to the light yet experienced. Gums less sore; tongue very foul; bowels open; pulse compressible.

7th.—Pinkish tinge in the conjunctiva yet manifest; the iris irregularly contractile; some pain in the right temple and back of

the head at night; bowels open; pulse rather full.

8th.—On account of the irregularity of the iris, the belladonna was re-applied.

Powders and gargle continued.

14th.—By this time the irregularity of the iris was lessened. No pain in temple and back of the head. No intolerance of light. Some indistinctness of vision. Gums not sore. Tongue clean. Pulse 92.

Belladonna used.

15th.—Sight less clear to-day; gums rather sore; bowels open.

Eye bathed with warm water; belladonna not employed.

16.—Iris yet irregular; bowels confined; house medicine prescribed; powder and gargle continued. Eye weak. Vinum Opii to be dropped into the eye.

17th.—Great improvement; sight better; no pain.

22d.—Slight circumorbital pain last night; iris a little irregular, but now contracts at its lower part; sight much clearer.

Vinum Opii to be continued.

24th.—Vinum Opii used again.

R Potassii Iodid. gr. iss.; Aq. ʒj.; ter die sumend.

26th.—Iris now moves under the influence of the light; bowels open; tongue clean.

Vinum Opii and Iodide of Potassium continued.

May 1st.—Pupil dilated from the effect of belladonna.

Vinum Opii used.

When I saw this patient last week, his eye was well; papular eruption had come out, but was dying away.

This case is full of instruction, being one of the best instances of a syphilitic iritis that has occurred in this hospital since its first establishment.

You know that an iritis is concluded to be venereal partly from the history of the case, and partly from the peculiar appearances of the iris and pupil.

Here the history informs us, that the patient, previously to the attack of the iritis, had been under treatment for different syphilitic complaints, especially chancre and bubo, for which mercury had been used. Then, just as the iritis was being cured, a papular eruption came out; one of the secondary symptoms of syphilis.

The iritis then was preceded and accompanied with other venereal symptoms; a fact sufficient to justify the suspicion of the connection of this affection of the eye with syphilis.

The suspicion may next be said to have been freed from all doubt, by the special appearances of the eye itself, particularly by

the formation of masses of fibrine in the shape of brownish or reddish-brown tubercles upon the iris; and by a tinge of the same hue about the pupillary margin of the iris. The inclination also of the pupil towards the inner commissure of the eye, or bridge of the nose—a circumstance much dwelt upon by Beer and others, as peculiar to syphilitic iritis—was exemplified in this patient. You must not, however, always expect to meet with it; and a case may be a syphilitic iritis without it. When it does occur, it tends, with other symptoms and the history, to throw light on the nature of the disease.

The particulars recorded of our patient furnish a good description of the symptoms of the venereal iritis in general, viz. the thickened state of the iris; the irregularity of its pupillary margin; the severity of the nocturnal exacerbations of pain in the eye, around the orbit, and at the occiput.

The case also shews the possibility by effectual treatment of dispersing the effused fibrine, even after vessels had been discerned in it with a magnifying glass. The whole tenor of the case shews the long tendency of the complaint to relapse, and the obstinate weakness of the eye consequent to it. It further proves the decided efficacy of a combination of antiphlogistic means with mercury and belladonna; the power of mercury in dispersing the effused fibrine, or the reddish-brown tubercles of lymph; and the usefulness of belladonna in keeping the pupil dilated, and the iris away from the capsule of the lens.

CASE IV.—*Wound of the Brachial Artery. Apoplexy.*

Matthew West, æt. 49; admitted under Mr. Quain, April 8, 1841.

On returning home this morning, he was seen leaning against a lamp-post, and was unable to answer any questions; but, from papers in his pocket, the place of his residence was ascertained, and he was conveyed home in a cab.

A surgeon was sent for, who attempted first to bleed him in the cephalic vein, and then in the median basilic, in doing which the brachial artery was penetrated. A tourniquet was applied, and the patient brought to the hospital. In consultation with Mr. Quain, it was decided, that, as the opening in the artery was large, and the limb paralytic, it would be better in this case not to try pressure, but to tie the wounded part of the artery without delay. While pressure was made on the artery in the middle of the arm by an assistant, Mr. Quain, therefore, enlarged the wound upwards and downwards, and outwards. The fascia sent off from the tendon of the biceps having then been divided, the wound in the artery was exposed, and a ligature applied above and below it.

The wound was then covered with the water dressing, and the patient put to bed.

He had lost about 1bjss. or lbij. of blood from the accident.

April 13th.—Died this evening, five days after the apoplectic attack and the operation. The particulars of the case from the period of the operation until that of the fatal termination are interesting, but, as not bearing upon surgery, I now omit them. Still I recommend the perusal of them to you, as they are recorded in the hospital book.

Some of the post-mortem appearances, however, I will notice.

Head.—On opening the dura mater about 3ij. of serous fluid escaped. The arachnoid membrane was much thickened, opaque, and studded with numerous little tubercles of lymph, over the superior part of the hemispheres.

Under the arachnoid membrane there was also a copious effusion of serum. The arachnoid and the pia mater between the hemispheres were more vascular than natural.

In the right ventricle bloody serum was observed. In the left, a large coagulum was found, occupying all the surface of the thalamus opticus and corpus striatum, which parts were dissolved into a soft bloody pulpy mass.

The arteries at the base of the brain were thickened, but not ossified.

The chief points of instruction afforded by this case relate—

1st, To the necessity of using great caution whenever you attempt to bleed in the median basilic vein, because, if the patient be in a fit at the time, drunk, or from any cause disposed to throw his arm about suddenly and violently, the artery will be endangered. I should say then, that, in such circumstance, prefer the median cephalic vein; but if you cannot get blood from it, let the patient's arm be well fixed at all events, feel for the pulsation of the artery, and make the opening in a part of the median basilic vein not immediately over it. The great Dupuytren had seen so many accidents arise from bleeding in the median basilic vein, that, as his lectures will convince, he had a kind of timidity about bleeding in this vessel. With the precautions, which I have offered, however, it may be performed with sufficient safety.

2dly, This case should impress upon your memories the expediency and correctness of the great practical rule applicable to a recently wounded arterial trunk requiring to be tied; namely, always to expose, if possible, by an operation, the wounded portion of the artery, and to apply one ligature above, and another below the orifice in the vessel. Thus all risk of hæmorrhage from the freedom of the anastomosis is prevented.

3dly, If our reasoning and management of this case were right, you should prefer the ligature to compression, when the artery has been extensively opened, and the limb paralytic; because the first circumstance would make the healing of the arterial wound less likely to succeed; and the second would render the limb less able to bear compression, without being followed by gangrene.

UNIVERSITY OF EDINBURGH.

GRADUATION OF PHYSICIANS.

ON Monday, August 2d, the annual graduation of Doctors in Medicine took place at this University, when one hundred and three gentlemen received that degree. The following is a list of their names. Such of the gentlemen whose Theses received honourable mention have a single asterisk (*) appended to their names; those selected for competition for prizes have two (**); and to those who obtained prizes three asterisks (***) are affixed:—

Of Scotland.—Kenneth M'Kenzie Adams.—**Thomas Anderson.—Hugh T. S. Beveridge.—William J. Carlyle.—Robert Cassels.—Hugh F. C. Cleghorn.—Thomas Cossar.—Alexander Cumming.—James Dewar.—**Walter Dickson.—Robert E. Dudgeon.—**George Duff.—Henry Dunbar.—*James M. Ferguson.—James G. Fraser.—Huntly G. Gordon.—James Grant.—Enoch N. Houston.—John Hunter.—Keith Jopp.—George S. Keith.—*James Kirkpatrick.—*Alexander Kirkwood.—Thomas Lookup.—John M'Cosh.—George Mackay.—*William Macleod.—William S. Pringle.—***Francis Reid.—*Henry Richardson.—John T. Sanderson.—*Walter Scott.—William Smith.—James Steel.—*William Stuart.—**Robert Taylor.—William M. Turnbull.—David Wilson.—*Charles A. Winchester.—*David Wyllie.

From England.—William H. Ashley.—*William Bates.—William Bowie.—***H. G. Bull.—*William Catlett.—*William Dalton.—**Richard G. Daunt.—William M. M. Fosbroke.—Richard Giles.—***John Graham.—James Harrison.—Edw. Haward.—Henry Holmes.—Thomas Holt.—Henry Homer.—George Horniblow.—William R. James.—Albert de Mierre.—*James Miller.—Robert Russell.—*Edward H. Sieveking.—**John E. Stephens.—*Robert Stevenson.—*John Underwood.—William Wood.

From Ireland.—*Alexander Armstrong.—John Arthur.—Joseph Beattie.—*Ethelbert H. Blake.—John J. Bowie.—Joshua Bull.—Robert V. George.—Alexander Gordon.—John Harrison.—John Henderson.—Edward Moorhead.—***Goodwin R. P.

O'Leary. — Thomas Osburne. — John T. Robinson. — **Thomas W. Smith. — *Robert Synnot. — Hastings Twiss. — Alexander Wark. — James Young.

From Ceylon. — *James G. Atkinson. — Edward F. Kelaart.

From Jamaica. — John E. Bird.

From Barbadoes. — *Francis G. Browne. — John F. Greenidge. — James Manning.

From Nova Scotia. — James R. de Wolfe. — Simon Fitch. — David Gordon.

From St. Vincent's. — William Huggins.

From New Brunswick. — George M. Odell.

From America. — James M. Reynolds.

From the East Indies. — John E. Freeman.

— *William Fasken. — Thomas G. Scot.

From Gibraltar. — Waldegrave R. Thompson.

From Italy. — Joseph Giglioli.

From Canada. — William D. Morrin.

From Poland. — *Severinus Wielobycki.

ON THE CURE OF CRETINISM.

By DR. GUGGENBUHL.

To diminish the considerable number of cretins in the narrow valleys of the Alps, an establishment has been formed for the education of children who, in their infancy, present indications of this condition. It has long been ascertained that all young cretins might be cured by transferring them from their low vallyes to the high mountains; and that at a distance of about 3000 feet above the level of the sea these unhappy creatures are no longer met with.

All the midwives of the narrow valleys easily recognise cretinism at the time of birth: they know that every child born with a stupid expression of face, uttering peculiar cries, and making strange movements, is sure to be a cretin. When such an one is born, a report will, in future, be made to the civil authorities, that the child may be taken to the national institution, which is now founded on a mountain in Berner Oberland, and in which all the necessary arrangements are taken to secure the complete physical, intellectual, and moral development of the young children. — *Gazette Médicale*, Mai 8, 1841, from *Haeser's Archiv für die Gesamnte Medicin*.

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Friday, July 30, 1841.

E. Dunn. — W. Potter. — R. Walton. — G. Tranter. — J. A. Lacking. — W. T. Borthwick. — A. J. Little. — W. Druitt. — G. Y. Hood. — W. G. Marshall.

Friday, August 6.

J. Wilson. — L. J. Monteith. — A. Gordon. — W. A. Loy. — J. Walmsley. — C. M'Shane. — J. Atkin. — S. P. Chennell. — G. Lacon. — R. Dawson. — A. Allcock.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, August 5, 1841.

G. C. H. Wigan, Portishead. — E. Hoskins, Derbyshire. — J. Buxton, Denmark Hill, Camberwell, Surrey. — L. D. Smyth, Bingham. — F. H. Kelson, Bath, Somerset.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 31st July, 1841.

Small Pox	14
Measles	24
Scarlatina	8
Whooping Cough	31
Croup	3
Thrush	6
Diarrhœa	11
Dysentery	1
Cholera	0
Influenza	2
Typhus	21
Erysipelas	10
Syphilis	1
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	156
Diseases of the Lungs, and other Organs of Respiration	233
Diseases of the Heart and Blood-vessels	15
Diseases of the Stomach, Liver, and other Organs of Digestion	71
Diseases of the Kidneys, &c.	8
Childbed	8
Ovarian Dropsy	1
Diseases of Uterus, &c.	0
Rheumatism	3
Diseases of Joints, &c.	1
Ulcer	1
Fistula	0
Diseases of Skin, &c.	1
Diseases of Uncertain Seat	81
Old Age or Natural Decay	52
Deaths by Violence, Privation, or Intemperance	14
Causes not specified	1

Deaths from all Causes 778

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 6° 3' 51" W. of Greenwich.

August.		THERMOMETER.	BAROMETER.
Wednesday	4	from 56 to 67	29.33 to 29.75
Thursday	5	57 65	29.71 29.58
Friday	6	56 67	29.59 29.79
Saturday	7	56 74	29.92 29.79
Sunday	8	57 67	29.68 29.60
Monday	9	52 67	29.59 29.65
Tuesday	10	48 65	29.72 29.76

Prevailing wind, S.W.

On the 4th, afternoon clear, otherwise cloudy, with rain. The 5th, cloudy, frequent showers of rain, with boisterous wind during the day. The 6th, morning cloudy, afternoon clear, evening cloudy, with rain. The 7th, clear. The 8th, generally cloudy, rain at times. The 9th, generally clear, raining fast between six and eight, p.m. The 10th, evening cloudy, with rain, otherwise clear.

Rain fallen, .585 of an inch.
CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

FRIDAY, AUGUST 20, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XLVI.

Cynanche Trachealis: Symptoms: Pathology: Prognosis: Treatment. Child-crowing, or spurious croup.

I PROCEED this afternoon to another of Dr. Cullen's species of cynanche; the last that I propose to consider in this part of the course: viz. *cynanche trachealis*—*tracheitis*—*croup*.

The essence of this complaint is violent inflammation, affecting the mucous membrane of that portion of the air-passages which lies between the laryngeal cartilages and the primary bronchi; in one word, of the trachea, or *wind-pipe*. That is the genuine seat of the disease: but the inflammation sometimes ascends into the larynx; and not unfrequently it dives into the bronchi and into their ramifications.

Cullen makes no distinction between *cynanche trachealis* and *cynanche laryngea*. Yet they are separated from each other by very definite boundaries. They differ in anatomical position: they differ in gravity. Both indeed are serious diseases, but croup is the more serious, because it seldom admits of that mechanical relief which, when rendered in time, deprives *cynanche laryngea* of its dangerous character. The two disorders differ also in respect to the period of life at which they occur. Idiopathic laryngitis is seldom met with except in adults; croup seldom after the age of puberty. *Cynanche trachealis* is indeed a very remarkable disease, for it exhibits an event of inflammation which does not usually be-

long to that process when it affects the *mucous* tissues. In this too it differs from laryngitis.

I say that croup is peculiarly a disease of early life. The interval that lies between the two periods of *weaning* and *puberty*, is the time during which its visitation is chiefly to be apprehended. Comparatively few cases of it occur during the first year of infantile life. There are more in the second year than in any other. This, in all probability, is connected with the change that ensues in regard to diet, upon the child's being weaned. Dr. Cheyne, whose experience of croup was very extensive, says, that the younger children are when weaned, the more liable are they, *cæteris paribus*, to this malady. From the second year onwards the number of children affected with croup gradually decreases. Of ninety-one instances referred to by Jurin, one only was after the tenth year. But it does occasionally happen subsequently to the period of puberty, and up to the twenty-first year; and even later. A girl of nineteen, one of my hospital patients, died of it. Some of the cases recorded of croup in the adult were probably in reality cases of laryngitis.

It is curious that inflammation should thus, at different epochs of life, fix itself upon limited portions of the same continuous surface, and give rise to consequences so diverse. We are unable to give any account of this.

Symptoms.—*Cynanche trachealis* is frequently preceded by a slighter and more diffused affection of the membrane lining the air-passages. The child has what is popularly called a cold; sneezes, coughs, and is *hoarse*. Now with respect to this last symptom Dr. Cheyne makes the following practical remark. Hoarseness (he says) in very young children, does not usually attend common catarrh. When noticed in a district where croup is not unfrequent, it ought to put the parents or the medical attendant of the child upon their guard;

especially as much depends upon the early treatment of the disorder. With these symptoms the child is feverish and fretful, and does not sleep well. In the course of a day or two the signs peculiar to croup begin to show themselves: they are well stated by Cullen, in his definition of the complaint. "*Cynanche, respiratio difficilis, inspiratione strepente, voce raucâ, tussi clangosâ, tumore fere nullo in faucibus apparente, deglutitione parum difficili, cum febre synochâ.*" These are the phænomena that characterize croup. Difficulty of breathing; and sonorous inspiration. This last is often almost enough, of itself, to identify the disease. Hoarseness; a gruff voice; sometimes a total loss of the power of vocal speech. A very peculiar and distinctive cough, to which the epithet "brassy" has been justly applied; the noise resembling that which would be occasioned by coughing through a brazen trumpet. This remarkable sound is always easily recognized when it has been once heard. It is a *ringing* cough; and the expiration has a ringing character; and either of these, the cough or the expiration, is followed by a loud *crowing* inspiration. Then there is the negative symptom; the absence of any difficulty of swallowing: and with all this, inflammatory fever; a flushed face, hot skin, a frequent hard pulse, thirst.

However, it is by taking the symptoms collectively that we judge of the existence of croup, and by the rapid progress of the disease; rather than by any particular or pathognomonic sign. Some of the symptoms may occur, separately, when there is no croup. The brassy or metallic cough, for instance, has been known to accompany some chronic affections of the larynx. Dr. Gregory—the late Edinburgh Professor of Physic—knew a man with a venereal disease of the throat who coughed so exactly the cough of croup, that he was admitted into the clinical wards of the infirmary every session for some years, that the students might have the opportunity of *hearing* this peculiar sound. So also the remarkable crowing inspiration may take place, as we shall soon see, without croup.

In the outset the fever generally runs high; and it is of importance, as respects the diagnosis, to mark the presence or the absence of pyrexia. As the obstruction to the passage of air increases, the blood ceases to be duly arterialized; and then, of course, the skin grows dusky, the pulse feeble and irregular, the extremities cold. The cough also, as the malady thus goes on from bad to worse, ceases to be loud and clanging, becomes husky, and inaudible at a short distance, and the voice sinks into a whisper; the head is thrown back; the nostrils, in perpetual motion, dilate widely; the face is pale and livid, and sometimes

bloated; the pupils often expand. When these indications of sinking have come on, the case usually terminates ill: the bottoms of the feet turn black and hard; drowsiness supervenes; some tossing of the arms perhaps; the breathing becomes gasping and interrupted, and the child dies after an *inspiration*.

In other cases the croupy symptoms make their attack very suddenly. A child shall go to bed apparently well, and in the course of the night have all the worst signs of the disease. And it is observable, that whether the attack be altogether unexpected, or whether it has been preceded by hoarseness, sore-throat, and catarrh, it usually comes on *in the night*.

Croup resembles laryngitis in this respect, that it runs its course rapidly: proving fatal sometimes within twenty-four, and often within forty-eight hours. It may, however, continue for five or six days before it terminates, whether death or recovery be the result. Dr. Craigie affirms that it is never protracted beyond the *eleventh* day; the fatal or the favourable issue having always taken place by that time. Life is destroyed, in pure circumscribed tracheitis, by the accumulation in the wind-pipe of a concrete membrane-like substance, which so frequently attends this disease, and is so peculiar to it, that it is called *the membrane of croup*. In cases of recovery this substance has been expectorated in the form of a nearly perfect tube, representing a cast of the trachea; at other times it is coughed up in flat or tubular fragments. In fatal cases it is found sometimes lying in close contact with the mucous membrane, and sometimes quite detached from it: so that it might have been expelled without much force or difficulty, if the child could have sufficiently inflated its lungs, and the requisite muscular power had remained, and spasmodic irritability of the glottis had not opposed.

But, in many instances, this albuminous exudation is not confined to the trachea. It often stretches down to, and enters, the ramifications of the bronchi, and reaches even to their termination in the pulmonary vesicles. Sometimes also it is found clothing the mucous membrane belonging to the laryngeal cartilages. This Dr. Craigie denies. But I shew you two specimens in which the false membrane, besides filling the trachea, evidently extends into the larynx. One of these comes from the museum upstairs: the other I have borrowed from the Middlesex Hospital museum: it is the larynx and trachea of the young woman whom I mentioned just now as having died there of croup at the age of nineteen. The false membrane reached from the tip of the epiglottis to the bifurcation of the trachea. You see the same thing represented in this excellent plate

of Dr. Carswell's, pictured from nature. Usually the adventitious membrane commences just below the larynx, where it is thin and soft: about the middle of the windpipe it is more dense and firm; lower down in the trachea, and in the bronchi, it is generally looser again, pulpy, and broken: it sometimes, I repeat, penetrates to the very air-cells. What are called (absurdly enough) bronchial polypi, branch-like casts of the smaller ramifications of the air-tubes, are then apt to be coughed up. And even when this concrete substance is not formed, we have other evidence, often, of the extension of the inflammation throughout the whole downward course of the membrane.

On the other hand, there are a few cases in which this adventitious membrane is not formed at all; the inner surface of the windpipe is seen to be merely reddened and tumid, and covered with viscid mucus; or perhaps with a shred or two of concrete albumen here and there.

The difficulty of breathing, and the characteristic sounds that accompany it, depend, in part, I believe, upon spasmodic contractions of the small muscles of the larynx: for remarkable aggravations of the dyspnoea are apt to occur, and to subside again; and these aggravations are sometimes brought on by sudden causes—by the movements of deglutition, for example.

I shall have to recur to this spasmodic constriction of the glottis: but I may here remark, that because it has not been (and cannot be) *seen*, doubts have been expressed by some about its having any thing to do with the dyspnoea. Such doubts seem scarcely reasonable. It is easy, at any time, by an effort of the will, to close the glottis, and to prevent the passage of air to and from the lungs. This is mainly effected by the action of the little muscles that bring together the arytenoid cartilages. But those muscles, like the other muscles concerned in respiration, act also independently of the will, spasmodically therefore, through the reflex function of the spinal cord. And it is by a providential and conservative arrangement that they do so act, as janitors, admitting, in the healthy state, the vivifying air, but barring the door against certain hurtful gases, and against solids and liquids which would be injurious to the respiratory apparatus. We know that if a drop of water, or a crumb of bread, or a whiff of carbonic acid gas, gets past the outer defence, the epiglottis, and into the larynx, spasmodic action of the little muscles in question is instantly excited. We cannot see these intruders, and voluntarily resist their entrance, but the unsleeping sentinel is there to guard the passage. We may well conceive, therefore (and I know not how the supposition can be disproved), that the

noisy and difficult respiration of croup may be caused, in part, by spasm.

The presence of the adventitious tubular membrane in the trachea affords a plausible explanation of one of the symptoms observed in these little patients; the tendency they shew to throw the head back. The cylinder of membrane is kept open in that position; whereas, if the head were inclined at all towards the chest, the membrane would be bent upon itself, and the passage through it obstructed.

This concrete exudation is often adduced to prove that the mucous membrane may exhibit, under certain circumstances, the phenomena of adhesive inflammation. Similar films sometimes form upon, or are thrown off by, the mucous surfaces of the intestines, and of the uterus. Whether they are to be regarded as essentially identical with the layers of coagulable lymph poured forth in inflammation of the serous and cellular tissues, may be made a question. There are certainly some strong points of distinction between them. The concrete membrane of croup is more brittle, less fibrous, more decidedly albuminous, than the false membranes that cover the inflamed pleura, pericardium, or peritoneum. A still more remarkable difference is this, that it is not *plastic*, in the sense in which that term was formerly explained; it never becomes organized, never connects itself by blood-vessels with the surface from which it proceeds. On the contrary, it is partially detached; and by degrees, if the patient live long enough, it is completely separated from the subjacent parts.

Hypotheses have been framed to account for the limitation of this product of tracheal inflammation to the early periods of life; and for its variation from the usual products of inflammation of the same part. Dr. Stokes thinks that the predominance of the white tissues in young subjects may explain the greater frequency of croup, with its peculiar membrane, in infants. Dr. C. B. Williams starts the very reasonable supposition that the inflammation involves the submucous cellular tissue, which is abundant during youth; and that the natural product of this phlegmonous inflammation transudes readily through the thin, simple, and delicate mucous membrane proper to that age.

The formation of this adventitious membrane, and even its renewal, appears to be sometimes very rapid. I have here a preparation made by the late Dr. Sweatman, illustrative of this. It now belongs to the Middlesex Hospital Museum. Upon a child, on the very brink of suffocation from croup, the operation of tracheotomy was performed, at one o'clock in the morning, by Mr. Chevalier. A tubular portion of membrane, of the shape and size of the trachea,

was presently forced through the artificial opening. Immediately the child's respiration became easy, and it fell asleep. In the course of the same morning, Dr. Sweatman was hastily summoned, and arriving at eight o'clock, found the child dead. It had slept six hours, and upwards; and then the distress of the breathing had returned, and was soon fatal. The trachea was found to contain a new tube of lymph, or of concrete albumen. The preparation shews that such a membrane may re-form in that short space of time; namely, in from six to seven hours.

The croup is not contagious; although, like *cynanche tonsillaris*, and for the same reasons, it is found sometimes existing at the same time, or in quick succession, in more than one child of the same family. Thus two twin children of Dr. Gregory's were seized with croup on the same night. They had both been walking in the evening on the sea-shore during a cold wind. This is in accordance with what Dr. Cheyne has stated, that the attack is almost always nocturnal, and often when the child has during the preceding day been exposed to the weather. It frequently occurs sporadically; but there are places in which the disease appears to be endemic. Dr. Cheyne found it so on the coast of the Frith of Forth. Indeed, the first distinct account of it that we possess was drawn up by Dr. Home, of Edinburgh, in 1765, from much personal observation of its ravages in Leith and Musselburgh. Cold situations, and damp places, more than such as are merely cold, are subject to the prevalence of this disease. It is accordingly frequent in the seasons of winter and spring. It is said to be most common near the sea-shore, and in the neighbourhood of large bodies of water generally. It occurs in low, moist, what are called in Scotland *carse* districts, more than in upland situations that are more exposed to cold winds. It is more common at Leith than it is in Edinburgh; and in Edinburgh, it is most frequent in the lowest parts of the town. This I learned from Dr. Alison, who, having long been physician to a dispensary there, had had ample means of observing the disease. Canal Street, and the Cowgate, both low spots, as some of you may know, have long been famous, or rather infamous, for cases of croup. Towns situated on the banks of rivers are more than commonly visited by it; and it has been observed to be particularly frequent among the children of washer-women in such places; and thus evidently connected with exposure to moisture. It has been known to prevail epidemically in towns so situated, after an inundation. And Dr. Alison has made a very curious remark respecting it. He says that it seems to be often produced by the child's sitting, or sleeping, in a room newly

washed: and that he has frequently known it to occur on a Saturday night—the only day in the week on which it is customary for the lower orders in Edinburgh to wash their houses.

Like *cynanche tonsillaris*, and unlike *cynanche parotidæa*, the croup is exceedingly apt to recur. Relapses may happen within a few days after apparent recovery; and these are very perilous. But besides this tendency to a renewal of the severer symptoms, the little patients are often affected with cough, and hoarseness, and even with aphonia, for a long time. And while these relics of the acute attack continue, it is easily brought back again. The first seizure is generally, I believe, the worst: but to this rule there are numerous exceptions.

Prognosis.—Croup is a disorder which justly excites extreme alarm in the friends and parents of the patient: for the prognosis can never be better than doubtful. It is said that four children out of five attacked by it used to die: but that now, the treatment being better understood than formerly, the number of deaths and the number of recoveries are nearly equal. We judge of the probable issue, in a given case, by the apparent circumstances and progress of the malady. If we could see the interior of the air-tubes, we should know that the chance of escape was small, in proportion as the inflammation, and its albuminous product, descended along the ramifications of the bronchi. But in these little patients, and amid the tracheal noises, it is difficult to ascertain the physical state of the lungs. The prognosis is chiefly to be collected from the general condition of the child. If the distress of breathing seems to remit, and free expectoration to come on, while the strength is yet entire, we venture to hope. On the other hand, we begin to despair when the lips are becoming blue, the skin is losing its heat, the pulse is already feeble and intermitting, and the little patient is drowsy or comatose: in other words, when we perceive the final symptoms of death in the way of apnoea. Some few patients die suddenly and unexpectedly without any previous coma.

Treatment.—The mortality will differ according as the disease is detected early, and treated vigorously,—or otherwise. And with respect to treatment, there is no specific remedy for this, any more than for any other inflammation. We must put in force the general principles upon which the treatment of inflammation is founded; adapting them, however, to the malady in question by those particular facts which the experience of the best observers have collected for our guidance.

I need scarcely say that where cough and catarrh, and especially hoarseness or loss of voice, are noticed in a young child, he

should be narrowly watched, and protected against all circumstances likely to excite or to aggravate inflammation: he should be kept in the house, and put upon farinaceous diet; and the functions of the bowels and of the skin should be attended to.

The three remedies that most require consideration are blood-letting, tartarized antimony, and calomel.

Bleeding is to be unhesitatingly employed when the patient is strong, and plethoric, and seen in the outset of the disease. In judging of its mode, and of its amount, we must recollect that what is no more than a topical bleeding in an adult, becomes equivalent, in its effect upon the system, to general bleeding, when it is used for a very young child. Abstraction of blood, by venesection or cupping in the case of older children, and by leeches in the case of infants, should be practised whenever the symptoms are violent, and there is much fever, and the patient is seen within a few hours after the commencement of the symptoms. The relief that is given by this measure, under such circumstances, is often so decided, that no doubt can remain of its usefulness and propriety.

It is impossible to lay down any fixed rules for the quantity of blood that should be taken in this complaint. Under two years of age, it should not, says Dr. Cheyne, exceed five ounces. I should esteem that a *large* bleeding at that age. Upon an average, a moderate bleeding will be produced by the application of a couple of leeches to an infant in its first year: and an additional leech may be employed for every additional year: so that six may be put to a child five years old; or eight if he be stout. Dr. Copland estimates the amount of blood which these patients may, with safety, bear, to be somewhat more than an ounce, or as much as an ounce and a half, for every year of their age. Much, however, must depend upon the special circumstances of the case: the quantity of blood extracted by a given number of leeches is less in one instance than in another; and then, of course, the number must be increased. They should be applied at the upper part of the sternum, and not upon the throat itself; for this reason:—that the pressure which may be necessary to stop the bleeding, or to regulate its quantity, cannot well be borne upon the throat in these cases.

After one sufficient evacuation of blood, whether by means of the lancet, or of leeches, or of cupping glasses applied between the shoulders, it will always be right, before repeating it, to ascertain the effects of other measures; such as emetics and purgatives, the beneficial operation of which in this disorder is often very remarkable.

Full vomiting sometimes affords relief so

sudden and so complete, as to lead to the persuasion that the symptoms had been principally owing to spasm. And even when the disease is unequivocally inflammation—sometimes even late, but particularly in the early part of its course—the effect of a vomit is often very striking. It promotes expectoration; and is not unfrequently followed by the expulsion of shreds of the adventitious membrane. When blood-letting is employed, it should precede the emetic; or, at any rate, it should precede the act of vomiting. Dr. Cheyne recommends that the bleeding should be practised ten minutes after the emetic has been swallowed. The loss of blood assists the operation of the emetic, and lessens the risk (which is not a fanciful one) of injurious congestion of the vessels of the head during the straining efforts of vomiting.

It is desirable, not simply to excite the act of vomiting, but to produce and to prolong a state of nausea and faintness; and so to depress and to keep down the increased action of the heart, and to keep empty the capillary vessels of the inflamed part. Now the substance best adapted to this purpose is tartarized antimony. This medicine, as I have often mentioned before, has great power over the inflammation of the mucous tissues: and there is one very great advantage belonging to it in cases of croup; namely, that children may be induced to take it without their knowing that they are taking medicine; for the solution of it has little or no taste: whereas the struggling which is often occasioned by the administration of other emetics may be the cause of much inconvenience, and even of much injury to the patient. It should be dissolved in boiling water, in the proportion of a grain to an ounce; and the cold solution given. A tea or a dessert-spoonful may be repeated every quarter of an hour till some effect is produced. When vomiting is thus excited on the very first appearance of the symptoms, and before the disease seems thoroughly formed, it sometimes puts it off; so that no other treatment remains necessary beyond the exhibition of some purgative medicine. But if this perfect relief does not ensue upon the operation of the emetic, Dr. Cheyne advises (and this is in conformity with the practice of many other persons) that a powder, consisting of two, three, or four grains of calomel, with two or three grains of James's powder, should be given at short intervals; every two or three hours for example. A dose of castor oil is to be administered occasionally, to clear the bowels. And another expedient, of great efficacy sometimes, and therefore never to be omitted, is the warm bath. This is often properly resorted to just after the act of vomiting, particularly if any tendency to perspiration is apparent. The temperature

of the water should not be lower than 98° Fahrenheit; and the child should remain in the bath for ten minutes at least. When taken out, he should be wiped dry, and put immediately into bed again. The change for the better produced by the bath is sometimes so marked and so speedy, as to strengthen the conclusion that the most distressing of the symptoms had resulted from spasm.

The usual effect of calomel thus frequently repeated is not, as in adults, that of causing salivation, but the discharge of a quantity of green faecal matter, resembling chopped spinach: and when stools of this kind begin to make their appearance, there is often a sensible mitigation of the symptoms. The green colour is a common consequence of mercury given to young children; and will occur, I believe, whatever be the disease, when the full effect of calomel as a purgative is obtained. The green matter has been found, after death, in all the intestines, small as well as large, up to the duodenum. I presume that the colour is owing to some chemical action that takes place between the calomel and the bile. We know that calomel does tinge bile green when mixed with it out of the body. It may be, however, that the calomel provokes a flow of altered bile.

Calomel, thus administered, is the purgative that has received the strongest recommendations. Its usefulness appears to have been fully borne out by the test of experience: and the well-known virtue belonging to mercury, of preventing or arresting the effusion of coagulable lymph in other textures, has formed (I conceive) one cogent reason for its adoption in this disorder, of which the chief peril results from the pouring forth of the albuminous part of the blood. But whether mercury really has the same power of controlling adhesive inflammation, when that process is set up in mucous tissues, which are so commonly exempt from it, may be questioned. On the other hand, the effect of full doses of tartar emetic in restraining active inflammation of those tissues, is well ascertained: and I should certainly make use of it in the early stages of this dangerous malady. The system can be brought to feel its decided influence with much more certainty, and in a much shorter space of time, than that of calomel: and if it fails to make a beneficial impression, it need not long interfere with the mercurial treatment. Let me quote to you the statement of Dr. Cheyne (whose experience of this disease was far ampler than mine has been) respecting the efficacy of tartarized antimony in what he calls the second stage of croup. He recommends that half a grain, dissolved in a table-spoonful of water, should be given to a child two or three years old, every half hour, till sickness and vomiting

ensue. In two hours after the last effort of vomiting, the same process is to be recommenced; and so repeated while the symptoms require it, and the strength will permit. This mode of treatment was suggested to him by the accidental observation of a particular case, in which it was remarkably successful. From that time he placed his whole reliance on that remedy in the second stage of croup; especially as he had found that blood-letting in that stage only accelerated the death of the patient. He noticed that the cases were *few* in which he had known children survive the second stage, but in *all* of these few, they recovered while using a solution of tartarized antimony. He held that no other medicine was, at that stage of the disorder, entitled to confidence. In short, he declared that tartar emetic, so given as to produce continued nausea, had been his sheet anchor, in the treatment of croup, since the year 1799. This was written in 1801, in a separate work on the pathology of the larynx and bronchi; and Dr. Cheyne recently has affirmed, in the *Cyclopædia of Practical Medicine*, that he still found reason to adhere to the same opinions and the same practice.

Now what is good for the second stage, would, *à fortiori*, I think, be likely to save life, if employed during the earlier stage of the disease. I therefore should say, take blood in the very outset, as largely as may seem prudent: then give the tartar emetic solution in the way already described. As soon as it causes vomiting, and pallor, and a sinking of the pulse, stop; and suffer the heart to recover itself. And if, with the rallying circulation, the difficulty of breathing returns, have recourse again to the same remedy. The faintness and collapse are sometimes so great as to threaten the extinction of life: the child, with a flying pulse, and a clay-cold surface, seems gasping its last. When this happens, a few drops of sal volatile, or of brandy, mixed with water, will presently bring the little patient round again. If no ground is gained after two or three repetitions of the nauseating treatment, then it will be well to make trial of the calomel plan.

Sometimes the tartar emetic acts severely on the bowels: it may occasionally therefore be necessary to combine it with a small quantity of syrup of poppies.

Blisters are often applied in this disease; but with very questionable propriety. In the outset they are likely to do harm; in the advanced periods they are not likely to do good. When used at all, they should be placed, not on the throat, but across the upper part of the sternum.

When signs of approaching death have come on—lividity of the lips, coldness of the skin, and a tendency to stupor—the question

will obtrude itself whether there may not still be a chance of saving the patient by performing tracheotomy. In the first place you will consider that the operation is even more difficult to execute upon children than upon adults; and is attended with more perplexing hæmorrhage. But there is a greater objection than this to tracheotomy in such cases—an objection which you will have anticipated—namely, the existence of the preternatural membrane; which often extends so far down, that air would not be admitted into the lungs, even when the incision into the windpipe was made at the lowest possible point. Another consideration, forbidding much hope of success from this expedient at any part of the disease, is that the ramifications of the bronchi and the ultimate air-cells get filled up with serous, or mucous, or puriform matter, or even sometimes with a membranous exudation, whereby suffocation is effected *in the lungs themselves*. The membrane in the trachea, being tubular, does not entirely exclude the air from those organs; but it does not admit it in sufficient quantity. Bronchotomy has again and again been practised in this complaint, to no purpose: and I should be inclined to look upon it as absolutely hopeless, but for two instances of its successful performance, recorded in the *Medico-Chirurgical Transactions*; the one performed by Mr. André, and related by Dr. Farre, in the third volume; and the other by Mr. Chevalier, in the sixth volume. These were both apparently desperate cases. Immediate relief followed the operation in both, and the patients recovered perfectly.

There seem to be just two predicaments in which there is a chance that tracheotomy may be useful. They are perhaps rare: yet they have been noticed by several observers. The one is where the preternatural membrane extends but a very little way down the trachea, and is chiefly confined to the larynx: and the other is where there is no preternatural membrane at all, or only a very slight coating in some part of the trachea, the impediment to the breathing having arisen mainly from a thickening of the mucous membrane. And you will observe that an impediment from this cause will always be the greatest at the narrowest part of the canal: and therefore incision of the windpipe in such a case may be expected to bring relief. The effect produced by the tracheotomy in Mr. Chevalier's case was very instructive. Air was fully inspired through the opening, then a strong cough took place, by which a large quantity of viscid reddish mucus was forced out by the natural channel, through the glottis. It was evident that the child could not expectorate before, simply because it could not sufficiently fill its lungs with air to drive the collected mucus out. Dr. Farre gives a cir-

cumstantial account of a case in which the adventitious membrane did not reach more than a finger's breadth below the cricoid cartilage; and the rest of the tube was so free that he was convinced that the child's life might have been saved by a timely opening into the trachea. Unfortunately, we cannot tell, before death, to what degree or extent the preternatural membrane exists. All that can be said, I think, is, that when dyspnoea and much croup come on suddenly or quickly, the disease is probably limited to the larynx and upper part of the windpipe: but that when the progress of the disorder is slower, and the croupy symptoms not so well marked, it is more likely that a greater extent of the trachea, below the larynx, participates in the mischief. Our expectations of success from tracheotomy will vary accordingly. It affords a bad chance at the best; but it affords also, in many cases, the *only* chance.

There is a sort of bastard croup, with which it is quite necessary that you should be acquainted, for it is not at all uncommon; nay, it is far more common, in this place at least, than the real disease. It has received a variety of names, which shows that it has been recognized, as a distinct malady, by various observers. Yet no doubt can be entertained that it has very often indeed been confounded,—and is still continually confounded,—with the true croup, with cynanche trachealis. In their most obvious symptoms the two affections are much alike. The broad and essential distinction between them, is the absence, in the spurious disorder, of inflammation and of fever—and consequently of any concrete or other effusion from the mucous membrane of the air passages. The child is seized all of a sudden, roused perhaps from its sleep, by a catch, or interruption of its breathing, more or less complete. It strives and struggles to inspire, but is apparently unable to do so; at length the effort is successful, and the breath is drawn in with a shrill whistling or crowing sound, like that which characterizes the inspirations of croup, or of hooping cough, and depending, no doubt, upon the same cause—a narrowing (in this complaint temporary) of the fissure of the glottis. *Spasmodic croup* is the most common of its names. It is the *thymic asthma* of the Germans. My late colleague, Dr. Ley, who, in a work published a short time before his death, has done much to elucidate the pathology of this curious disorder, adopts from Dr. Mason Good the appellation of *laryngismus stridulus*. Dr. Gooch called it *child-crowing*, a homespun term which I much prefer to the somewhat pedantic and cacophonous title bestowed upon it by Dr. Good. The crowing noise, and its con-

comitant phenomena, take place in paroxysms, which vary in respect to frequency and severity, and which are separated by intervals of easy and natural breathing.

“When the closure of the chink of the glottis is not perfect, the child struggles for its breath: the respiration is hurried; the countenance generally blueish or livid; the eyes staring; and each inspiration is attended with a crowing noise. When the closure is more complete (and this state was found by Dr. Ley, whose words I am now quoting, to be much the most frequent at the commencement of the paroxysm) the function of respiration is entirely suspended for a while; there is an effectual obstacle to the admission of air. The child makes vehement struggles, by some termed convulsive, to recover its breath. At varied intervals, from a few seconds up to a minute, or upon some occasions nearly two minutes, air is at length admitted through the glottis, now partially open; and this rush of air, passing through a very narrow chink, produces the peculiar sound. To these symptoms not unfrequently succeed a fit of coughing or crying, which terminates the scene: or, if the glottis be not thus partially open, the child, at the end of from two to three minutes at the utmost, will die suffocated. Pallid and exhausted, it falls lifeless upon its nurse’s arms; and it is then that the child is generally said to have died in a fit.”

Sometimes, but not always, with the symptoms now described there is a contracted state of the flexor muscles of the thumb, fingers, wrist, ankle, and toes; giving to the foot an appearance approaching to that of club-foot.

Now most of those persons who had learned not to confound this child-crowing with true croup, have been of opinion that it was connected with *cerebral* disease, or disorder; and that the state of the glottis, and the flexion of the hands and feet, both depended upon tonic spasm. Dr. Ley doubts the correctness of that notion, and believes that the bending of the limbs results rather from feebleness or paralysis of the extensor muscles, than upon spasmodic contraction of the flexors. It is obviously a great point to make out, whether the disorder depends upon pressure *within the head* or not. Our treatment will be regulated by what we know, or believe, in that respect. But what is chiefly original, and very interesting, in Dr. Ley’s views concerning this bastard sort of croup is this, that he attributes the temporary closure of the glottis to pressure made by enlarged glands in the neck or chest upon the recurrent nerve, or upon some part of the eighth pair; “subverting the exact antagonism by which the glottis is automatically and involuntarily kept open, and allowing its

margins to come together, and to occasion the peculiar kind of inspiration so much like that of croup.” Dr. Ley looks upon the affection altogether as more allied to paralysis than to convulsive movement. This certainly is a very original view of the matter; but I must refer you to his book for the facts and reasonings upon which it is founded. The important practical fact is the connexion of the child-crowing with tumefaction of the glands in the neck and chest, and with the entanglement of the pneumogastric nerve or its branches among these glands. “Scarcely an instance (says he) has occurred to me since my attention has been very much directed to the subject, in which there has not been the strongest foundation for the belief that either the glandulæ concatenatæ of the neck, or the thoracic absorbent glands, have become morbidly enlarged.”

Even in the short period that has elapsed since Dr. Ley wrote, this curious and intricate knot has been somewhat farther (though it is not yet completely) untwisted. Dr. J. Reid has ascertained, by a well-contrived set of experiments, that the inferior laryngeal (or recurrent) nerve, is an efferent or motor nerve, by which nearly all the movements of the larynx are regulated; and that the superior laryngeal is an afferent or incident nerve. We may easily conceive, therefore, how pressure upon, or irritation of either of these nerves, may affect the aperture of the glottis. If the superior laryngeal nerve be implicated, the impression is communicated to the spinal cord, and thence reflected, through the recurrent, upon the laryngeal muscles. And it seems probable that afferent fibrils of the fifth pair of nerves may have a similar exciting power; for a transient crowing is readily produced in some children, by exposure of the surface of the face and chest to a breeze of wind, or by their being suddenly tossed in the arms of a nurse. We may even suppose that the effect produced upon the central cord may, by reflexion, influence other muscles, and cause the contractions that are sometimes observed in the flexors of the feet and hands. Again, if the recurrent nerve itself be pressed upon, or interfered with, undue contraction or paralysis, according to the kind or degree of interference, will be likely to ensue of the muscles belonging to the glottis. Spasm of those muscles would close the chink, and stop the breath. And Dr. Reid has shewn that their palsy, except while the breathing is perfectly quiet and tranquil, sensibly impedes inspiration, and alters its character. We have, I say, in these considerations, the materials for ultimately solving a very curious problem.

It is interesting to observe how Dr. Ley’s view of the matter harmonizes with what has

been noticed of the predisposing causes of this crowing inspiration. In the first place, it is often manifestly connected with *dentition*. Now one effect of dentition is the production of glandular swellings of the neck; which happen even in the absence of all strumous taint; but with still more certainty if any such taint exist. And this explains the fact, that the disorder has appeared in the most robust as well as in the most delicate infants. This explains also another well-known fact, viz. that, when child-crowing accompanies painful dentition, the symptoms do not vanish instantaneously, as if by magic, the moment that the tooth starts through the gum; but pass off by degrees. The truth seems to be, that, "after the gum and enveloping membrane of the tooth have been relieved from swelling and inflammation by the free use of the gum-lance, *some time* is still required for the irritation and tumid state of the cervical glands to subside."

Again, this child-crowing is found to occur in connexion with excoriations behind the ears, and with inflamed and irritable scalp; and these complaints very frequently lead to enlargement of the absorbent glands of the neck, which enlargement forms thus an intermediate link in the chain of events. And upon some occasions Dr. Ley has had strong reason to suspect that bronchitis, or other *disease of the lungs*, has occasioned enlargement of the bronchial glands, and thus given rise to the crowing inspiration.

I think that Dr. Ley has made out fair grounds for his view of the pathology of what is called spasmodic croup. At the same time I am not quite persuaded that the child-crowing does not occasionally spring out of a cerebral affection. And of this I am sure, that disease of the brain, and a temporary affection of the glottis, producing stridulous inspiration, sometimes go together, if they be not connected as cause and effect.

The practical *fact* which you have to remember is, that croupy breathing may occur, and return in paroxysms, when there is no croup. And the practical *lesson* which you have to learn is, how to discriminate between these two similar, yet different disorders. I have already specified the distinctive characters of cynanche trachealis. The complaint that copies it may be known by its sudden accession and its sudden departure; by the freedom of breathing in the intervals between the paroxysms; by the absence of fever, of preceding or present catarrh, of hoarseness, and of any abiding cough. The diagnosis, easily enough reached when these points are sufficiently attended to, will be still more sure, if you discover enlarged glands in the neck.

It must be unnecessary for me to urge the *importance* of the diagnosis. Those active measures which befit the outset of the in-

flammatory disease, would be misplaced and mischievous in the other. It was the result of Dr. Ley's experience that "those children have upon the whole a much better chance of being preserved, who are not subjected to very severe discipline, than those who, in compliance with the prevailing doctrine, have been treated by very copious bleeding, large doses of calomel, and such other remedies as the supposition of the invariable dependence of the disease upon cerebral turgescence or excitement has suggested."

Do not, however, imagine, from what I have just been saying, that this disorder, child-crowing, is a trifling disorder, and unattended with danger. It is really a perilous, as well as a terrifying condition. The respiration is sometimes so long suspended that death takes place in the paroxysm. And each paroxysm is accompanied by a tendency to stagnation and congestion of blood, in the brain, lungs, and heart: a tendency which, by its frequent repetition, may lay the foundation of serious and fatal disease in one or other of those vital organs.

The treatment of this affection must depend a good deal upon the nature of the predisposing and exciting causes. To go into it fully would require that I should speak of the mode of cure in painful dentition, in eruptive complaints about the head, in certain pulmonary and cardiac diseases, and in all those conditions which are apt to give rise to enlargement of the catenated or bronchial glands.

In addition to the specific methods of treatment which these several diseases may need, attention to the diet of the child, and to the state of the skin and bowels, and (more than any thing else, perhaps) removal from the air of a city or town to the purer atmosphere of the country, will often prove eminently serviceable.

In the paroxysm itself the warm bath might be useful, if it could be got ready in time. The application to the throat of a large sponge from which hot water has just been squeezed, is a more accessible, and often a very effectual expedient. Sprinkling the face and chest freely with *cold* water will sometimes unlock the spasm, and set the little sufferer free.

I should have stated, that Dr. Ley's views were abundantly supported by the researches that he had made into the morbid anatomy of this affection. Had he lived, I should have had it in my power to shew you some very beautiful and interesting preparations, illustrative of the connexion of the disorder with enlarged glands, which had come to press upon, or stretch the par vagum, or its branches. As it is, I can only shew you the published engravings from some of them, appended to his book.

LECTURES
ON THE
FUNCTIONS OF THE NERVOUS
SYSTEM.

BY W. B. CARPENTER, M.D.

LECTURE IX.

(Concluded.)

ON studying the conjoint movements of the eye-ball, we are led to observe the very curious fact that they are not so much *symmetrical* as *harmonious*; that is to say, the corresponding muscles on the two sides are rarely in action at once; whilst such a harmony or consent exists between the actions of the muscles of the two orbits, that they work to one common purpose, namely the direction of both eyes towards the required object. In order to study them properly, it is necessary to reduce them to some kind of classification. 1. If one eye be rotated *inwards*, and the other *outwards*, the internal rectus of one eye, and the external rectus of the other, are evidently put in action together. This movement is harmonious or consensual, but not symmetrical. 2. *Both* eye-balls are *elevated*, by the contraction of the superior recti. 3. *Both* eye-balls are *depressed*; this is at first effected by the conjoint action of the inferior recti muscles, and so far the movement is, like the preceding, both harmonic and symmetrical; but, if it go beyond a certain point, both eyes are rotated inwards, showing that the internal recti are then in action. 4. *Both* are drawn directly *inwards* and *downwards*, as when we look at an object placed on or near the nose; this movement is symmetrical, but not harmonic; and it is effected by the action of the internal rectus, joined either with the inferior rectus or the superior oblique. 5. When *one* eye is rolled *upwards* and *inwards*, and the *other* *upwards* and *outwards*, the inferior oblique is probably operating on one side, whilst the superior rectus unites its action with that of the external on the other. And, 6, when *one* eye is drawn *downwards* and *inwards*, and the *other* *downwards* and *outwards*, the inferior rectus is probably operating, along with the internal rectus, on one side, whilst the superior oblique is the chief cause of the latter movement. All these movements may be voluntarily performed by man; but it is not so clear that the muscles by which they are effected are equally influenced by volition in each case; and there are some curious diversities in our power of operating on different muscles, which throw some light on the matter. Of those which are entirely subjected to the will, we can only put that pair in action together, which will operate without

destroying the symmetrical position of the two eyes, namely the superior recti. We cannot voluntarily abduct both eyes, nor roll them downwards and outwards, by the conjoint action of the two external recti or superior obliques. Nor, again, can we bring any of these voluntary muscles—the superior oblique and superior rectus, for example—to act against each other in the two eyes, so as to destroy their symmetry. In almost every movement in which the harmony of the two eyes is preserved, whilst the symmetry is destroyed (as in those of the first, fifth, and sixth of the foregoing classes), one or more muscles of *voluntary* motion are acting on *one* eye, and one or more of the *automatic* group are chiefly concerned in producing the rotation of the *other*. This idea is an extremely ingenious one, and will be found to be supported by other facts. The orbit is not the only part in which it is difficult, if not impossible, to put the corresponding muscles on the two sides into similar action at the same time, as a very simple experiment will show. Let the right hand be moved in a circle, as if it were winding a cord on a reel placed in front of the body; that is to say, if the hand be held up in front of the chest, it will first move downwards and forwards, then forwards and backwards (towards the body), next upwards and backwards, and lastly upwards and forwards*. Now, if the left hand be made to move in the same direction, as if it were winding another cord round the reel, it will be found much more difficult to make it revolve *with* the other, than at the distance of half a circle from it—that is to say, it will be found much easier to move the two hands in such a manner that the points of the fingers of one shall be passing *up*, whilst the others are going *down*,—or *forwards*, whilst the others are going *backwards*. But, what is still more remarkable, it will be found totally impossible to move the two hands, with any degree of rapidity, in contrary modes,—as if they were winding two cords on the same reel, in opposite directions,—unless they are guided by actually holding such a cord. Here, then, we have an instance in which it is more easy to call dissimilar or unsymmetrical muscles into action, in order to produce harmonious movements, than it is to employ corresponding muscles to execute movements not consensual.

But there are two kinds of movement of the eye-balls which are not at all voluntary. In

* This attempt to describe the very simple kind of movement intended, is a very awkward one, as the author is well aware; but though the fact is *shown* in an instant, it is not readily explained in words. He hopes, even by some circumlocution, to place before those interested in the subject, a very beautiful illustration, which he has not seen elsewhere applied.

the first of these, *both* eye-balls are rotated *upwards* and *inwards*, by the action of the inferior oblique. In the other, *both* eye-balls are directed inwards, by the action of the rectus internus. Now in both these cases, the harmony of the movements is destroyed, but it is by two similar muscles, both acting automatically, and subjected, therefore, to the same stimulus. In the first of these cases, the stimulus may originate in some part very distant from the eye itself, and may be of a purely reflex kind; as when the eye is rotated under the lid, in the acts of sneezing, coughing, &c. The latter we shall find to be another result of the same cause as that which secures the usual harmonic movements of the eye-ball.

It may, I think, be stated as a physiological fact, that single vision with two eyes is dependent upon the formation of the image upon parts of the two retinæ, which are *accustomed* thus to act with each other. In many physiological works you will find it stated that single vision is the result of the impressious being made on *corresponding* parts of the two retinæ,—that is to say, on parts equally distant from the axis, on one side or the other; but this is, I think, a manifest absurdity; since patients who have been long affected with convergent strabismus, and who see equally well with both eyes (as many do), are not troubled with double vision. On the other hand, when a person whose eyes look straight before him, is affected with any complaint in his head which renders their motions in any degree irregular, he is at once affected with double vision; and the same I have myself noticed to be one of the almost invariable results of the successful operation for the cure of strabismus. Although the images were previously formed on parts of the retinæ which were very far from corresponding with each other, yet no sooner is the position of the eyes rectified, so that the relation between the situation of the images is the same as it would have been in a sound eye, than the patient sees double—unless (as frequently happens) the sight of the eye most affected be of so little use to the patient, that its image is not regarded by him. Now in these cases the difficulty very speedily diminishes, and the patient soon learns to see single. I cannot imagine, then, that any other cause than *habit* can explain the long-disputed phenomenon of single vision with two eyes. The mind receives the two images, frequently combining them together (as Mr. Wheatstone's ingenious experiments with the stereoscope have most satisfactorily shown) to produce a picture in relief; and so long as these be conveyed to it in the accustomed manner, it reconciles them together, even if the parts of the retinæ on which they are formed do not correspond; but if any cir-

cumstance break this chain, and cause the images to be transmitted to the sensorium through a new channel, the mind requires some little time to adapt itself to this impression, as it does by habit to almost every other.

If this be admitted, we gain, I think, an important step in the explanation of the consensual movements of the eye. The object to be attained is evidently this—that the usual axes of the eyes should always be directed towards the object to be viewed; and this, as we have seen, involves the necessity (in a great majority of cases), of unsymmetrical movements being performed by the two eye-balls. Now I think it is fair to argue from the facts already stated (respecting the distribution of the third pair, and the known functions of its inferior branch), that, in directing our eyes by a voluntary effort to any particular object, the will acts chiefly upon one eye, and that the other follows its direction by an automatic movement. This automatic movement I believe to be governed by the relative place of the images upon the retinæ. It is well known that in children born blind, the movements are not consensual; I have seen them very far from being so, in cases of congenital cataract, where a considerable amount of light was evidently admitted, but where no distinct image could be formed; and in such cases the movements were most consensual where the object was bright or luminous, so that the impression made upon the retina was more vivid. It is no objection to this theory to say, that persons who have become blind may still move their eyes in a consensual manner; since, the habit of the association of particular movements having been once acquired, the known laws of nervous action account for its continuance, and, as a matter of fact, a want of consent may be often noticed where the blindness is total. The idea which I desire to place before you, therefore, is simply this; that, where one eye is voluntarily directed towards an object, the consensual movement of the other will result from an *instinctive* tendency on the part of the nervous system to bring the other into such a position, that *its* accustomed axis also shall be directed towards the object. This principle fully accounts also for the convergence which any one may induce by looking at an object brought very close to the eyes. A slight degree of convergence must always exist, unless the object be infinitely distant; as, however, it is brought nearer, the convergence increases, until, if the attention be fixed upon it, with an effort *not* to see it double, the two eyes are very obviously directed towards each other. Now although, in the classification of the movements just now given, Valentin has placed the movement of both eyes directly inwards,

under a distinct head from the rotation of both downwards and inwards—considering the former as automatic, whilst the latter can also be accomplished voluntarily—I am disposed to explain them both in the same manner; for the latter can, I think, only be accomplished by directing the eyes towards the nose as an object; and then it depends on precisely the same considerations as the former.

That there is a *greater* tendency to *consent* between the images, when they are formed upon corresponding parts of the retinae, I readily admit; and I think that this is a principle of some importance, in explaining the re-adjustment of the eyes, after the operation for strabismus. Every one who has seen much of this operation is aware, that the readjustment of the eye is not always immediate; but that, after the muscle has been freely divided, the eye often remains somewhat inverted for a few days, gradually acquiring its straight position. I have known one case in which, after such a degree of temporary inversion as seemed to render the success of the operation very doubtful, eversion actually took place for a short time to a considerable extent; after which the axes became parallel, and have remained so ever since. Another argument, in favour of the consensual movement being chiefly dependent upon the place of the impressions on the retina, is, that the operation is much more successful in those cases in which the sight of the most displaced eye is good, than in those in which (as not unfrequently happens from long disease) it is much impaired. In cases of the latter class, the cure is seldom complete. There is one more curious fact to which I shall advert in reference to this subject: that strabismus not unfrequently arises from the formation of an opaque spot on the centre of the cornea, which prevents the formation of any images on the retina, except by the oblique rays; and nature seems to endeavour (so to speak) to repair the mischief, by causing the eye to assume the position most favourable for the reception of these.

To one more point only, connected with the subject of strabismus, shall I now allude. I am well convinced, from repeated observation, that those surgeons are in the right, who have maintained, in a recent controversy, that, in a large proportion of cases, strabismus is caused by an affection of *both* sets of muscles or nerves, and not of one only; and that it then requires, for its perfect cure, the division of the corresponding muscle on both sides. You will frequently meet with cases in which this is evident; the two eyes being employed to nearly the same extent, and the patient giving to both a slight inward direction, when desired to look straight forwards. In these cases the double ope-

ration is almost invariably required. In general, however, one eye usually looks straight forwards, whilst the other is greatly inverted; and the sight of the inverted eye is frequently affected to a considerable degree by disease, so that, when the patient voluntarily rotates it into its proper axis, his vision with it is far from being distinct. Some surgeons have maintained that the inverted eye is usually the only one in fault, and consider that the division of the tendon of its internal rectus is sufficient for the cure. They would even divide its other tendons, if the parallelism be not restored, rather than touch the other eye. I am myself satisfied, however, that the restriction of the abnormal state to a single eye, is the exception, and not the rule, in all but very slight cases of strabismus; and to this opinion I am led both by the consideration of the mode in which strabismus first takes place, and by the results of the operations which have come under my notice. If you watch the eyes of an infant affected with cerebral disease, you will frequently observe in them very irregular movements; the axis of the two being sometimes extremely convergent, and then very divergent. You will rarely or never see this irregularity confined to one eye. Now in a large proportion of cases of strabismus, the malady is a consequence of some cerebral affection during infancy or childhood, which we can scarcely suppose to have affected one eye only. Again, in other instances we find the strabismus to have resulted from the constant direction of the eyes to very near objects, as in short-sighted persons; and here, too, the cause manifestly affects both. Now it is easy to understand why one eye of the patient should *appear* to be in its natural position, whilst the other is greatly inverted. The cause of strabismus usually affects the two eyes somewhat unequally, so that one is much more inverted than the other. We will call the least inverted eye A, and the other B. In the ordinary acts of vision, the patient will make most use of the least inverted eye, A, because he can most readily look straight forwards or outwards with it; but to bring it into the axis, or to rotate it outwards, necessitates a still more decided inversion of B. This remains the position of things,—the patient usually looking straight forwards with A, which is the eye constantly employed for the purposes of vision,—and frequently almost burying under the inner canthus the other eye, B, the vision in which is of very little use to him. When, therefore, the tendon of the internal rectus of B is divided, the relative position of the two is not rectified. Sometimes it appears to be so for a time, but the strabismus then begins to return; and it can only be checked by division of the tendon of the other eye, A;

after which, as far as I have seen, the cure is generally complete and permanent. That it has not been so in many of the cases on which operations have been performed, I attribute, without the slightest doubt in my own mind, to the neglect of the second operation. As I just now stated, the sight of the most inverted eye is frequently very imperfect; indeed it is sometimes impaired to such an extent, that the patients speak of it as entirely useless. That this impairment results in part from disuse merely, seems very evident, from the great improvement which often succeeds the rectification of the axes. I cannot help thinking it probable, however, that the same cause which produced the distortion of the eye may, in some instances at least, have affected the optic nerve, as well as the motor nerves of the orbit.

It may be asked,—in what respect do the *consensual* movements of the eyes, which have been stated to be the involuntary results of sensory impressions made upon the retinae, differ from the ordinary reflex movements, for which it has been argued that sensation is not essential? The answer is simply this,—that the impressions which excite them are made upon an organ of *special* sensation, which operates in many respects under peculiar conditions. As a general rule I believe that it may be stated that instinctive acts do not result merely from *impressions* upon the nerves of special sensation, but that impressions must become sensations in order to give rise to them. Upon further examination, however, we shall find that between the sensation and the motion there is not an immediate connection; but that some act of mind intervenes. This is evidently the case in the large class of actions termed *emotional*, which, as I explained in a former lecture, are entirely distinct from volitional actions, and with which I believe that the actions ordinarily termed *instinctive*, as distinguished from *intelligent* or *rational*, will be found to bear a close correspondence. This term has been used in a great variety of senses; it has been sometimes made by physiologists to comprehend *all* the actions which are performed in direct response to an external stimulus,—thus including reflex movements, and even the contraction of muscles on the direct application of a stimulus. Others, again, have used the term rather in a metaphysical sense, to indicate those actions that are performed by animals with an evident purpose or design; of which design, however, there is reason to believe them to be unconscious; these actions being invariably performed by all animals of the same species, forming their peculiar habits, and admitting of only a very limited modification, according to the peculiar circumstances of the in-

dividual. It will be convenient for us to employ it to indicate actions of the same class; but we shall define it rather more physiologically. Instinctive actions are those immediately prompted by sensations or emotions, felt or remembered; whilst in voluntary acts, neither sensations nor emotions directly affect the body, but only serve to stimulate the reasoning processes, and to supply motives to the judgment; the operations of this terminate in the formation of a volition, the commands of which are conveyed to the muscle through a channel structurally distinct, as cases of paralysis fully prove, from that which is the medium of emotional actions. Dr. M. Hall is of opinion that the spinal system of nerves constitutes the channel of emotional actions. I do not see any other evidence for this, however, than that, in cases of paralysis, it occasionally happens that reflex and emotional actions manifest themselves, when voluntary control is lost. Such cases only prove, however, that emotional actions are *not* volitional; they are far from proving them to be spinal. If the essential correspondence between the purely emotional acts of man, and the instinctive acts of the lower animals, be admitted, I think that we may reasonably localize them more satisfactorily in that chain of ganglionic masses, which only occupies the centre of the base of the brain in man, but which, in the lower Vertebrata, possesses an aggregate dimension far exceeding that of the cerebral hemispheres. We are led to such a localization by a very simple and (as it appears to me) satisfactory train of reasoning. The actions in question are not simply reflex; since sensation, and something of the nature of emotion, both involving consciousness, are elements in their performance; and, moreover, these sensations are rather of the *special* than of the *common* character, involving, therefore, the olfactory, optic, and auditory ganglia. No intelligent person can doubt, that, as we descend the scale of being, instinct is gradually superseding reason; and that in the lowest Vertebrata (I go no further, because no comparison between the parts of the nervous centres could be made with equal certainty between animals of a different type) the manifestations of the latter are extremely feeble, nearly all the actions of life being guided by the former. Now on looking at the encephalon, we perceive a difference in the relative proportions of its principal divisions so closely corresponding with these, that it is difficult to imagine them unconnected. In proportion as we descend the scale, we find the cerebral hemispheres diminishing in relative size, whilst the ganglia at the origins of the nerves of special sensation increase to a remarkable degree; and I cannot, therefore, but consider it probable,

that these ganglia and tracts of grey matter, whose size is in man so trifling in comparison to the bulk of his cerebral hemispheres, are subservient to those instinctive actions which are prompted by sensations, but in which volition does not partake. It may be said that, in attributing to this division of the nervous centres a function different from that of the spinal cord, on the one hand, and of the brain, on the other, we are unnecessarily multiplying the systems of nervous fibres which must then be supposed to exist in every trunk;—one, namely, for reflex actions,—another for the instinctive and emotional,—and a third for the volitional. But the tendency of neurological research has certainly been to shew, that different functions are performed by the same trunk, in virtue of its containing fibres which are connected with different divisions of the nervous centres; and knowing, as we do, that these three distinct sources of action have a real existence, it cannot be regarded as improbable that their channels also should be separate. Those who entertain the idea that different parts of the cerebral hemispheres perform different offices in the reasoning process—an idea which I cannot but regard as highly probable,—cannot refuse to admit it as probable that the ganglionic matter which lies near the base of these has a function quite distinct from that of the convolutions. On turning to the Invertebrata, we find important confirmation of these views in the fact, that, in general, the principal ganglionic masses occupying the place of the brain of higher animals are closely connected with the organs of special sensation situated in the head, and are therefore analogous to the optic and other ganglia in Vertebrata; whilst scarcely any traces can be found of superadded ganglionic bodies at all resembling the cerebral hemispheres. The almost exclusively instinctive character of the actions of such animals harmonises well with the opinion, that these ganglia are the chief sources of them.

That many of the lower animals possess psychical endowments, corresponding with those which we term the intellectual powers and moral feelings in man, cannot be doubted by any person who will attentively study their characters; but in proportion as these are undeveloped, in that proportion are they under the dominion of those instinctive impulses, which, so far as their own consciousness is concerned, may be designated as blind or aimless, but which are ordained by the Creator for their protection from danger, and the supply of their natural wants. The same may be said of the human infant, or of the idiot, in whom the reasoning powers are undeveloped. Instinctive actions may in general be distinguished from

those which are the result of voluntary power guided by reason, chiefly by the two following characters:—1. Although, in many cases, experience is required to give the will command over the muscles concerned in its operations, no experience or education is required, in order that the different actions which result from an instinctive impulse may follow one another with unerring precision. 2. These actions are always performed by the same species of animal, nearly, if not exactly, in the same manner; presenting no such variation of the means adapted to the object in view, and admitting of no such improvement in the progress of life, or in the succession of ages, as we observe in the habits of individual men, or in the manners and customs of nations, adapted to the attainment of any particular ends, by those voluntary efforts which are guided by reason. The fact, too, that these instinctive actions are often seen to be performed under circumstances which reason informs us to be such as to render them nugatory for the ends to be accomplished by them (as when the flesh-fly deposits her egg on the carrion-plant, instead of a piece of meat, or when the hen sits on a pebble, instead of her egg), are additional proofs that the instinctive actions of animals are prompted, like the consensual movements we have been now inquiring into, by an impulse which immediately results from a particular emotion or sensation being felt, and not by anticipation of the effect which the action will produce.

In the succeeding lecture, the application of this doctrine to the physiology of the human mind will be inquired into; and a sketch will be given of the present state of our knowledge of the functions of the cerebrum and cerebellum.

FINAL RESULT OF A CASE
IN WHICH
DIEFFENBACH'S OPERATION FOR
STAMMERING
WAS PERFORMED: WITH OBSERVATIONS.

To the Editor of the Medical Gazette.

SIR,
ALTHOUGH experience has already determined upon the value of the treatment of stammering by operation, I think it my duty to communicate to the profession the result of my operation, which, as it was performed on March 1st, may now be considered as final.

Up to the 9th of March, when I wrote the account of the operation, which was published in the number of

the *MEDICAL GAZETTE* for March 11th, 1841, no sign of that dreadful degree of stammering, almost equivalent to dumbness, nor of those violent convulsions of the muscles of the face and neck, from which the patient was suffering before the operation, had made its appearance; but about a week after this time, while speaking to him, I was sorry to notice some slight quivering motion of the upper lip, which almost every day became worse, and extended to the muscles of the cheek, eyelid, and lower lip, and in about a fortnight (or a month after the operation) the whole face was just as much convulsed as before the operation; the neck, however, was far less convulsively affected, and the former spasmodic rising or elevation of the larynx was not observed. The stammering also had gradually returned to the same degree as before the operation, but the patient on being questioned made me understand, that, when he was about to speak, or even at the very time when the convulsions of the face were at the highest, so that they stopped his speech, by firmly compressing his lips, and thus closing his mouth, his tongue felt less in size and less rigid; nor did he experience the sensation as if the cavity of the mouth were plugged up by the tongue, and the passage in the throat obstructed, from which, as the mother of the patient told me, he suffered much before the operation.

As the patient was of strumous habit, and nervous temperament, and, moreover, somewhat reduced by the operation, although in other respects quite well, I prescribed for him steel medicines, meat diet, with porter, sponging the whole body every morning with cold water, and frequent exercise in pure air. I also gave him some instructions as to the best manner in which he ought to exercise himself in reading and speaking, and as he, on account of his severe affliction, had always been of a very melancholy disposition, I gave myself all pains to raise his spirits, and make his mind easy, which directions I also communicated to his mother. This plan of treatment has been pursued since the end of March, and, I am happy to say, with a favourable result. When I lately visited the patient, after an interval of more than two months, I was

quite surprised to find the convulsions of the face considerably diminished, and his speech, although stammering, yet decidedly improved: in fact, I had never before heard him speak so much; he spoke even one or two sentences without any hesitation. His eyes no longer protruded, nor did they water; the neck was not at all affected by the convulsive motions, and the cheeks and lips in a minor degree; they appeared now to be confined to the lips more especially. I therefore advised him to continue the above plan of treatment, and tried to impress upon his mind, that, if he persevered in this plan, he would, in time, overcome the calamity, which assurances satisfied his mind, as was evident by the change in his countenance.

When I examined the patient's tongue on my last visit, I found the dorsum linguæ presenting a smooth surface; a small fissure, which the operation had left at one side of the tongue, had perfectly filled up. The cicatrix appeared as a very narrow line across the tongue, with a deficiency of papillæ. The tongue could be freely and well moved in all directions. The sense of taste, which was lost immediately after the operation, returned in the space of a week, and since that time has been as acute as ever.

This case, which certainly was the worst I ever saw, being almost equivalent to dumbness, and in which a wedge-shaped portion was cut out of the root of the tongue, on the dorsum linguæ, half an inch in breadth, and reaching right across and through it, where every attention was paid to the healing of the wound, which proceeded most favourably, is therefore only one proof more of the correctness of the opinion in which the treatment of stammering by operation is now generally held by the faculty at large. Although, in this case, the present amelioration in the stammering and convulsions of the face has not been effected by the operation, but principally by acting upon the patient's constitution, both of mind and body, yet the smaller degree of rigidity of the tongue, the feeling of the cavity of his mouth and throat being less obstructed, and the absence of the convulsions of the neck, and of the spasmodic rising of the larynx, leave no doubt that the excision of a wedge-shaped portion of

the tongue has not only rendered it smaller in size, but has actually produced some change in the functions of the nerves in the tongue and larynx. This circumstance at first suggested to me the idea, that, if the convulsions of the face which closed the mouth could only be removed by a subcutaneous division of the muscles principally affected, the patient might perhaps be enabled to speak without stammering. This idea of dividing some of the muscles of the face in spasmodic affections, which was originally proposed by Stromeyer, and has, as I find, already been put into execution by Dieffenbach, appears certainly based upon the fact, that spasms of the muscles, and even purely nervous affections, have, in many instances, been cured by division of the affected parts; but a closer consideration made me abandon it, since, in my opinion, we are, from our present state of knowledge respecting spasmodic diseases, not as yet in a position to estimate the division of muscles and tendons as an antispasmodicum.

The temporary removal of stammering which is generally observed instantly after the operation, can only be attributed to the shock of the nervous system, and perhaps to the loss of blood, as also to the careful manner in which the patient endeavours to speak during the healing of the wound; and the same causes may even produce a permanent cure, which is said to have taken place in some cases. In confirmation of the possibility of a cure by these means only, I may be permitted to communicate a case that happened under my observation, eleven years ago, and which I have already mentioned to several of my medical friends in London, since the various operations for stammering have been brought forward. Whilst at the University of Leipsic, a fellow-student of mine, of nervous constitution, who had a decided impediment of speech, especially when in the least excited, happened to fight a duel, in which he received a sabre wound in his face, extending from the outer angle of the left eye down to the chin, completely severing the cheek. The bleeding was of course enormous, so that he several times fainted, and was eventually only stopped by the actual cautery. The wound, however, healed, and he recovered;

and, to our great astonishment, his stammering was completely cured, nor did it ever return. Another case was communicated to me by good authority, of a gentleman who was affected with stammering, and on account of some inflammatory disease was bled to a great extent, and when he recovered from the inflammation his impediment of speech was cured. He is now a judge in a court of justice, to which situation he would not be fitted had his stammering returned. Now in these two instances we can only account for the cure by the loss of blood, and in the first case by the shock also on the nervous system; a cure which therefore appears no more accidental than that which may take place after the performance of one or the other of these operations for stammering. A far safer and milder method is, on the other hand, a moral management and constitutional treatment, as also a proper education, as it were, of the organs of speech. I am convinced that in those instances, where the stammering is not attributable to an organic or mechanical cause (which true stammering very seldom is), but dependent on a functional derangement in the nerves of the organs of speech, and occurring, as it almost always does, in nervous persons, much may be effected by this plan of treatment; and it appears to me probable, that such a plan, if judiciously pursued, the patient possessing at the same time sufficient determination and perseverance, and his circumstances permitting the use of all means requisite for the management of the constitution of his mind and body, may, in a majority of cases, secure a perfect and permanent cure.

I may here be allowed to state, that four different medical gentlemen, amongst which I mention Dr. Trettenbacher, because he is named by Prof. Dieffenbach, in his Memoir addressed to the Institute of France, told me that when they left Berlin (the last of them left in the beginning of June), F. Doenau, on whom Dieffenbach performed his first operation, on Jan. 7th, is one of those (although few) which remain free from stammering, and from facial convulsions, and that the splendid success of a permanent cure in this case, induced him especially to practise the operation on others, who in great numbers entreated him to per-

form the operation ; which, however, the Professor soon felt reluctant to repeat. That case, which proved fatal, occurred in J. Heilmann, a Jew, who, after the operation, was brought into a ward to cool his mouth with water, until the Professor had performed some other operation, but whence he made his escape, and went to a great distance home, where he tore out the ligatures, and of course brought on a fearful return of hæmorrhage, which could not be stopped by a surgeon in the vicinity until the next morning, when Dieffenbach was sent for, and again renewed the ligatures : the patient, however, died on the fifth day.

Having given a faithful account of the result of the operation, as also of the success of the psychical and constitutional treatment, although in a single case only, I hope my endeavours to show forth the actual worth of these operations will be an ample apology for the length of the report ; and I remain, sir, your obedient servant,

AUGST. FRANZ, M.D. Leipsic,
M.R.C.S. &c.

19, Golden Square,
Aug. 5, 1841.

ON A

CASE OF CONGENITAL IMPERFECTION OF THE VAS DEFERENS AND TESTICLE.

BY JAMES PAGET, M.R.C.S.,

Demonstrator of Morbid Anatomy at St. Bartholomew's Hospital, &c.

[*For the London Medical Gazette.*]

AT the inspection of the body of a robust man, aged 71, who had died of acute inflammation of the œsophagus, I observed that he seemed to have but one testicle. By a close examination the following circumstances relating to the condition of the genital organs were ascertained :—

The scrotum was small, and unusually narrow ; on its anterior surface there was no trace of raphé, but on its posterior part the raphé of the perineum was continued for a short distance. The penis hung on the left side of the scrotum, between it and the left thigh, and was of ordinary dimensions. The bladder, prostate gland, and vesiculæ seminales, were healthy and well formed. The left vesicula was rather smaller than the right : both contained the

usual brownish fluid. The right vas deferens and testicle, and the parts connected with them, differed in no respect from those of healthy men. The left vas deferens followed its usual course from a healthy ejaculatory duct to the internal inguinal ring, where, meeting with the other parts of the spermatic cord, it became larger and slightly tortuous. After two or three short curves it terminated nearly opposite the external ring in a rounded cul-de-sac : it was pervious to its very extremity, and its canal was of the ordinary diameter. The remainder of the spermatic cord passed towards the outer and lower part of the left side of the scrotum, and there expanded into a small flat mass of an oval form, adhering to the surrounding cellular tissue, but clearly defined by its rather closer texture. In this part of the spermatic cord there was no trace of an obliterated duct : it seemed composed of little more than fine cellular tissue ; and in the mass at its extremity there was neither a cavity, nor any appearance of tubules, tunica albuginea, or any other part of a testicle. The left spermatic artery was derived from the left renal, and passed in its usual direction through the inguinal canal ; but it was very small, and, not having been injected, it could not be traced beyond the closed extremity of the vas deferens. The inguinal rings had their usual character.

In this case (of which the preparation is preserved at St. Bartholomew's Hospital)* there are presented two very rare congenital defects, namely, an imperfect development of one of the vasa deferentia, and a complete absence of one of the testicles. As they both possess some interest, as well in physiology as in practice, I shall venture to make some observations on each of them, and on the few other cases of the same kind that are recorded.

I. *Of deficiencies of the vas deferens.*—Of the imperfection of that portion of the vas deferens which should be connected with the testicle, three cases have been accurately described by Hunter†, Brugnone‡, and Dr. Fisher§. In

* Ser. 23, No. 92, N. S.

† On the Glands called Vesiculæ Seminales. Complete Works, vol. iv. p. 23.

‡ Observ. Anat. sur les Vesicules Seminales. Mem. de l'Acad. Roy. des Sciences à Turin, 1786-7, p. 625.

§ On the Functions of the Cerebellum : American

the first of these the testicles were normally formed, but both the vasa deferentia terminated abruptly with closed extremities at a short distance from the inguinal rings; in the second, no part of the duct of the right side was developed, except a portion about an inch long, which had its usual connection with the vesicula seminalis; in the third, both testicles were absent, and each of the vasa deferentia ended abruptly in the scrotum.

A few cases also are recorded in which the vasa deferentia were defective at the part which should be connected with the vesiculæ seminales. In that by Mr. Hunter, already referred to, they were not only deficient near the testicles, but they both terminated in irregularly formed vesiculæ seminales, and had no canals of communication with the urethra. In a case by Bosscha,* the whole of the left vas deferens of a robust and otherwise well-formed man was wanting, except two inches that ascended from its natural connection with the epididymis. And again, in two cases described by Tenon†, the vasa deferentia of children affected with extroversion of the bladder passed in the usual manner from the testicles, but terminated in the pelvis at some distance from the bladder.

To these might be added many and various cases of defects of the vas deferens in the Hermaphrodite series of monsters, and in those in whom nearly the whole genital apparatus is irregularly formed; but in them the deviations are usually so complicated that no illustration of the simple defect at present under consideration can be drawn from them. Rather, it may be among the advantages of studying this,

that it will throw some light on one of the elements of those other more obscure and more general departures from the orderly course of development of the organs of generation. For the cases just described show plainly what were the conditions of those organs when, by an unusual circumstance, the natural course of development was arrested in them, and when, though permitted to increase in size, they were permanently fixed in a form which should have been only temporary.

In the normal course of human development, the proper genital organs are in either sex developed in two distinct pieces: namely, the part for the formation of the generative substance, the testicle or ovary, and the part for the conveyance of that substance out of the body, the seminal or ovi-duct. The testicle or ovary, as the case may be, (and in their earliest periods they cannot be distinguished), is formed on the inner concave side of the corpus Wolffianum, and the seminal or ovi-duct, which is originally an isolated tube closed at both extremities, passes along the outer border of that body, from the level of the formative organ above to the cloaca or common sinus of the urinary, genital, and digestive systems below. The perfection of development is attained only by the conducting tube acquiring its just connections at once with the formative organ, and, through the medium of the cloaca, with the exterior of the body. The sexual character is first established when, in the male, the formative and conducting organs become connected by the development of intermediate tubes which constitute the epididymis; or when, in the female, a simple aperture is formed at the upper extremity of the conducting tube, and is placed closely adjacent to the formative organ. In both sexes alike, the lower extremities of the conducting tubes first open into the common cloaca, and subsequently, when that cavity is partitioned into bladder and rectum, or bladder, vagina, and rectum, they acquire in each their just connections, and become in the male the perfect vasa deferentia, and in the female the Fallopian tubes and uterus*.

Journal of the Medical Sciences, 1839, vol. iii. p. 352. I am not aware of any other cases being accurately described. Mr. Wilson (Lectures on the Male Urinary and Genital Organs, 1821, p. 423), mentions that he once met with an example. Meckel (Pathol. Anatomie, Bd. i. p. 685), alludes to one by Pallington, (Scelta di Opusculi Interr.) and Voigtel, (Handb. der Pathol. Anat. Bd. iii. p. 406), refers to a case of deficiency of both epididymides by Rhodius, (Mantissa Anatom. Obs. xlv.) The case seen by Palletta, (Nova Gubernaculi Testis Descr.) and sometimes quoted as an example of this defect, was probably one of degeneration of the vas deferens from disease. The preparation, No. 237, in the malformations at the College of Surgeons, exhibits a defect of this kind in the horse.

* Diss. Inaug. sist. Obs. de Vesiculæ seminalis defectu, Leidæ, 1813, p. 5.

† Mem. sur quelques Vices des Voies Urinaires, &c. in Mém. de l'Acad. Roy. des Sciences à Paris, 1761, p. 115.

* Müller, Hand. der Physiologie, Bd. ii. p. 750. Valentin, Entwicklungsgeschichte. In the female the vagina is generally stated to be partitioned from the common uro-genital sinus; but I think it is more probable that not the uterus only, but

Now, in Brugnone's case, and in Bosscha's, we have examples of one of the male conducting tubes being developed in only a very small portion of its natural extent. These, therefore, clearly confirm the description just given, which is drawn from direct observation of the natural course of development in mammalia; for they prove that the testes may be formed quite independently of the vasa deferentia, and are therefore not of the number of the secernent glands, which grow by tubular efflorescence from the surface on which the secretion is at last poured.

In the other cases the vas deferens was probably formed originally in its whole length; but it seems to have failed of acquiring its due connection in the one series of defects at the end next to the testicle, and in the other at the end next to the bladder. In the first series of cases, in which the upper extremity had been permanently fixed in its earliest condition, the vas deferens exhibits a state of complete indifference as to sexual characters: its extremity is approximated to the testicle, or to that which occupies its place, just as the Fallopian tube is to the ovary, but it has neither a terminal aperture nor any intermediate connecting tube.

The condition of the second series of cases, in which the failure of connection happened at the lower ends of the vasa deferentia, adds, I think, somewhat to the reasons for believing that those ducts are not either wholly or in part formed of the ducts of the corpora Wolffiana; for it is improbable that the extremities of the latter having once opened into the uro-genital sinus should be closed up, or, as in Tenon's cases, removed to a distance, in the partitioning of that cavity; and still more improbable that, if their orifices were never rightly formed, the embryo should live to the period of the development of the testes.

The cases by Hunter and Brugnone illustrate, in some measure, the mode of formation of the epididymis. In the former case the epididymis of the right side, "about half way along where it lies on the body of the testicle, at first became straight, and soon after seemed to terminate in a point;" and of that of

the left side nearly an inch of the lower part was deficient. In the latter case "the right epididymis was almost entirely wanting; there was nothing but the head of it, which formed a number of lobules filled with serum." Müller* says that the coni vasculosi are formed by the development of a substance intermediate between the upper end of the primitive simple vas deferens and the testis, and that the body of the epididymis is produced by the mere growth and convolution of the vas deferens. These cases, on the whole, confirm his account; but they prove also that a part of the body of the epididymis may be formed by the growth of tube from the side of the testis alone, for in all of them there was more of the epididymis than is formed by the coni vasculosi. They afford, therefore, another example of that which seems to be a general rule in development, that if, when two parts are growing in opposite directions to meet each other, they fail of meeting at the usual point, either of them may go on to an uncommon extent. Thus, in infantile hydrocephalus, the parietal and other bones grow to a far greater distance towards meeting each other, than they would have grown had they met at the due limit.

In the cases by Hunter† and Bosscha, we have also illustrations of the mode of development of the vesiculæ seminales. In the former the vasa deferentia passed to a bag divided internally into two parts, which contained the ordinary fluid of the vesiculæ, but had no ducts leading to the prostate gland. In the latter, the right vas deferens and vesicula seminalis were quite healthy; and immediately above the latter, but unconnected with it, was a body shaped like the letter S, the representative of the left vesicula. It was closed on all sides; its cavity had a reticular lining, and contained a pellucid glutinous fluid; it had no connection with the urethra. Both cases together prove, as Dr. Vrolik‡ says, that the vesiculæ seminales, like the testes, are formed separately, and not by an outgrowing from either the vasa deferentia or the urethra;

* Loc. cit. p. 750.

† There is little doubt that this case is the same as that described by Dr. Baillie (*Morbid Anatomy*, p. 370) and figured by him in his 8th Fasciculus, pl. 1, f. 2.

‡ *Handboek der Ontleedkundige Ziektekunde*. 1st. Deel. p. 210.

the vagina also, as far as the labia minora, is formed by the coalescence of the oviducts.

and in this mode of development, as in many other circumstances, they differ from the gall-bladder and other reservoirs.

II. *Of the congenital absence of the testicle.*—Although it is common to find no trace of testicles, at the time of birth, in cases of extensive malformation of the lower part of the body in both man and animals, yet when the body generally is well developed, no organs seem to be more rarely absent. M. Isidore St. Hilaire*, after an examination of all the cases in which they were supposed to be congenitally deficient, concludes that though the occurrence of such a defect is very possible, and even probable, yet “no one of the facts contained in the records of science is at once sufficiently authentic, and sufficiently complete, to prove that this anomaly has ever been met with.” And in accordance with this statement, the occurrence of congenital deficiency of one or both of the testicles—or, at least, of such a deficiency without any other more extensive anomaly—is generally doubted or denied. In addition to the case which I have detailed, I can find records of only two in which this defect occurred; namely, that already referred to by Dr. Fisher, and one in a foetus described by Mr. Thurnam†.

In the latter there was a remarkable malformation of the ureters; and it is said, “on the left side no testicle would appear to have been formed; the spermatic vessels on this side terminated in a little mass of fat; but the vas deferens was present, and apparently as well developed as that of the perfect testicle.”

Dr. Fisher's case was that of a man aged forty-five, in whom the late Dr. Warren discovered the deficiency of testes soon after birth, and who never evinced any sexual appetite. All the genital organs were very small. There were no testes, nor any traces of them in the scrotum. In the upper part of

the left tunica vaginalis (which was naturally formed) the spermatic cord was discovered extending into its cavity about half an inch, and terminating abruptly in a point of a semilunar shape. The cremaster muscle extended in numerous small fibres beyond the end of the cord which spread themselves out upon the tunica vaginalis. The cord itself was much smaller than is usual in adults. The vas deferens was properly formed, and nearly of natural size; its cavity terminated in a cul-de-sac at the end of the cord. The arteries and veins were exceedingly small. The right side of the scrotum, and the right spermatic cord, differed in no respect from the left, except that the latter extended to the bottom of the scrotum, and turned upwards a quarter of an inch. The vesical extremities of the vasa deferentia were not examined.

To these cases I may add another, although it has not received the confirmatory evidence of dissection. It is that of a man, between thirty and forty years old, in other respects well formed, who was under the care of Mr. Stanley during the last winter. He gave a definite and clear statement that he had never had more than one testicle in the scrotum; but Mr. Stanley assures me that the vas deferens of the side on which there was no testicle could be distinctly traced with the rest of the spermatic cord from the inguinal ring, and terminated abruptly in a point at the middle of the scrotum. There was no trace whatever of any of the structures of a testicle, nor any sign of injury or disease of the remaining genital organs.

There can, I think, be little question that the testicles were absent at the time of the birth of the adults whose cases have been related. The only source of doubt is in the chance that, for some time after birth, they lay in the abdomen, and there wasted, and that after their atrophy the vas deferens was alone drawn down into the scrotum. But this is very improbable; and the more so because in all these cases there is a character in which they differ essentially from those in which the testicle once formed has subsequently been atrophied; namely, in the absence of any trace of tunica albuginea.

In all the cases of atrophy of the testicle from disease which I

* *Histoire des Anomalies*, t. 1, p. 709.

† Case of Congenital Malformation of the Urinary Apparatus. *MEDICAL GAZETTE*, August 12, 1837. I think it is very probable that in some of the older cases the defect of one or both testicles was congenital, though their data are so insufficient that it is impossible to prove that it was so. Such are those by De Graaf (*De Virorum organis generationi inservientibus*.—Lugd. Bat. 1668.) Des Naves (*Zodiacus Medico-Gallicus*, 1680-5, Ann. iii. p. 6.) Itard de Riez (*Mem. de la Soc. Méd. d'Emulation*, An. iii. p. 393. Ansiaux. *Journal de Médecine, Chirurgie, &c.*, 1807, t. xiv. p. 262.)

have seen or been able to refer to, there have existed distinct traces both of the obliterated vas deferens and of the tunica albuginea. It is indeed stated loosely, in some of the cases, that after disease the testicle wasted till not a vestige of it remained; but these were not dissected, and they are therefore, for the present question, useless. In far more, even of those that were not dissected, it is said that the testicles wasted to some definite small size, and then ceased to diminish*; and in all that have been dissected there remained some traces of the tunica albuginea, if not of other parts of the testicle. In the last class of cases are included those by Palletta†, Sir A. Cooper‡, Sir B. C. Brodie§, and Rudolphi||; and I may add to these a remarkable example preserved in the museum of St. Bartholomew's Hospital¶, in which no trace of the form of the testicle or epididymis remains, but the vas deferens, gradually diminishing to a very small size, terminates in a flattened oval sac about half an inch long, and formed of tough fibrous tissue, widely different from that in which, in the case of congenital absence of the testicle, the spermatic cord expands.

This permanency of the fibrous investment of an organ whose whole essential structure has wasted from disease, is not peculiar to the testicle. In the extremest atrophy of the eye I believe that traces of the sclerotica are always found; and these, if they were necessary for the purpose, would distinguish atrophy from congenital deficiency of the eyes, in which their tissues are either all absent, or all defective in size. So also, in many other cases, congenital defects are often complete; but the results of atrophy are not totally destructive.

In all the cases of mere atrophy of the testicle, the vas deferens, though small and generally impervious, has still existed in its whole length; so that, in any case where it is altogether

or in part deficient, it would be fair to believe that that part was congenitally absent; for atrophy never, I think, removes any duct or vessel altogether: not even in the process of development are any completely taken away, except such as are removed in the very earliest periods of foetal life, *e. g.* the omphalomesenteric and the Wolffian: all the others, such as the ductus arteriosus and venosus, and the urachus, have permanent representatives in the form of obliterated cords.

Either of the defects which existed in conjunction in the case which is the first subject of this paper, if it occurred at once on both sides of the body, would of course render the person absolutely impotent. The subject of Dr. Fisher's case had all the characters of an eunuch. Mr. Hunter does not mention what was the general appearance of the man whom he examined; but his silence, and the full size of the crus penis represented in the plate, render it most probable that the body had the ordinary male characters. If it were so, it deserves notice that he only had the general characters of an eunuch in whom the testes did not exist; for this would prove that the male form is connected with the power of secreting semen, but has no necessary relation to the power of discharging; it existing where that power is absolutely prevented from being exercised by deficiency of both the vasa deferentia.

The only case that I can find, which affords any probable account of the condition of a man during life in whom both the testicles are healthy, but the vasa deferentia are interrupted, is by an old physician, Christian Helwich*. He tells us of a young Prussian, whose wife, finding him utterly unable to consummate their marriage, applied for a divorce on the ground of his long-proved impotence. The case was referred to the medical faculty, and they found all the external organs of generation well formed and proportioned, and two testes (*mendaces virilitatis testes*), as large as pigeons' eggs, in the scrotum. After much questioning the young man confessed that he had never had either an erection or an emission of semen.

Lastly, these cases add something to

* Larrey, Boyer, Pott, Cooper, &c.

† Nova Gubern. Testis Descr. in Sandifort's *Opuscula Anatom.*

‡ On the Structure and Diseases of the Testis, pl. x. fig. 2.

§ Clinical Lectures on Diseases of the Testicle, LONDON MEDICAL GAZETTE, vol. xiii. p. 620.

|| Encyclopädische Wörterbuch der medic. Wissenschaften, Art. Anorchis. B. ii. 615. Two cases are related here.

¶ Ser. 22, No. 53, M.A.

* De Eunuchis Naturâ Tali, in *Miscellaneis Acad. Naturæ Curiosorum*, Dec. 3. An. iv. 1696. p. 86.

the evidence for suspecting that, if one or both testicles be not found in the scrotum, it is connected with some defect in their formation; but they prove also that a complete descent of the testis affords no certainty of its structural perfection, and that, as in Hunter's and Helwich's cases, a man may be incurably impotent, though, to all outward appearance, his sexual organs are well formed. They do not, however, confirm Mr. Hunter's idea, that a "stimulus of perfection" is necessary to make the testicles assume the disposition requisite for their descent; for they show that the vasa deferentia may be drawn down though the testes be wholly deficient.

ON

PURULENT OPHTHALMIA IN THE
INFANT.

By WILLIAM W. COOPER.

(*For the London Medical Gazette.*)

A LARGE proportion of the miserable objects that are to be seen in the streets, especially of those disfigured with projecting staphylomata, nebulous opacities of the cornea, or shrunk and collapsed globes, owe their affliction to the effects of purulent ophthalmia. This is a disease peculiarly rife amongst the lower orders, although not confined to them: where poverty and misery exist, there purulent ophthalmia is to be found; and as it occurs amongst that class who have to earn their bread by their own exertions, the deprivation of sight is doubly severe, and falls most heavily upon them.

It is a disease which, although exceedingly common, does not appear to be generally understood; at least there are many members of the profession who would seem, from the cases brought under our notice, not to be fully alive to its symptoms, its formidable character, or the treatment by which it is to be subdued. The patients being infants, again, are unable to express their sufferings, and the mothers or nurses, from carelessness or ignorance, too often neglect to apply for advice until the disease is beyond our art, and the eyes have been irrecoverably destroyed.

It may not, perhaps, be unacceptable to some of my readers if I sketch cur-

sorily the symptoms of purulent ophthalmia, adding a few observations upon its treatment, before proceeding to relate the cases I have selected.

If a child be presented to our notice in whom we are informed that on or about the third day after birth a light yellow discharge had made its appearance from one or both of the eyes, that the lids were glued together, that the ciliary margins were of a pinkish hue, and that there was some intolerance of light, we may be pretty sure that this was the commencement of an attack of purulent ophthalmia.

Upon examination, the appearances will vary, according to the length of time which has elapsed since the commencement of the attack and the severity of the symptoms with which it has set in. If still in the early stage, the lids will be swollen, the discharge greater or less, and the palpebral conjunctiva red and villous.

When the disease is more advanced all the symptoms will be aggravated; the lids much tumefied, red and shining; a purulent discharge pours freely from between them, and the child evinces, as far as it can, the greatest suffering. It is, perhaps, impracticable to obtain a view of the eyes, but if they can be seen, the conjunctiva will appear intensely red and villous, and more or less chemosed.

This is the stage in which we are most usually consulted, and it is of course of the utmost importance to ascertain the condition of the eyes. As, however, from the tumefaction of the lids, this is often rendered impossible, it is of consequence to bear in mind the points which enable us to form a tolerably accurate opinion as to the present state of the organs, and probable termination of the case. If the eyelids are tense, of a bright shining red, and the secretion thick and yellow, like what is called "laudable pus," the cornea has not suffered. If, on the contrary, the discharge be thin, whitish, or sanious, the lids flaccid, dull, and of a purplish hue, the diagnosis will be unfavourable, as sloughing of the cornea has probably taken place.

The aspect presented by that tunic (if it can be exposed), under the latter condition, will depend upon the degree to which the mortification has extended. It is at first hazy and dull, then loses its firmness and consistence,

and lastly becomes converted into a slough of a greyish or brownish colour.

A greater or less portion of the cornea may suffer; the humours escape, and collapse of the globe ensues; or, that may be prevented by the adhesion of the iris to the remains of the cornea, and staphyloma, partial or general, may succeed. These, with permanent opacities of the cornea from interstitial deposition, are the most frequent consequences of purulent ophthalmia.

The utmost care and gentleness is requisite in examining eyes affected with this disorder. The slightest violence or pressure upon the globe may destroy the organ by evacuation of the humours if any degree of sloughing of the cornea exists, and Mr. Tyrrell has known the crystalline lens to be projected several feet, from ill-timed violence thus applied. The best time for making an examination is whilst the child is asleep, but as the pain of the disease produces extreme restlessness and irritability, it is not often that we have that opportunity. If an examination be attempted, then, when the infant is awake, the best mode of conducting it is that described by Mr. Tyrrell, and is as follows. "Let the child be placed upon its back, across the nurse's knees, with the head projecting on one side towards the examiner, who should be seated in a high chair by the side of the nurse, so that he can receive the child's head between his knees; he can thus command the child's head, whilst the nurse commands the body and arms.

"In examining the right eye, the surgeon should place the point of the fore-finger of his left hand upon the centre of the free or ciliary margin of the superior palpebra, and the extremity of the thumb of his right hand a little below the centre of the inferior palpebra on the cheek. (The hands would be changed in examining the left eye.) He should then depress the lower lid, by stress upon the integument, as in the former instance, whilst he elevates the superior by carrying its free margin towards the eyebrow with the point of the fore-finger; but in doing this he must keep the point of the finger against the globe without pressing it, or he will evert the eyelid, and thus defeat his object: the operation will be facilitated by investing the

thumb and fore-finger with linen when employed in the examination, as it prevents them from slipping."

The great point in the treatment will be to prevent the accumulation of the discharge in the eyes, which is to be accomplished in the following manner.

When an accumulation has taken place, it should be gently wiped away with a soft linen rag, dipped in warm milk and water. A tepid solution of alum, four grains to the ounce of water, should then be carefully injected into the eyes, and this cleansing and injecting should be repeated every twenty minutes or half an hour, day and night, until the disease is brought completely under control. The application may then be diminished in frequency, but should not be entirely remitted until every symptom of discharge has disappeared. So long as the complaint continues at all severe, the most careful attention should be paid to the injection of the lotion at the proper intervals of time: neglect of this point will entirely defeat our efforts, and surely bring with it destruction of vision. The eyes of the child are at the mercy of the nurse, and upon her patient and unwearied vigilance must we depend for their preservation.

The common unguent. cetacei, or an ointment composed of one drachm of ung. hydr. nitratis, to eleven ounces of lard, may be applied with advantage to the edges of the lids, to prevent their agglutination together. If the bowels are confined, a drachm of ol. ricini will be beneficial; and the mother, or person who suckles the child, should regulate her diet, and abstain from wine, spirits, or other stimulating fluids. Should the swelling and inflammation of the palpebra be very severe, speedy and decided relief may be obtained by the application of one or two leeches; care being taken that the bleeding is checked as soon as the desired effect has been produced, in order that the powers of the infant may not be brought down too low.

It would be foreign to my purpose to enter more fully into the details of the symptoms, effects, &c. of purulent ophthalmia; for them I must refer to the standard works of Tyrrell, Lawrence, Mackenzie, and others. The following cases afford good illustrations of the disease in question:—

CASE I.—*Staphyloma and Nebulous Corneæ.*

M. A. Finson, æt. 7, was brought by her mother to the Royal Ophthalmic Hospital, Moorfields, to know if there was any chance of her daughter recovering her sight.

The mother stated that on the third day after birth, the usual symptoms of purulent ophthalmia commenced; great discharge from the eyes, and swelling of the lids. The medical man who attended her during her confinement desired that the infant's eyes should be washed occasionally with a lotion which he gave her: this was done, but without particular benefit being derived, and the disease, as the mother described, "seemed to wear itself out." At the expiration of a month she attracted the attention of the surgeon to what she called "a skin growing over the eyes;" but he said it was "of no consequence." The mother not feeling satisfied, in a short time requested him to look at them again; which he did, and then desired her to take the child "to some regular eye-doctor."

At the expiration of three months from the commencement of the attack the child was brought to Moorfields; but disorganization had proceeded so far, that nothing could be done.

The condition of the eyes, after the lapse of nearly seven years, is as follows:—The left is partially staphylo-matous, and all trace of cornea is lost in an opaque white membrane, shaded off in parts into a bluish grey. The right globe is partially atrophied, the place of the cornea being occupied by a similar dense opaque membrane, which is supplied by two or three large tortuous vascular trunks: this eye is totally blind, but a glimmering of light can be perceived with the left. The case, however, was perfectly hopeless.

CASE II.—*Collapse of Globe and Cuticular Conjunctiva.*

Isabella Blandy, ætat. 20, an intelligent young woman, applied at Moorfields for advice on the 12th July. Her mother accompanied her, and stated that on the third day after birth a purulent discharge commenced from the right eye, and soon after the left became also affected. The nurse most scandalously concealed this circum-

stance for a week, but at the end of that time a surgeon was consulted, who thought badly of the case, but ordered a rag wetted with an infusion of poppies to be constantly applied to the eyes, and a leech to the right temple. The eyes continued bad for three years, when collapse of the right globe occurred; and it is now completely shrunken up.

In the left, symblepharon has taken place, the conjunctivæ of both lids having become inseparably connected to that of the globe, leaving a considerable portion of the eye exposed. The consequence has been, that the conjunctiva covering the cornea and sclerotica has become perfectly dry and cuticular, inducing hopeless blindness; for although the patient can distinguish light from darkness, which would indicate that the retina is sound, the condition of the external coverings preclude all expectation of benefit from treatment or operation.

CASE III.—*Destruction of the Right Eye.*

Keziah Taylor, ætat. three weeks, was brought July 27th. The mother stated that on the fifth day after birth a considerable discharge of thick yellow matter began to flow from the left eye, the lids becoming much swollen. On the eighth day the right eye was attacked in a similar manner, and in a short time the discharge became more profuse, and the swelling and inflammation of the lids greater than in the one first affected. The mother applied to a surgeon on the day after the commencement of the attack in the left eye, who ordered a lotion, and two leeches to the lids; and on the day that the right became bad, two leeches were applied to the palpebræ of each eye; the lotion being merely directed to the external surfaces of the lids, and not introduced into the eye.

Upon examination on the above date the tumefaction of the lids was found to be so great, that it was impossible to obtain a view of the condition of the eyes, a profuse purulent discharge gushing from them, and streaming down the cheeks, whilst the cries and restlessness of the child bespoke the severity of its sufferings.

The eyes were directed to be frequently bathed with warm water, to

cleanse the lids from the thick crusts which adhered to them, and the lotio aluminis to be injected into the eyes every ten minutes.

August 2d.—A great improvement has taken place; the swelling and redness of the palpebræ have nearly disappeared, and the child opens the eyes voluntarily. Upon examination, however, the right was found to be destroyed; collapse of the globe having followed sloughing of the cornea, and evacuation of the humours.

There is some opacity of the cornea of the left eye, but it is going on favourably; the discharge being greatly lessened, and inflammation diminished.

To continue the treatment.

6th.—Much better: in fact there is now scarcely any discharge, and the left eye is nearly free from inflammation.

The lotion to be applied three or four times a day.

10th.—The patient is now convalescent.

CASE IV.—Ann Howes, ætat. three weeks, was brought July 1st. Purulent matter, we were informed, began to flow from the right eye three days after birth, and on the fifth day the left eye was attacked.

At the period of application, the lids of both eyes were enormously swollen, their margins encrusted with dried matter, and a profuse discharge of thick yellow pus flowed from between them. With some difficulty a view of the eyes themselves was obtained, and it was ascertained that the corneæ were upon the point of sloughing, from the extent of chemosis which existed.

The lids were directed to be carefully cleansed, and the solution of alum to be injected into each eye every quarter of an hour.

5th.—Discharge and chemosis much less, and corneæ regaining their natural appearance.

To continue the treatment.

12th.—All the symptoms have disappeared; the lids are perfectly natural, and the eyes free from inflammation, bright and clear.

REMARKS.—Here was a case in which the eyes were in great peril, but the mother attended strictly to our instructions, and bestowed the greatest pains upon washing and cleansing them

as directed. It is instructive, as proving the efficacy of this simple treatment, in overcoming a very formidable attack.

CASE V.—Elizabeth Costin, æt. five weeks: applied July 9th. The mother states, that, on the third day after birth, a purulent discharge commenced from the left eye, and the lids became swollen and puffed up, the child at the same time being restless and crying much. On the 5th day the right eye became similarly affected; the lids of both being exceedingly swollen, and the discharge great. The medical man in attendance ordered a lotion to be applied occasionally, but of what description is not known.

At the present time the swelling is not very considerable, and the lids are of nearly their natural colour; but a free discharge of thick straw-coloured purulent matter pours from the eyes. The mother states, that the quantity of discharge varies from day to day. I could not examine the eyes; but from the healthy aspect of the lids, their natural colour, and the absence of that corrugated appearance which generally exists after destructive inflammation, together with the fact of the secretion being thick and of a pure straw colour, instead of thin, sanious, or mixed with particles of blood, from these circumstances, I judged that the eyes were safe.

The mother was ordered to wash them carefully whenever matter accumulated, to inject the solution of alum every hour, and to anoint the edges of the palpebræ with unguent. cetacei.

July 13th.—Going on exceedingly well; the quantity of discharge much diminished, and the lids regaining still more their natural appearance.

To go on with the treatment.

16th.—The discharge is now very inconsiderable, and the eyes are brilliant and healthy in appearance. The mother was directed to continue the injection occasionally, until all discharge should have ceased. The patient did not return again.

CASE VI.—Sarah Stopher, æt. eight days, was brought July 27th. Profuse discharge of purulent matter commenced from both eyes three hours after birth. The lids soon after began to swell; and on the third day the swelling and inflammation were very

intense, and the discharge exceedingly copious. The treatment adopted had been frequent washing of the eyes with warm water and the milk of the mother.

Upon examination, the lids appeared much tumefied, but not very red; and a thick yellow discharge rushed from each eye on an attempt to open them. The child was very irritable, and intolerant of light.

The eyes were directed to be frequently cleansed, and the lotio aluminis to be injected every quarter of an hour.

Aug. 3d.—The tumefaction has entirely left the lids, and they have resumed their natural appearance. The discharge is much less, and the child opens the eyes voluntarily.

6th.—Patient not so well, and the discharge has increased, in consequence of strict attention not having been paid to the periodical application of the lotion. The necessity of this was impressed upon the mother, who was directed to cleanse the eyes, and inject the lotion every half hour.

10th.—The good effects of attention to the points enforced have been made manifest by the eyes having now resumed their natural appearance. The discharge is scarcely perceptible; but, to complete the cure, the mother was directed to continue the treatment until it has entirely ceased.

I might bring forward other cases, but these are sufficient to illustrate the symptoms and effects of purulent ophthalmia, as also the simplicity and efficacy of the means by which it is to be subdued. It will be seen that, in the three latter, although they were severe, the treatment of astringent lotions, cleanliness, and attention, were sufficient to overcome the disease. The three former examples present melancholy illustrations of its destructive effects, and convey useful lessons even upon paper: had the originals been seen, they would have made a lasting impression upon the mind.

In conclusion, I would impress upon all my readers the importance of cautioning mothers and nurses as to the contagious nature of purulent ophthalmia. They should be particularly warned not to use, or apply to their own eyes, any cloth or rag which may have been in contact with those of the

child. It is only a short time since I saw an instance of an eye being destroyed from neglect of this precaution: the disease was communicated to the mother, from her having applied to one of her own eyes the handkerchief with which she had been cleansing those of her infant.

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Regent's Park.

SOME ADDITIONAL CASES AND
REMARKS ON
COMPOUND FRACTURES OF THE
CRANIUM.

BY JOHN CHARLES HALL, M.D. F.L.S.
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[*For the London Medical Gazette.*]

IN the MEDICAL GAZETTE, Nov. 27, 1840, I ventured to offer some remarks on this important class of cases. It has been my lot to meet since then with some additional information on this subject, and I now venture to lay before your readers the result of such investigation. It will, however, be well to give, first, an outline of the following cases:—

CASE I.—Master W., a little boy, æt. 10, in passing through the streets of Kensington, near some men playing at quoits, received a blow upon the left temple from one of the quoits, which struck him with considerable force. He was carried home senseless. When I saw him, about half an hour after the infliction of the injury, he was cold and shivering, and complained of sickness. Shortly afterwards the contents of the stomach were ejected, and the vomiting continued during the night. There was a large open wound of the scalp, about two inches in length; and on removing the coagulated blood, a fracture with depression could easily be distinguished. The boy described the pain in the head as considerable. The wound in the integuments being carefully brought together, the boy was put to bed, and cold water ordered to be applied constantly to the part.

R Hydrarg. Chloridi, gr. iv. hac nocte.

18th, 8 A.M.—Has passed a very restless night; complains of great thirst; constant sickness; pain in the head; skin hot; tongue dry and coated; pulse 120.

Apply ice to the head. V.S. ad 3viii.
 R Sodæ Sesquicarb. ʒj.; Syr. Aurantii, ʒj.; Aquæ, 3x. M. ft. haustus
 c. coch. med. succi limonis quâque
 3tiâ horâ sumendus.

Vespere.—Sickness less constant than in the morning.

Four grains of Calomel at bed-time.

19th.—No change.

20th.—Complains of pain in the head; skin hot and dry; has been restless, talked in his sleep, and is constantly asking for something to drink. Bowels open; tongue white and coated.

Continue the ice to the head. To take an aperient every four hours.

21st.—The tongue still continues white and coated; pulse slow and laboured; complains of pain in the head; skin hot and dry; bowels open.

Continue the aperient mixture, and ice to the head.

22d.—More pain in the head.

V.S. 3vj.

The wound, which has been closed with adhesive plaster, looks puffy round the edges; there is also considerable tenderness of the scalp, and great intolerance of light. A few punctures were made with a lancet, and a poultice was applied over the seat of injury.

23d.—Skin still hot and dry; tongue coated; pulse 100.

Apply six leeches over the part originally injured; to take six grains of calomel at bed-time, and a draught, composed of senna, sulphate of magnesia, and infusion of gentian, in the morning.

24th.—Much better. The boy from this day continued to improve, and is at this moment as well as before the accident.

I have seldom seen a more severe injury than this. The fracture extended from the edge of the frontal to the squamous portion of the temporal, and thence to the left parietal bone. The wound in the integuments was two inches in length, the scalp much bruised, and the periosteum in several parts torn from the cranium. The depressed portion of bone was at least one inch and a half in length, and driven down below the level of the cranium more than the eighth of an inch. So apparent was it, that a woman who was standing by remarked that "his head was driven in." The treatment, as above

stated, consisted in cleansing the scalp, and carefully closing the wound with sticking-plaister, and applying very slight pressure with a bandage; also ice and evaporating lotions. It is necessary to be very careful in closing the wound. Every particle of air should be excluded, and every means employed to cause the parts to heal, if possible, by the first intention. In this case the boy was not bled the first day, because he had lost much blood from a branch of the temporal artery, which was divided by the accident, and my friend Mr. Carrick thought with me that a sufficient quantity of blood had been lost.

CASE II.—A girl, about fourteen years of age, had a fracture, with depression of part of the temporal and parietal bones. The girl was kept quiet; bled, placed in bed, cold applied to the head, and aperients administered. She got well without any bad symptoms.

CASE III.—A Frenchman, æt. 54, was brought to the Hôtel Dieu, having received a blow upon the frontal bone, which was fractured. The scalp was much bruised; the skin around the wound puffy and discoloured, and the wound in the integuments ragged, as though the injury had been inflicted by the rough edge of a brick-bat. I could not, however, learn how the injury was sustained. He complained of soreness in the wounded scalp, which was "*all he cared for.*" He was bled, much against his inclination, and kept upon a low diet; an aperient being given every morning. When I left Paris the wound had healed; and I have no doubt in a day or two the man would be sufficiently recovered to leave the hospital.

CASE IV.—This case will shew the necessity of strict attention to diet after injuries of this nature. Every thing tending in any way to derange the system must, for months after fractures of the bones of the head, be avoided, or the most fatal results may take place.

Mary Burton, a little girl, æt. 13, the daughter of a small farmer, fell down in her father's yard upon the edge of a stone trough, out of which the cattle were supplied with water. The integuments over the right temporal bone were wounded; and on introducing the finger, a fracture, with some degree of

depression was detected. My friend, under whose care she was placed, adopted the plan of treatment already recommended, and in a few weeks she had apparently completely recovered, with the exception of some little tenderness of the scalp. At this period, (about five or six weeks after the infliction of the injury), in company with some young friends, she partook of a quantity of unripe fruit, sweet-cake, and "made wine." Two days after this she was seized with rigors, followed by great pain in the head; the scalp over the original seat of injury being so tender that the slightest examination caused considerable pain. At this period I was called to attend in consultation with the family surgeon. After a very careful examination of the bone under that portion of the scalp where the bone had been fractured, we could discover nothing that induced us to suppose that the symptoms present altogether depended upon the local affection, and therefore our attention was directed to the system generally. The bowels were distended, the skin hot and dry, the tongue furred, and the slightest noise appeared to cause an increase of pain. She dozed a good deal, remaining in a half-comatose state; now and then asking for water. Six leeches were applied to the temples, and six grains of calomel and four of rhubarb given immediately, followed by a draught of infusion of senna and manna. This plan of treatment was continued until the bowels were thoroughly emptied of a quantity of undigested food, mixed with dark foetid fæces. Under these remedies a manifest improvement took place. The only local treatment, in addition to the leeches, was an evaporating lotion, which was constantly applied to the head. The little patient gradually recovered. When the bowels were unloaded, the tenderness vanished not only from the epigastric region, but also from the head. The skin, however, remained yellow, the hands moist, and the system was evidently still suffering. Some hydrag. c. creta, and rhubarb, at bed-time, with the infusion of cheryta and soda during the day, exercise, and a strict attention to diet, gradually worked a change, and we had the pleasure of seeing our little patient quite restored to health.

I have not recorded the above cases

from any supposed novelty they may possess; on the contrary, the student of any of our London hospitals may probably witness similar accidents every week, and he will not be long in practice before he is called to injuries of this nature. No class of cases are more important: first, from the frequency with which they occur; and secondly, from the fatal results with which they are too often attended. But are such cases to be disregarded because they are common? Not at all. I am well aware that students run to see the *great operations*, and read all the *curious cases* in the medical publications of the day. *Cui bono?* Let me here repeat the remark of one of the most distinguished surgeons of the present day, Sir B. C. Brodie. When I began my professional studies I became a pupil under this surgeon at St. George's Hospital. Before doing so I called upon him, having a letter of introduction from a friend. "Let me advise you, sir (said he), to attend regularly at the hospital. Never mind cases of hernia and stone, at first, but look at the *cut fingers*, *ulcerated legs*, and *broken heads*; they are of the most importance; you will be called in early life to attend them, and they are the foundation on which to build your future reputation."

Looking at cases of fractured bones of the head, and carefully watching the treatment of them, I soon found that *many cases died*, in which the trephine was applied, and that *many recovered* in which this operation was not performed. It therefore was necessary, in the next place, to inquire if we are justified in performing this operation in the absence of symptoms denoting pressure upon the brain. The result of an experience of some years induces me now to conclude, that in the great majority of cases of compound fracture of the skull with depression, we ought not to trephine unless it appears clear that the brain is suffering from pressure. In that case, of course, the operation ought at once to be performed. True, the chances for the poor patient are few; still many cases have recovered after the performance of this operation. In all cases, for example, where the injury is extensive, the wound in the integuments large, and the bone broken into several pieces, even in the absence of symptoms, some interference may be necessary; some of

the pieces may be picked from the brain, and others elevated, and this without additional injury to the scalp.

CASE V.—James Betts, æt. 10, was endeavouring, during the month of July 1841, to get upon a cart belonging to Earl Spencer. He fell with his head between the cart and the wheel, and was drawn onwards some distance, his head acting as a wedge, and causing the wheel to be locked for some yards. The bone was not fractured, but the parts were bruised and discoloured. I saw him shortly after the accident, and although every care was taken, a rather sharp attack of fever came on. In truth the symptoms were much more severe than in some of the cases of compound fracture already mentioned.

If, then, injury done to the scalp and bone, although there is no fracture, produces, not unfrequently, exfoliation of the bone, inflammation of the brain, effusion between the arachnoidea and dura mater, abscess, &c., it does appear that the operation of trephining is more likely to produce, than to prevent, inflammation of the brain; and all the inflammatory symptoms which are expected to succeed to all violent blows on the head are doubtless aggravated by the additional injury inflicted by the operation; to say nothing of the danger of the operation itself. As my friend Mr. Lane, the lecturer on anatomy, well remarks, “if you were to trephine twenty men, in a state of health, how many would survive the operation? I apprehend not many.”

The above cases, in addition to numerous others that have been published, prove that many persons recover after their skulls have been fractured, and a portion of bone driven upon the brain. The degree of pressure the brain can sustain doubtless varies in different individuals; for in some, slight pressure produces great inconvenience; in others, the greatest depression is observed to cause little or no uneasiness. It will be well also, in every case, carefully to distinguish between the symptoms arising from concussion and compression. And this may easily be done, if some little time is given in order to recover from the stupefaction a severe blow is almost certain to produce. Now, as the effects of concussion gradually abate, a very little delay will, in every case, enable the surgeon correctly to ascertain the pre-

cise nature of the mischief, and to act accordingly.

Where the patient retains his faculties, nothing is necessary except to carefully close the wound, keeping the head cool. Rest, confinement to bed, purgatives, bleeding, and a very strict adherence to the antiphlogistic plan, will of course be required. If, however, from some peculiarity in the system, from some peculiar disposition of the brain, the torpor continues, then, notwithstanding its danger, the patient must not be allowed to die without an effort to save him. The trephine must be used, and the bone raised.

It appears clear that the ancients trephined much too frequently; while some, in the present day, witnessing the fatal results attending this operation, too generally condemn the practice. The above cases, however, clearly show that it would have been useless to have trephined in them, and that the brain will bear a considerable degree of pressure without injury. The cases mentioned by Abernethy, Bell, Lawrence, Hill, Latta, and others, prove this opinion to be correct.

In dubious cases it will be well to wait, rather than perform the operation at once; for be it remembered it is so dangerous a procedure, that nothing can justify the use of the trephine but the most urgent necessity. The bone once removed, the natural support of the brain is taken away; and if inflammation comes on, the parts contained in the cranium will be considerably increased in volume, by the preternatural fulness of their vessels, and subsequent effusion of fluids. The dura mater gives way; the pia mater, now exposed, takes on a more inflammatory action; this too ulcerates; and the last barrier removed, a fearful state of things may be looked for. On examining the wound, a soft dark claret-coloured mass is seen projecting from the dura mater, and shortly afterwards a true *hernia cerebri* is rapidly produced.

In all cases in which it is necessary to use the trephine, the augmentation of the contents of the cranium ought to be prevented by the most copious evacuations. The strictest attention must be paid to the unfortunate patient. A cough must, if present, be subdued if possible; for it endangers the bursting of the dura mater by violently forcing the blood to the head.

The crying of the child, and the struggle of the delirious adult, have ere now produced the most fatal results.

Whilst it is our duty, on the one hand, not to shrink from the performance of this operation when required, the remembrance of the dangers attending it, on the other, will cause us carefully to abstain from interference in the absence of symptoms denoting pressure greater than the brain can sustain. It is pleasing, however, to reflect that frequently, by strict antiphlogistic measures, every danger is avoided, and our patient restored in perfect health to his family and friends.

Bath Place, Kensington,
August 1841.

MIDWIFERY CASE.

To the Editor of the Medical Gazette.

SIR,

SHOULD you think the following case sufficiently interesting, I shall feel obliged by your giving it a corner in your valuable periodical.—I am, sir,

Your obedient servant,

JAMES KIRK, Surgeon.

405, Gallowgate, Glasgow,
July 2d, 1841.

June 3d, 1840, at noon, was called upon to attend Mrs. Mac——, a stout healthy woman, aged about 35, in the eighth month of her sixth pregnancy. On arriving found her in labour; learnt that the waters had been discharged at three o'clock this morning; and discovered, on examination, that the os uteri barely admitted point of finger. A point of bone felt presenting. No foetal motion has been perceived for the last three weeks. Called in the afternoon, and again at ten at night; patient has been going about; pains weak and trifling; labour has made little progress.

4th, 11½ A.M.—Labour has been going on slowly since last night; os uteri about three parts dilated; pains pretty strong; elbow presenting. Gave ninety drops of laudanum, and waited some time, in order to permit of its operation allaying the uterine contractions; then introduced the right hand, and searched for the feet; but finding the pains still powerful, ordered thirty additional drops of laudanum. Found, what I imagined to be the bend of the knee,

and brought it down; but on doing so it proved to be the elbow of the opposite side, upon which returned it immediately.

I again endeavoured to find the feet, but being quite unable to reach them, in consequence of the complete evacuation of the waters, and the firm hold which the uterus had upon the body of the child, and its closely embracing every inequality of its surface, without having recourse to a degree of force, which I considered both unjustifiable and dangerous, I endeavoured to bring down the head in the natural manner, and by getting one finger in the mouth and another in the orbit, I was enabled to bring it down a considerable way. But by this time my hand had become so cramped and powerless, from the uterine contractions, that I was obliged to withdraw it; and on introducing the other, I found, to my great mortification, that the head had again receded.

Completely foiled in both of my attempts, and quite wearied by my efforts, which had now been continued for a considerable time, I, in this dilemma, recollected that the growth of the child had been arrested at the end of the seventh month; that the mother had borne children of the full natural size; and feeling the head to be very soft, flaccid, and easily compressible, I resolved to bring it down doubled on the chest. I accordingly brought down first one arm and then the other, and the pains having entirely ceased, gave two small doses of the ergot, and waited some time. Brisk uterine action followed. I next got my finger hooked round the neck, and making slight traction during the pains, the head was in a short time expelled, with the face doubled on the chest.

The labour was then speedily terminated, and the woman recovered without a single bad symptom. The child presented the usual macerated appearance of one that has been dead for some time; the bones of the cranium were widely separated, and the scalp felt like a bag containing loose shells. I should certainly have preferred to terminate this case either by turning or bringing down the head, but finding it impossible to accomplish this, after steady, patient, and long-continued efforts of several hours duration, I consider it to have been fortunate for my patient that there still existed a third

mode by which her labour could be terminated; and of which I took advantage, only when I became convinced that it could not be concluded in the former way.

Of course I should not have thought of acting as I did, if the child had either been alive or come to the full size.

CASE OF SUDDEN DEATH.

To the Editor of the Medical Gazette.

SIR,

IF the following case is worth a place in your GAZETTE, it is very much at your service.

Edward Earl, seaman, aged 23, of a full habit, tall, and dark complexion. He appeared at the hour of visit on the morning of the 30th of May last, complaining of oedematous swelling in the feet and ankles: the insteps puffed, glossy, and pitting; the ankles, and a little upwards, swollen slightly. Complained of stiffness in the parts, but no pain. It had been observed for a few days only; it had increased little, and, for which he could assign no cause, the swelling was confined to the feet entirely. The thighs were free, as well as the arms and body. He complained of nothing further; his appetite he said was good, he slept well, bowels regular, was free from all pain, appeared to breathe in a natural way, and had a pulse beating steadily at 80.

Although the symptoms appeared trivial, and the cause temporary, it occurred to me at the moment that it might originate from some deeper source, from some internal obstruction about the centre of circulation; but nothing could be elicited. Respiration seemed natural and easy, with nothing remarkable either in the force or frequency of the heart's action: he appeared, in short, a man in health, full and muscular. A dose of aperient medicine was prescribed, and he disappeared, and, like many others whose cases seem trivial or unimportant, it was not entered.

The following evening, about ten o'clock, the ship's corporal tapped at my cabin door, and said a man had been taken very ill, and was carried into the bay. I went up immediately,

and at the door met the senior assistant, who said the man was dead. He was lying on the deck, and the people who had carried him standing about. The information was perfectly correct; life was extinct; there was not the slightest trace of vitality.

The information drawn from his mess-mates was, that he had gone to bed as usual, about nine o'clock; and that a little before ten he got out, and went to the head; that while there he complained to one or two people, who happened to be on the spot, of pain in his breast, and difficult breathing; and that after conversing for a few minutes, he suddenly staggered, and fell, and was by them conveyed below; that he had for four or five days at times complained of his breathing, but that otherwise there was no alteration: he ate, and drank, and slept, as usual; that he was jocular, and did his duty as was his custom; and that he had been playing draughts in his mess at eight o'clock.

Autopsy.—The body was opened sixteen hours after death. The thorax and abdomen seemed rather high, rounded, and prominent, but otherwise it appeared full, muscular, and well formed. The thorax was first entered: in dividing the costal cartilages the knife penetrated accidentally the pleura; a stream of serum immediately sprung out, rising by the surrounding pressure some height from the surface, of a light straw colour, and translucent. The sternum being raised with the adjoining cartilages, the internal parts came into view: the lungs, of a light grey, colour of ordinary size, soft, and free from induration, as well as all inflammatory sign, floated lightly in a fluid of the same description: on the left side three narrow bands extended between the lung and costal pleura, and were apparently of some standing. The pleuræ on both sides were diaphanous, and of a natural hue, two or three spots only excepted, which were reddened slightly. The heart was small, but healthy, and free from induration, with the valvular apparatus complete; the serum effused was of a light straw colour, and computed to be about three, and some thought four, quarts. The stomach was inflated intensely, and pressing upwards, as well as the liver, which was large, but otherwise healthy. The diaphragm

was depressed, by the quantity of fluid, forming a kind of hollow and rounded sac. In the abdomen traces were evident of inflammatory action; the omental surface, as well as the intestinal and gastric, were matted closely with surrounding reddened inflammatory patches, but no effusion.

The deduction to be here drawn is the discrepancy which obviously subsisted between the symptoms of the disease and the general state: this man, although in the most dangerous condition, continued, without any equivalent symptom, to do his duty, to eat, and sleep, and work as usual; and had been on the very day of his death away in a boat, and without any complaint, or that of the most trifling in appearance; exhibiting clearly that symptoms, and the disease from which they proceed, are not in unison always, and how far the former may proceed without its being appreciable by the latter. The man was on the sick list five or six weeks before with what appeared catarrh, the symptoms of which readily yielded to bleeding, blistering, and so forth, and he returned to his duty; but it is probable that the foundation of this was then laid.

This case shows how guarded all should be in prognostics about the issue of chronic or obscure diseases, but above all naval or military surgeons, on the soundness of whose judgment, or their prudence the punishment of individuals often depends. It is possible that this man might have been convicted of some offence, and brought to punishment two days before his death. He might have complained, or he might not; and it is possible that complaints at such a moment might be overlooked. However that might be, if punishment should then have been enforced, the probable result would have been his sudden and unexpected demise. Too much caution can never be exercised in the investigation of all points in any way dubious, particularly in situations which involve such fearful results.

I am, sir,

Your obedient servant,

J. WILSON, D. Surgeon.

H. M. S. Vanguard, Malta Harbour,
July 8, 1841.

CASE OF SUBACUTE BRONCHITIS,

WITH PLASTIC TUBULAR SECRETION:

WITH A DRAWING.

To the Editor of the Medical Gazette.

SIR,

SHOULD the following case, of what I believe to be an uncommon variety of bronchial inflammation, be considered by you worthy the perusal of your readers, I shall feel its insertion as a favour.—I am, sir,

Your obedient servant,

W. F. RANKING, M.D. Cant.,

Physician to the Suffolk General Hospital.

Bury St. Edmunds,

Aug. 12, 1841.

I was summoned on the morning of the 2d of June last to meet Mr. Robinson, of Mildenhall, in consultation upon the following case:—

The patient, a gentleman, ætat. 20 years, of a nervo-sanguineous temperament, was seized on awaking by a paroxysm of coughing, accompanied by expectoration of sputa tinged with blood. He had suffered three separate attacks, previous to the present one, of what was considered to be pure hæmoptysis, and dreaded accordingly as the forerunner of phthisis. He appears to have been for some time subjected to dyspnœa and palpitation upon exertion; and catarrhal attacks, to use his own expression, always “settled upon the lungs.”

The first seizure of his present ailment occurred in January last, a second occurred in February, and a third in April, all of which yielded to topical bleeding and digitalis. The present attack came on rather unexpectedly: our patient having been noticed the day before to be in excellent health. The expectoration consisted of masses, which, upon casual observation, appeared to be mucus tinged with blood, and was expelled by an effort more resembling “hawking” than cough. Upon more minute inspection these sputa were seen to consist of a whitish membrane, distinctly tubular, and accurately moulded to the form of the bronchial tubes, even to their most minute ramifications. The consistence of these varied; in some portions the membrane was tough and opaque; in others thin, and raised into minute pouches by bubbles of air. (See figure.)



The blood was small in quantity, and could be easily removed from these tubes by agitation in water.

There was little disturbance in the system beyond that produced by mental agitation; the appearance of the blood having given rise to the greatest anxiety both to the patient and his friends. The pulse was 80, and soft; the skin cool, and tongue natural; and there was little or no pain in the chest. I made a very careful stethoscopic examination relative to the existence of the tubercular deposit, but could not discover any grounds of apprehension. The chest was of an unusually round figure, and gave a clear sound on percussion throughout. The respiratory murmur was of an intensity almost puerile, and audible every where, with the exception of the neighbourhood of the larger tubes, where it was marked by a sibilous râle. There was considerable præcordial dulness, and extensive but not forcible impulse. There was less difference than natural in the "*timbre*" of the first and second sounds. "The diagnosis I formed from these was, subacute inflammation of the bronchial mucous membrane, with plastic secretion; many tubes obstructed partially by membranous deposit; and a dilated heart of less than average power."

Under the impression that digitalis, which had been prescribed on former occasions, was not suitable to the state of the heart, I gave him internally the acetate of lead—a remedy which has been found so useful in bronchitis, by Henderson, guarded in the usual manner by acetic acid draughts: aperient medicine had been exhibited before my

visit. Externally to the chest I applied the Unguent. Acet. Potass. Tart. The acetate of lead was suspended after forty-eight hours, and eight-grain doses of alum were substituted.

Under this treatment considerable amendment was perceptible on the third day, and by the end of the third week every unpleasant symptom had vanished.

The patient now remains in average health, and improves daily under a more generous diet than he had been allowed by his former medical attendant, and the daily use of tepid salt-water sponging, followed by friction with horse-hair gloves. Upon every tendency to catarrh he applies the Linimentum Terebinthinæ to the chest.

The secretion of a membranous substance by the pulmonary mucous membrane is familiar to every one, as it occurs in croup; in fatal cases of which disease the bronchial tubes are often found to be filled with the same plastic material as is furnished by the lining membrane of the trachea. But the production of membranous secretion in the lungs of the adult, and unconnected with tracheal disease, must be considered as a rare event.

We have, however, several instances on record, by Barthollini, Ruysch, Tulpius, and Morgagni, under the names of bronchial polypus and bronchial worms.

Hippocrates*, also, probably witnessed something of the kind in the case of Phericides, whom he describes as spitting up "*γαλακτώδεα*," "white milky substances."

Dr. Bergen, of Frankfort on the Maine, has left us a description of an epidemic catarrh, accompanied by the expectoration of membranous tubes, which occurred in 1759. "*Hanc tristem experientiam in propriâ filiâ feci, in quâ hoc singulare simul se obtulit phænomenon, quod ante mortem, tussi et screatu rejecerit tubulum membranaceum. Hunc tubulum judico esse portionem membranæ tubulosæ per ramos bronchiorum, durante morbo, generatæ.*"

Another case is related by Dr. Warren†, who is generally thought to be the first person who has given an

* De morbis popular. lib. vii. ss. xli.

† Med. Trans. vol. i. Art. 16.

accurate description of the disease. It will be seen, however, that he did not entertain any more concise ideas concerning it than Berger, who preceded him by many years. The case is as follows:—

A girl, æt. eight years, of strumous habits, was suddenly seized with dyspnoea and cough, which yielded to medical treatment. At the end of six weeks she had a second attack, accompanied by night sweats; the symptoms ceased upon the expectoration of what Dr. Warren calls “a large polypous concretion.” The girl suffered several relapses, but was at length freed from them entirely by the formation of an abscess connected with caries of the os calcis.

Baillie had never met with a case of what he calls “*bronchial polypus*,” but had seen preparations of it.

Cheyne describes two forms of “*bronchial polypus*,” one of which is evidently nothing more than the fibrinous portion of blood effused into the bronchial tubes. The instance given by Laennec, as occurring in the progress of a case of phthisis, was of the same nature.

Dr. Casper* has published the case of a girl, æt. twelve years, also of strumous constitution, who, in the course of inflammatory catarrh, coughed up “a whitish yellow polypous body, of a tenacious character, and corresponding to the bronchial ramifications.”

A case will be found in the *Med. Repository*, vol. xviii., by Mr. Iliff, and also in a memoir read before the Royal Academy of Medicine, on obliteration of the bronchial tubes, by M. Reynaud.

The last case I shall mention is one published by Dr. Starr†, of Daventry, under the denomination of “chronic croup,” and which exactly resembles the case of my patient. A girl, ætat. 22 years, complained of pain in the chest, with a sense of general oppression. Membranous tubes accurately moulded to the bronchial tubes, and, as in my case, tinged with blood, were expectorated. The blood was doubtless effused from small vessels ruptured in the exertion necessary to dislodge the tenacious sputa. Authors are far from agreeing as to the precise patho-

logical condition of the bronchial mucous membrane which gives rise to the plastic secretion. There are two principal and opposite opinions: one, which attributes the production of membrane, instead of the ordinary more fluid secretions of inflamed mucous tissue, to the existence of a high degree of irritation; another, which ascribes the phenomenon to an excess of the albuminous constituent of the blood. The generality of writers are in favour of the first opinion; the latter is embraced, among others, by Copland*. The question is discussed at some length by Andral†, by whom it is decided that a high degree of irritation is not sufficient *per se* to cause the secretion of plastic membrane; but that there must be, in addition, some special conditions of innervation or sanguification.

It appears to me that neither of these opinions is the correct one, and that the true explanation is still to be sought for. If the more frequent formation of plastic membrane in children than in adults be due to the existence of a larger quantity of albumen in the blood of the former, then ought we to see such productions the common consequence, in them, of inflammations of the mucous tissues. Such, however, is not the fact. One half, at the least, of the diseases of infancy and childhood, consist of irritations, of greater or less intensity, of the pulmonary and gastro-intestinal mucous membranes. Yet in cases only of croup and diphtherite do we see the production of plastic membranes—cases which, numerically considered, are rare.

The same objections hold good against the opinion which refers this peculiar secretion to the intensity of the vascular excitement. If the degree of irritation were the true cause of such secretion, we ought to see it more frequently among the numerous cases of bronchitis and gastro-enteritis which present themselves to our notice. In our patient, as well as in that of Dr. Starr, every symptom opposed the idea of high vascular excitement; yet was the formation of the membrane of the most perfect kind.

It is, however, in this as well as in many other points connected with

* Wochenschrift für die Gesamte Heilkunde.
† MEDICAL GAZETTE, Feb. 7, 1840.

* Vide art. Croup, Copland's Dictionary.
† Anatomie Pathologique, p. 484.

our difficult science, far more easy to find objections to any particular opinion, than to frame a better; nor in the present instance do I pretend to do so.

The treatment pursued was directed by the two following indications:—1st, to allay the existing irritation; and 2d, to prevent its recurrence. The first end was accomplished by counter-irritation, and the internal use of the acetate of lead and alum, the sedative and astringent properties of which restored the bronchial capillaries to their normal condition. The second I hope to accomplish by judicious regimen, by sponging and friction of the chest, combined with the constant use of flannel, and by having recourse to cuticular irritation upon the slightest occurrence of catarrhal symptoms.

MEDICAL GAZETTE.

Friday, August 20, 1841.

“Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri; potestas modo veniendi in
publicum sit, dicendi periculum non recuso.”

CICERO.

DETECTION OF ARSENIC.

THE report lately presented to the French Institute, and of a part of which we have given a translation in our present number, is, with all the circumstances that preceded and have followed it, full of interest and importance. Without entering into any of the coarse discussion, as much opposed to science as to decency, of which the Academy of Medicine has, for several weeks past, been the arena, we will mention a few of the points connected with this question. Urged on by two trials of unusual interest which have lately taken place on the continent, and in which the whole strength of the evidence rested on the possibility of determining beyond a doubt the existence of arsenic in those supposed to be poisoned, M. Orfila, whose name already stood highest in Europe at once for skill and for experience in

toxicological investigations, engaged in a most laborious series of inquiries into the whole of the medico-legal history of arsenic. The results which he obtained were, from time to time, communicated to the Academy of Medicine, and his essays received the highest honour that could be conferred upon them, in the privilege of being published in both the Bulletins and the Memoirs of the Academy. In the end he was generally deemed to have conferred a very great boon on science, by having confirmed or discovered several most important facts in the subject of his inquiry. And of these the chief were, the possibility of tracing arsenic in the substance of the tissues of those from whose stomach it had disappeared; the means of distinguishing arsenic which had been carried into the tissues in consequence of poisoning, from that which (as he was believed to have discovered) exists naturally in the bones, muscles, and some other parts, of even healthy men; and the pointing out how, by means of certain *arsenical spots*, in which the arsenic, first separated from the gas that has passed through a Marsh's apparatus, collects, its presence might be determined in smaller quantities than hitherto. There were some other less important points, but these will suffice for the history.

Whether it be merely that his reputation is very high both in France and all Europe, or that he holds a high rank in his profession in Paris, being generally regarded as the leader in the *Ecole de Médecine*—or for any other reason which we cannot suspect—all those who have endeavoured to cast doubt on the accuracy of M. Orfila's investigations have not failed to do so with a most virulent acerbity. They have put forward their corrections of his errors with an insulting tone of anger which is amply sufficient to give an honest man a prejudice in favour of

the object of their attack. But avoiding both this prejudice and, as far as possible, that derived from the long reputation which M. Orfila has deservedly enjoyed, we cannot help seeing that his conclusions were, if not rather hastily drawn, yet certainly too definitely, too positively, set down. For even if M. Orfila himself were not mistaken, or if he had really been able to correct some of the errors into which he had fallen before the publication of their corrections by others, there can be no doubt that the great majority, even of experienced investigators, after reading his memoir, would have entered on the search after arsenic, in a case of suspected poisoning, with several not unimportant errors in their minds. It may be very true that it is *now* possible so to read these memoirs as not to be led astray by them; but this can be done only by picking out, and setting prominently forward, passages and words in them, which, at first reading, receiving no more attention than they were intended to attract, would have made little impression on the mind.

The chief error which, if it be not inculcated in M. Orfila's memoir, is at least to be guarded against, is that of relying on the evidence of the arsenical spots, especially when they are small and thin. For these spots, it is certain, may be imitated by others which are very apt to form when the combustion of the organic matters supposed to contain arsenic, though in reality containing none, has been insufficiently or not properly performed. It is for their having given the due importance to these *false spots* that MM. Flandin and Danger deserve their chief credit. They did not indeed discover them, for these, or spots like them, are described (though with just that unfortunate want of prominence which we have already mentioned) by M. Orfila, under the name of *taches de crasse*; nor is

there any probability that M. Orfila himself ever mistook false for true spots, or allowed himself to be drawn into an important error by them. In their general aspect, indeed, and in all their physical and some of their chemical characters, these spots resemble those really formed by arsenic; but they are not incapable of being distinguished by so experienced an eye as M. Orfila's, and the rules which the report of the Institute establishes for examining them by appropriate chemical tests, are laid down for safety's sake rather than of necessity. Still, the lesson which every one should learn who may be called on for a medico-legal opinion in a case of poisoning by arsenic, is plain enough: it is, never to be content till he has formed a genuine ring of metallic arsenic in his glass-tube, and subjected that ring, or a well-marked arsenical spot, to all the tests for the detection of the metal. For in every such case a medical witness has a double duty to perform: he must not only satisfy himself, but he must be able also to satisfy the doubts of the public, and to secure that the course of justice is not hindered. It is not less to his disgrace than to the public damage, if a guilty man is acquitted because his counsel can throw doubt on the medical evidence; and who can doubt that when reports of these false and true spots find their way, as they slowly will, into courts of law, the imagined difficulty of distinguishing them will be made the strong-hold of defence in every questionable case of arsenical poisoning? We cannot, therefore, too strongly urge upon our readers the propriety of a careful study of the report in all that relates to this part of the subject; for uncertain as the mere physical characters of an arsenical spot may be, nothing can be more undeniable than the evidence which it may be made to afford in every case in which its

quantity is not so small that the appropriate chemical tests cannot be applied to it.

The occurrence of these false spots is generally due, we have said, to the imperfect destruction of the organic matters; and in regard to them M. Orfila certainly placed himself in a position in which it was possible for MM. Flandin and Danger to seem to correct him. There is little doubt also that their mode of proceeding, which will be found still further improved in the report, was in some respects better than his; but the credit which they merit for this is but small: they only saw their improvement after the giant had raised them on his shoulders, and it was not till the more difficult part of the way had been cleared that they came in. They had all the advantages also of that which, if it involved no discovery, was for all credit equal to a series of discoveries,—namely, the establishment by M. Orfila of the extreme caution necessary to be used to prevent the introduction of arsenic into the substances to be experimented on by the various apparatus and tests employed for its detection, but, in fact, themselves containing it. All this M. Orfila worked out in a most admirable manner, and fully established the necessity of either a previous examination of all the materials employed in the investigations, or, as the report recommends, of making a blank experiment, (that is, one into which all the materials except the suspected substance are introduced) at the same time with, or directly after, that on the results of which the ultimate conclusions are to be founded. Here, therefore, is another lesson which we who can look calmly on the matter may draw from the subjects of these vehement quarrels, and another point in which a medico-legal witness will do well to permit no doubt to be thrown on the value of his testimony.

But after all, the highest honour which M. Orfila will draw from these investigations will be due to his having established the possibility,—and yet more, the practicability,—of obtaining arsenic from the tissues of the body, after it has been removed from the stomach, and when, but for his researches, it would have been for ever lost. In this no one can deny that he has discovered a main and important fact; for the certainty of the conclusion to be drawn from arsenic found in the tissues is not less than that derived from the evidence of its being found in the stomach and intestines; and while in one class of cases it furnishes the only possible evidence, it will hereafter afford in all a desirable supplemental proof; for it is not open to objection, from the chance, slight and remote as it may be, that the poison was introduced after death. Whatever improvements in the manipulations for its detection, or in any part of the details of the subjects, others have introduced, or may still propose, the whole credit of this foundation-fact is M. Orfila's; and had he done no more than establish this, and had he loaded this with the weight of many heavier errors, he would have greatly added to his reputation, and would have deserved treatment the very opposite of that which he has received from many of his professional brethren.

The most obvious error that M. Orfila committed was in thinking that he had detected arsenic in the healthy bones, and other tissues; an error which, if we remember rightly, he shares with M. Couerbe, and which has the further palliation, in a practical view, of lying on the right side, since, while it engendered greater caution in drawing conclusions, it introduced no real cause of doubt, the supposed natural arsenic being easily distinguishable from that which was

introduced. As a question of credit for priority of correction too, no one has the advantage of M. Orfila, for as far back as last November he had deposited a sealed letter with the President of the Academy, announcing his own suspicion of the correctness of his former conclusions; and before the publication of the other investigations he had been engaged in investigations on the point with members of the Institute.

These are the main features of a discussion which has fired the whole medical profession in Paris, and has given rise to more gross personalities, and more bullying, than was probably ever before introduced into any publicly entertained question of science. But it may, and doubtless will, lead to good results. The whole question will be well sifted, and probably many of the chief difficulties that have hitherto lain in the way of these investigations will be removed: already much has been done, and, under the excitement even of bad tempers, much more may still be accomplished. It would have been well if all this could have been effected without so great a contempt of the calmness that should mark the conduct of men pursuing science; if the love of truth for its own sake could have been made to shine more than the love of slander; and if the cause of public justice had not been made to wait on that of private malice. But how rarely do we find, in questions respecting the most frivolous discoveries, that men regard their own little scientific reputations as of less importance than their characters for decency and love of truth! There are some men in our own country who may plainly see their own features in the recent history of the French Academy of Medicine.

SUBSTANCE OF A CLINICAL LECTURE
ON
SEROUS OR AQUEOUS ENCYSTED
TUMORS,

Given at St. George's Hospital, June 1st.

BY MR. CÆSAR HAWKINS.

1. *Serous Cyst in the Abdomen.*
2. *Serous Cyst in the Neck.*

THERE are two cases in the hospital at the present time, gentlemen, neither of which is very common, and as it so happens, as is often the case with rare diseases, that they have occurred together, and are of the same character, I will bring them under your notice in to-day's lecture, believing them to be well worthy of your attention.

I. The first is the case of a young woman, Harriet Herbert, 25 years of age, in Drummond Ward, admitted May 19th, with an abdominal tumor of about seven years' duration, for which she has been tapped twice, and each time the fluid was perfectly transparent, and clear, and colourless. The first time was about four years ago, when I drew off twelve pints of watery fluid; the second was about two years and a half ago, when three pints of fluid were drawn off by a medical man, with whom she has lived as servant, and who was kind enough to send me some of it to examine. The swelling now occupies more of the right side than of the left, and is of moderate size; it sometimes causes difficulty of breathing, but otherwise she suffers no inconvenience, except from its weight. General health good. Bowels rather costive. Catamenia regular. Pulse quiet and regular.

May 27th.—I tapped her, and drew off about eight pints of perfectly transparent liquid, which our notes say was rendered very slightly turbid on the addition of nitric acid, or the application of heat; in fact, however, it required to be held against the light to make it evident that it was at all altered, and the quantity of albumen was so small, as to leave the liquid still quite transparent without the least deposit; it was only a little white, and on the former occasions there was no evidence whatever of albumen.

She was a little feverish the next day, being very nervous and hysterical, and had slight tenderness of the abdomen, or rather of the cyst, but is now almost well enough to go home again.

When this patient was admitted into the hospital, my informant remarked that a woman had my card over her bed, but that she laboured under *ascites*, meaning that she should have been placed in the medical wards; I knew, however, from former experience,

that medical assistance would be of no avail, and had ordered her in merely for the relief afforded by the operation of tapping, her case being one of *encysted dropsy*. How, then, are you to distinguish between ascites, and such a case as this, of fluid deposited in a cyst unconnected with the general cavity of the peritoneum? Very often you are assisted by the negative evidence afforded by the absence of all those symptoms which indicate disease of the heart, or liver, or peritoneum, or whatever the part may be that causes the ascites. In this young woman, for example, there has never been at any time the least derangement of the general health, and you see her now perfectly free from any illness whatever, and she only complained to a certain degree of the weight of the gallon of fluid which has been taken away. Sometimes, indeed, when the cyst is very large, and the pressure very great, the intestines, or stomach, or lungs, may be so pressed upon, as greatly to affect the health, and cause swelling of the legs, or ascites, or sickness, or constipation, or dyspnœa, in consequence of which the case may end fatally; but then you can trace the gradual effect of the tumor, instead of the symptoms which precede the deposition of fluid in ascites. Sometimes, again, you have the positive evidence of the particular part in which the swelling was first perceived; on one side or the other the patient will tell you it was first noticed by her before the whole abdomen swelled, and when small it may even sometimes happen, as with the ovary, that the cyst may be actually moved by the hand, or by the patient's change of position, so that its figure may be determined by examination. Our patient was scarcely sensible of this on the first occasion when I tapped her, though our notes say that the swelling seemed at present a little more on the right side than on the left. Except when the cyst is very large indeed, you can almost always detect the nature of the case by the sounds on percussion, the fingers reaching the alimentary canal in many places in ascites, while in encysted dropsy the part is perfectly dull, because the alimentary tube is behind it. So that when lying on her back you can, in such a case as Herbert's, feel no air in front, except in the higher part where the stomach and colon are situated, but you can detect it at the sides, behind the tumor. In ascites, on the contrary, you can feel it in front, but none at the sides, because the fluid gravitates there. Make the patient with ascites turn on one side, and you will probably feel the bowel again on the uppermost side, from the fluid sinking into the most dependent position; while, in encysted dropsy of any considerable size, the position of the cyst is not altered by change of position, and the sound

remains the same, however she may lie. It must be confessed, however, that there are cases of either kind, especially when the quantity of fluid is very large, which may be mistaken for each other, till an operation shews the nature of the fluid; and, as you may have heard, encysted tumors may be believed to exist, and operations attempted, when there was in reality no tumor whatever in the abdomen; besides which there will be difficulty of diagnosis in some cases, in which there is some effusion in the peritoneum in addition to that in the cyst, and caused by the irritation of it. I have several times drawn off the fluid from both situations at once.

The disease, then, was not ascites, but a form of encysted dropsy. But where was the fluid situated? In forty-nine cases out of fifty, when the fluid is in some cyst in the abdomen, you would believe that it is an ovarian tumor, and this case bore every resemblance to one of this kind; but when you tap an ovarian tumor, the fluid is almost always a thick kind of mucous substance, of every sort of colour and appearance, which I have known to be so full of albuminous substance, that it has become solid enough, immediately after it has been extracted, for a spoon to stand in it, as in a solid jelly, and it is at least almost always very tenacious and thick. In this case, however, you have seen that the liquid was perfectly transparent and watery, and so it was at the two previous times of operation. Even now, on the third operation, there is only a very faint trace of albumen. Most probably, therefore, the case is one of those serous or aqueous encysted tumors, which may occur in any part of the body, and you may see on the table several specimens of them in various situations; but you may see them most frequently in the liver, where they are seen in the preparations in every degree of development. When she was in the hospital, four years ago, under Dr. Chambers's care, I removed, as I have mentioned, a gallon and a half of the same liquid, and from its appearance we then believed the cyst to be in the liver; and from its being so long after the operation before the liquid began to accumulate again (for after the last tapping she says it was a year and a half before she perceived any return), and from its still possessing the same qualities, I am inclined to believe at present, as I was then, that it occupies this situation.

I will refer you to a paper which I drew up, and which was published in the 18th volume of the *Medico-Chirurgical Transactions*, for a full account of these serous or aqueous cysts, and of their effects, more especially when situated in the liver. You will find there also the distinctions between such tumors—which are often erroneously

called hydatids—and the real hydatid tumors of the liver, such as are seen in these other preparations; the cyst which contains the hydatids being indeed like the cyst of the aqueous tumor, but the two diseases being otherwise quite different. They are generally called serous cysts from their appearance; but the nature of the fluid secreted by them is very different from that of any natural serous membrane, all of which, except the arachnoid, secrete a liquid containing a considerable quantity of albumen, which is easily precipitated by heat or nitric acid. These new cysts, on the contrary, contain, according to Dr. Marcet's analysis, nothing but a very small quantity of animal matter, which he calls muco-extractive, with a few grains in one thousand of saline matter. Mr. Spitta has been kind enough to analyse the fluid in this case, and finds it to correspond with this description; and whenever you find such watery fluid in the body, you may conclude almost certainly that it comes from an artificial cyst, and not from any natural cavity, in a dilated state; and on this account I prefer the term aqueous cyst to the more common term serous tumor.

I do not wish you to understand, however, that the reverse holds good, and that when you let out *serum* (that is, transparent liquid, holding a good deal of albumen in solution) the fluid is always *necessarily* secreted by a natural cavity. On the contrary, many of the newly formed cysts, at a subsequent period of their growth, and in some cases even when the cyst is of small size, undergo some change in their state, by which the secretion is altered, and the cyst also changed. Sometimes, for example, the cyst is lined by a thick layer of lymph, which at first sight appears to be a large hydatid, but which I rather believe to be a thick mass of adhesive lymph, or albumen, like the masses often seen in the pleura, which exactly fills up, and is in contact with, every inequality of the interior of the cyst. Here is such an appearance, which was found unexpectedly in two cysts in the liver in a patient of mine, who died of some other complaint; you may see the cysts of considerable size, with their lining; and there was a similar one in one lung of the same patient. I remember a patient being brought into the hospital, who had fallen from a waggon containing some empty baskets, one of which had fallen upon him, and had ruptured a very large cyst of this kind in the liver, containing a layer of this lymph, with several pints of liquid.

At another time there will, in cases of newly-formed aqueous cysts, be evidence of inflammatory action afforded by portions of coagulable lymph floating loosely in the liquid, which will itself also shew that it contains a good deal more in a state of solu-

tion; or the fluid contents will be darker coloured, and mixed with coloured blood, so that it will coagulate from this cause, without distinct evidence of the secretion of lymph by inflammation.

In a third case, inflammatory action will cause suppuration in the cysts, as in an interesting case, from which this preparation was taken, of aqueous encysted tumor of the kidney, the history of which I published in the same volume I have before referred you to. There was great obscurity as to the nature of the tumor in this case till I punctured it, and let out eighteen ounces of nearly pure water; when, for the reason I have already given, it was clear that it was an aqueous cyst, probably from its situation connected with the kidney. It filled again, however, and became of immense size, and the irritation it excited carried off the little patient; and, on examination, out of five pints of fluid which it contained, a quarter perhaps was formed by a secretion of white purulent liquid. The kidney, as you may perceive, was quite healthy, with a small one separate from the natural kidney.

In other cases, again, the fluid secreted by the cyst is formed of a thick tenacious mucus; this may be found in other situations, but is especially common in the ovarian cysts, where immense quantities of it are met with. It would appear probable, from the observation of Dr. Babington, that this is the result of an inflammatory change, by which pus would be secreted were the rest of the fluid acid, but the pus is converted into mucus by the alkaline nature of the watery part of the liquid. You can thus, in diseases of the urinary organs, observe tenacious mucus and pus alternate with each other, and will require no other test of the acidity or alkalescence of the urine.

Lastly, you will find an extraordinary extent of sloughing and fungous appearance when aqueous cysts are opened, as in this cast and preparation of what I believe to have been an aqueous encysted tumor of the liver, for which change I must also refer to my paper. I may observe, indeed, that Dr. Malcolmson has sent me a paper of his, intended to show that the cases I had described of this singular process were abscesses of the liver; but although his paper showed me, what I had not myself observed, that abscesses of this organ may undergo a similar sloughing process, there were many circumstances which convinced me, as the preparation perhaps will show you, that those I had published were not of this description, but were instances, in all probability, of abscesses in these encysted tumors.

I said that the cysts also undergo changes, and in general the thinner the cyst itself the more simple is the nature of the fluid. The sac becomes by degrees (in some cases only,

however,) thicker and more dense, till it is converted into a tough fibrous substance of considerable thickness; so that some persons have called them, when in this state, fibrous tumors, in contradistinction to their earlier condition of serous cysts; and in cysts whose structure is thus fibrous I believe the secretion is always albuminous to a greater or less extent, and the colour also is generally darker than in those of thinner texture. And then, finally, when according to the usual laws of transformation of morbid tissues, a thin and transparent serous cyst has been converted into a tough fibrous substance, the fibrous cyst may itself undergo another conversion into bone; so that you may see some of these serous cysts of the liver, and elsewhere, more or less osseous; hardly any part of the sac, in a few instances, escaping the ossific process.

There is yet one further alteration to which the aqueous encysted tumors, and indeed almost any cyst, are occasionally liable, especially in some situations, such as the ovary. It is the development in their interior, in the manner described by Dr. Hodgkin, of secondary cysts within the coats of the original one, or of masses of half gelatinous substance in numerous cysts, or in a solid shape, such as you may see in these preparations. These new bodies are often called malignant; but I believe that they are often quite free from malignant properties, though closely resembling the cysts, and the changes of these cysts, which take place in really malignant diseases. This is a subject, however, which we will not at present enter into.

You will find, then, that there are cases of aqueous encysted tumors in internal situations, which are often fatal in their effects, especially as they are inaccessible, and beyond the reach of medical or surgical remedies. Our patient, Herbert, supposing her to have one of them in the liver, has derived no advantage whatever from medicine, and living in the service of a medical man, she has tried a great variety; nor have local remedies any material power over such cysts when deeply situated, as in her case, where it is within the abdominal muscles, even if not in the liver. Relief or cure is therefore to be expected only from some operation, if they are accessible to this at all; but I must refer to the 18th volume of the *Medico-Chirurgical Transactions* for a full account of the treatment necessary for these tumors when situated in the liver.

In Herbert you have seen that the cyst has been three times emptied by the trocar, and the former operations have not been succeeded by obliteration of the sac by pressure, which is probably not unfrequently the case, when in the liver, as in some cases which I have seen. Neither was there any suppura-

tion or sloughing away of the cyst, which took place in the girl from whom this cyst escaped: she was tapped by Sir Benjamin Brodie, and some aqueous fluid evacuated from a cyst apparently in the liver, after which a good deal of fever and irritation ensued, succeeded by a discharge of pus from the bowels, and then this cyst came away, which looks exactly like one of these tumors, which may have passed into the colon, just as the contents of hydatid encysted tumors of the liver so often escape when the same route has been established by adhesion and ulceration. The operation you have witnessed will of course afford the patient temporary relief, but probably the sac will fill again as it has already done, though much more slowly than is usually the case in similar tumors in the ovary. You have seen, also, that the danger of the operation is inconsiderable; in fact, what little irritation she experienced was rather hysterical than inflammatory.

II. This case, then, shows you what may be the course of a serous or aqueous encysted tumor in an internal situation; our other case shows you the disease in an external part. I have not *proved* to you, indeed, that it is a case of the kind, but I believe it to be, and I will take it for granted that it is so, in order to make some remarks on the nature of such a disease, and of its treatment when it takes place in the neck, which is a not uncommon situation for its development. It is the case of John Morgan, seventy-two years of age, or, as I was recently informed, seventy-eight, who was admitted into Harris's ward May 25th, with a large tumor occupying the right side of the neck; it is soft, has an even surface, and fluctuates distinctly; the trachea and oesophagus are pushed by it quite to the left side of the neck, describing a considerable curve; the vessels are pushed to the outside, at least the carotid artery can be felt pulsating along its outer border; the sterno-mastoid muscle is also pushed to the outside above, and covers it in part below; and the omo-hyoideus can be perceived, when he swallows, crossing it obliquely, and it is covered on its inside by the sternal muscles, the sterno-hyoid and thyroid, so that it is only at its upper part that it comes very near the skin. The tumor moves freely with the larynx; there is no pain in it, nor any impediment to respiration or deglutition, notwithstanding the great curve described by the trachea and oesophagus. There is no apparent enlargement of the thyroid gland, and the tumor is large enough probably to contain six or eight ounces of liquid.

The tumor, then, is an *aqueous encysted tumor* in the neck, or, as it has been called by Maunoir and O'Beirne, who have given

some very good descriptions of the disease when situated in this part, *hydrocele of the neck*. Considering its size, and the derangement of parts produced by it, it excites surprisingly little disturbance in this man; but I have known the tumor occasion by its pressure serious disturbance in respiration and deglutition, with bleeding from the nose and mouth, and affections of the brain, from impediment to the circulation above, and much action of the heart, and threatened suffocation, by its effects on the larynx and lungs. These varieties of effects depend not only on the size of the tumor, but much also on its situation, and the manner in which it is bound down or left unrestrained by the muscles and fasciæ, and other adjacent parts. Sometimes it is situated entirely on the inside of the mastoid muscle, on one or both sides of the neck, or in front of the windpipe, when its pressure will be great on the important parts there situated; sometimes it is partly to the outside of the sterno-mastoid muscle, or entirely to its outside, above the clavicle, when its effects are of course much less serious; sometimes it is only at the upper part of the neck, near the jaw, and sometimes it is found in all these situations at once.

The largest tumor of this kind which I had ever seen was one that I attended about a year ago with Mr. Langley, which had been growing for twelve years without relief, as the patient had been recommended by several surgeons of eminence to have nothing done to it, under the belief that it was a solid tumor. It probably had been much harder and more solid at first, but when I saw it there was no difficulty in recognising the nature of the disease; and by means of a small needle I immediately evacuated full a pint of reddish serous fluid. At this time she was weak and emaciated, and nearly dying from its effects; she had for many weeks been unable to lie down, and even when asleep in the sitting posture, was constantly awakened with the dread of impending suffocation; and occasional fits of difficulty of breathing urgently threatened her life. The tumor filled the whole space from the jaw to the clavicles, and projected so much forward that it was some years since she had been able to depress her chin, so as to see any part of her person; it was very irregular in figure, globular portions projecting on all sides of the mastoid muscles, and one square prominence reached over the right clavicle upon the chest; and, from the anterior part of the tumor covering it completely, no part of the windpipe could be felt. She was nearly cured by the means presently to be mentioned, but she was carried off about six months afterwards by an affection of the chest.

On examination I found the sac nearly

obliterated in every part, and the thyroid gland quite healthy, except one little portion of the right lobe, the size of a nut, which was hard and chalky.

This tumor had always been believed to be a bronchocele or enlargement of the thyroid gland, and the cyst in these tumors is often supposed to be formed by the growth of some one or more of the cells of that gland; but in almost every case the tumor has, as in that I have just narrated, nothing whatever to do with that body, and if situated near the thyroid gland, on the inside of the sterno-mastoid muscle, the cyst is precisely of the same nature as if it were on the outside of the muscle, with all the vessels between it and the thyroid gland.

Before operating on these tumors I recommend you always to puncture them with a needle to ascertain their contents, and this small needle, like a cataract needle, but somewhat larger, or a grooved needle like this, will generally empty the cyst, or nearly so, if you wish to do it, before proceeding to any other measures. You see, by the case I have mentioned, that the cyst may easily be mistaken for a solid tumor, especially if it is covered and bound down, as most part of the tumor of our present patient is concealed by thick muscles; and on the other hand a projecting portion of the solid thyroid gland may easily be mistaken for a cyst. A friend of mine once called me to see a lady on whom he had begun an operation on what he thought was a small cyst, but he found so much arterial blood coming from it that he left her sister pressing on it while he came for me; we found it, on a little further exposure, to be a small piece of the spongy isthmus of the gland, and after tying a ligature round it, there was no further trouble.

In our patient, the part, even where nearest to the skin, is covered by some dense fascia, or else the cyst is of some thickness; but I have sometimes known the sac and skin so transparent as to allow the light of a candle to be seen through it, as in hydrocele of the testis. This was the case in a patient not very long ago in the hospital under Mr. Babington's care; but I do not recollect to have seen it thus transparent, except when it was on the outside of the mastoid muscle, between it and the trapezius above the clavicle.

Except where the sac is transparent, the needle is not only sometimes necessary to prevent the errors of diagnosis I have alluded to, but because, where you have no doubt of the existence of fluid, you ought to know the nature of that fluid before you determine what treatment to adopt. There are, in the first place, cysts of *arterial blood* formed in the neck in connexion with the thyroid gland. Sir B. Brodie was relating

to me a day or two ago a case in which, on opening a cyst in this situation, he found it could not be emptied, but continued to pour out scarlet blood, which was easily stopped by pressure, and the fluid subsequently was absorbed. The nature of the disease in such cases, and the necessity of the caution I am pressing on you, are shewn in a case which occurred to Mr. Dalrymple, in which that gentleman, after opening one of them, lost his patient a few days afterwards by repeated hæmorrhages. The tumor was composed of several cysts, and the vessels of the isthmus of the thyroid gland opened into these cysts, and gave rise to the fatal hæmorrhage.

In the second place, there are cysts of *venous blood* occasionally met with in the neck, which, like the preceding, do not admit of the same free measures that may be used in aqueous cysts. Some of these have been described by Mr. Hey, of Leeds; and I remember a case of this disease in an out-patient of this hospital many years ago, in whom the tumor, which was of the size of an orange, was punctured, and found to contain dark venous blood; it was closed, but the man not being willing to remain went home, and died shortly afterwards, I think in a few hours, from hæmorrhage. It is probable that these may be connected with the thyroid gland, like the cysts of arterial blood; but I do not know from any dissection whether this is the case, or whether they may be, as Hey suggests, a kind of aneurism communicating with the jugular vein. From the early age of some cases, and their curability, I do not think the latter conjecture very probable.

Having said thus much of the diagnosis of these tumors, we will now consider the means of treatment applicable to them, which must depend on many points—the nature of the fluid, the thickness of the cyst, the circumstances of the patient, and so on.

1. You may puncture the cyst, not only to ascertain the kind of fluid it contains, and the thickness of the cyst, in order to guide you in your future proceedings, but as a palliative means of treatment. When the patient is afraid, or circumstances do not at the present time allow of a cure being attempted, the fluid may be evacuated from time to time by a small hydrocele trocar, or even by a needle, if the skin and cyst are thin, so as to allow of a ready escape. I have thus let out six or eight ounces repeatedly without seeing any bad consequences.

2. If the fluid is watery, and the covering thin, the fluid will sometimes be absorbed, and the cyst obliterated, by a stimulant application: Pott's lotion of camphorated spirit and goulard, or a solution of muriate of ammonia, or a strong solution of iodine and iodide of potassium, or an ammoniacum plaster. I have known this succeed after

puncture, when perhaps it would not otherwise have answered; but I believe the plan will not effect a cure when the cyst is thick, or much covered, nor when the fluid is thick.

3. After the fluid has been let out by the trocar, a stimulating injection, such as one containing iodine or sulphate of zinc, has been sometimes known to induce adhesive inflammation, so as to effect a cure; but it generally fails in the encysted hydrocele of the spermatic cord, which is the same disease; and I have not myself tried it in the aqueous cyst of the neck. I know no objection to a trial of this plan, if the cyst is tolerably thin, and does not extend in several divisions among the muscles, since it does not, if it fails, prevent the subsequent employment of other means, which will generally succeed.

4. These means are such as induce suppurative inflammation, and excite a continued irritation in the interior of the cyst. One method of this kind is the insertion of a slip of lint into the sac through an incision in its most prominent part, and keeping it there till the cavity appears nearly obliterated by contraction. Sometimes this will succeed very easily, as in a patient of Mr. Babington's some time ago, whom some of you may remember; but it is not always free from danger, as in the patient from whom the cyst in this preparation was removed by operation, the opening previously made having bled so as to be dangerous. The excision, however, was unfortunately followed by fatal sloughing of the cellular tissue. And if the cyst is large, or divided into compartments, when a portion, for instance, is on each side of the mastoid muscle, the simple insertion of lint will generally, I believe, be insufficient, and the continued irritation of a seton is a better method of treatment. The mode in which it may be used is to evacuate the fluid at one end of the sac by a small hydrocele trocar, through the canula of which you introduce a long probe, so as to ascertain the position of the vessels and other important parts, and determine in what direction a second opening may most advantageously be made, which is generally in the longest diameter of the sac. Some persons then cut down upon the probe, and carry the silk through the opening; but a far easier method is to employ a long fine trocar, which will pass through the canula easily, and the pointed end of which is larger than the rest of the instrument, so that two or three threads of silk in the other extremity (where there is an eye) will easily be carried through the opening made in the skin by the point. The trocar I shew you will easily make the opening you require, and with it, in the case I described to you before, I made a second opening, about seven inches from the first, from one side of the neck to the

other across the trachea. Where the sac is of a complicated figure, a second seton is sometimes necessary, which may be inserted at a subsequent period through one of the openings first made, so as to pass across a portion of the membrane, which, by going under muscular fibre, may have a small communication with the rest of the sac, and thus be beyond the influence of the first seton.

The introduction of the seton will occasion in some persons a good deal of constitutional disturbance; and you must especially be on your guard against the formation and confinement of foul matter, and the irritative fever, which it excites, which you can easily obviate by a little enlargement of the most dependent of the openings, and the injection of a little tepid water to wash out the cyst, from time to time, when the secretion is unhealthy. Sometimes again, if the seton is not speedily followed by the contraction of the sac into a kind of sinus along the track of the silk, you can assist its operation by injecting a little stimulant of zinc, or iodine, or caustic solution.

The time you should continue the seton varies according to its effects, but six or seven weeks will be generally enough. Mr. Bransby Cooper has published a case in which the irritation was so great that he was obliged to remove the silk in a few days; but I have not seen this, nor do I think you need expect it in any case, if you attend to the foul purulent secretion, which requires removal in the way I have described. If, on the other hand, the irritation is too little, or the sinus is extensive under the mastoid or other muscles, you had better let the silk remain in for several months, till the cyst has evidently quite contracted.

5. In one instance, where the cyst was rather thick, and the fluid sanguineous, I touched the interior of the cyst, which reached from the trachea to the acromion, under the mastoid muscle, with nitric acid, to destroy it, and on the fourth day it seemed to have nearly succeeded, when the man was unfortunately attacked with erysipelas. I do not recommend this method, however, unless every other fails, on account of the dangers of this kind, which may arise from its use, besides the connection of the sac with the vessels, which I could feel with my finger in that case, and which, in the dissections I have made, may almost be said to form the back of the cavity; the sac being extremely thin behind, and in close contact with the vessels and nerves.

Now in our present patient's case I had intended to let out the fluid and employ a seton, believing him to have been much younger than he proves to be; but in a man of seventy-eight years of age, who really suffers scarcely any inconvenience from the

tumor, I do not think I should be justified in doing what would probably occasion a good deal of irritation, and at his age cannot be considered free from actual danger. It will be very easy to do something effectual for him, if the tumor is found to increase, or gives him any trouble, and in the meantime he may use a stimulant lotion of sal ammoniac*.

6. There is only one more remark I will make to you, which is to caution you against attempting to remove an aqueous cyst in the neck by operation, as being both unnecessary and hazardous. Mr. Bransby Cooper in one case began an operation, believing the tumor to be a solid one, but very properly desisted from it, and cured the disease by suppuration, when it was found to be a cyst of this nature. If you desire to know the curious manner in which the vessels and nerves are connected with the cyst, so as to be concerned in an operation, you may read the description given by Dr. Warren of some cases of this kind, and that equally whether the tumor be at the lower part of the neck, or high up on the inside of the mastoid muscle below the jaw; almost every nerve and vessel of these parts requiring to be dissected, notwithstanding the tempting facility apparent before the operations were begun.

ON THE DETECTION OF ARSENIC.

THE following important report was read at the Academie des Sciences, on the 14th of June, by M. Regnault:—

The Academy has charged M. M. Thenard, Dumas, Boussingault, and myself, to give in a report on several memoirs and communications addressed to it concerning the employment of Marsh's apparatus in medico-legal researches. (Here follow the names of five memoirs.)

Before pointing out the results contained in these writings, and before mentioning the experiments which we have made to verify them, it appears to us indispensable to state, as briefly as possible, the state of the question at the time when they were addressed to the Academy. (Here the reporter passes to the origin of Marsh's apparatus, &c. and then enters on the description of the numerous experiments undertaken by the commission in order that it might decide on the important questions referred to it.) These experiments permit the following propositions to be established:—

1. Marsh's plan easily renders sensible $\frac{1}{1000000}$ of arsenious acid in a liquid; spots begin to appear with liquid containing only $\frac{1}{2000000}$.

* The patient in fact would not even allow it to be punctured with a needle.

2. The spots are not shewn better when a large quantity of liquid is employed in the apparatus than when a smaller quantity is used, provided the same proportional quantity of arsenious acid is present; but they form during a longer period in the first than in the second case. It thence results that there is an advantage in concentrating arsenical solutions, and in operating on a small volume of the liquid; for much more intense spots are thus obtained.

3. It is of the greatest importance, when it is desired to produce spots by means of Marsh's apparatus, to place in the way of the gas a tube three decimetres long filled with amianth or with cotton, in order to catch the little drops of solution which are always carried forward mechanically by the gas; otherwise one is exposed to the danger of obtaining oxi-sulphate of zinc, which often presents the appearances of arsenical spots.

4. The process proposed by M. Lassaigne may produce good results. It consists in making the arseniuretted hydrogen gas pass through an exactly neutral solution of nitrate of silver, then decomposing the liquid by chlor-hydric acid, evaporating it to drive off the acids, and then testing the residue with the arsenic-reagents. It is especially convenient for transferring into a small quantity of the liquid a very small portion of arsenic existing in a large volume of liquid which cannot be concentrated by evaporation, and consequently for permitting one to obtain much more marked arsenical spots by treating the now concentrated arsenical liquid in a very small Marsh's apparatus. Only care must be taken not to decide on the presence of arsenic, because the solution of nitrate of silver is rendered turbid, and lets fall a deposit during the passage of the gas, since the deposit may be produced by gases not arsenical when mixed with hydrogen, and even by hydrogen alone if the operation is carried on under the influence of light. The solution of nitrate of silver may be replaced by one of chlorine, or of an alkaline chloride.

5. The plan pointed out by MM. Berzelius and Liebig, and variously modified by Kapelin and Kaupmann de Colmar, demonstrates quantities of arsenic which are not shown at all, or only in a doubtful manner, by the spots. This plan therefore presents the advantage of condensing the arsenic in a much more complete manner; only it will frequently happen that the arsenic will be found mixed with sulphuret of arsenic, which may alter its colour, especially if the arsenical substance exists in small quantity.

It is to this last plan that your commissioners give the preference for separating arsenic. They think that the apparatus

should be disposed in the following manner:—A straight-necked and wide-mouthed flask is closed by a cork bored with two holes. Through the first of these holes a straight tube, one centimetre in diameter, is passed down to the bottom of the flask, and in the other is placed a narrow tube bent at a right angle. The end of this tube is placed in another larger, about three decimetres long, filled with amianth. A tube of hardly fusible glass, two or three millimetres in diameter, is adapted to the other end of the amianth tube, and is drawn out at its entrance, and enveloped for a short distance with gold or silver leaf. The flask must be able to contain all the liquid to be tested, and to have still an empty space equal to about one-fifth of its total capacity. It must be remembered, however, that it is important that the volume of the liquid should not be too considerable, if the liquid to be examined be one that contains only traces of arsenical matter. The disengagement tube is terminated *en biseau* at the end which is passed into the flask, and carries a little hollow sphere at some part of its vertical branch. This disposition is not indispensable, but it is convenient, because it condenses almost all the water that is carried up by the gas, and makes it fall back into the flask.

The apparatus being thus disposed, some laminæ of zinc are introduced into the flask, then a layer of water to close the aperture of the safety-tube, and then a little sulphuric acid. The hydrogen which is disengaged drives the air out of the flask. The tube is now heated with a coal fire at the part which is enveloped with gold-leaf; a small screen being interposed to prevent the parts too far off from being heated. When the tube is red hot, the suspected liquid is introduced by the open tube through a fine-tubed funnel, so as to make it descend along the walls, and prevent it from carrying air into the flask. If the disengagement of gas is lessened after the introduction of the liquid, a small quantity of sulphuric acid must be introduced, and the operation must be made to go on as slowly and as regularly as possible.

If the gas contains arsenic, it is deposited in the form of a ring in the part of the tube which is anterior to the heated part. The gas which is disengaged from the tube may be set fire to, in order to try and collect spots on a little cover of porcelain; or the tube may be bent, and have its extremity passed into a solution of nitrate of silver, to condense, if necessary, the last portion of arsenic.

The arsenic being deposited in the tube in the form of a ring, it is very easy to determine all the physical and chemical properties which characterize that substance. Thus, one may determine easily, first, its volatility;

secondly, its change into a white powder, arsenious acid, when the tube is heated during the passage of a current of air through it; thirdly, by heating a little nitric acid or aqua regia in the tube, the arsenious is made to pass into arsenic acid, which is very soluble in water. The liquid, cautiously evaporated to dryness in a little porcelain capsule, will give a brick red precipitate when some drops of a neutral solution of nitrate of silver are poured into the capsule; and lastly, when all these trials have been made, the arsenic may be again produced in its metallic state. For this purpose it is sufficient to add a small quantity of black flux in the little capsule in which the precipitation by nitrate of silver has been effected, to dry the mixture, and introduce it into a little tube closed at one end and drawn out at a lamp. By submitting the part of the tube which contains the mixture to a full red heat, the arsenic passes to the metallic state, and forms, in the very narrow part of the tube, a ring which presents all the physical characters of arsenic, even when only very small quantities of that substance exist.

6. It is easy to find, in commerce, zinc and sulphuric acid which do not give any signs of containing arsenic with a Marsh's apparatus, even when considerable quantities of zinc are dissolved. The sulphuric acid which we employed was purified by distillation, and the zinc was laminated zinc in fine slips.

In all cases it is indispensable to test previously, with the greatest care, all the substances to be employed in researches. We even think that some preliminary attempts do not give a sufficient guarantee of security; and that it is necessary for the experimenter to make, at the same time as, or directly after, his examination of the poisoned substances, an exactly similar blank experiment, employing all the same reagents in the same quantities as in the real operation.

7. The processes of carbonization of the animal matter by nitric acid or nitrate of potass may succeed completely; but still it sometimes happens that one cannot prevent a very vivid deflagration at the end of the experiment; and this may give rise to a notable loss of arsenic. The carbonization by concentrated sulphuric acid, and the treatment of the resulting material with nitric acid or aqua regia, appears to us, in a great number of cases, preferable. This plan, which is suggested by MM. Danger and Flandin, requires the employment of a much smaller quantity of the reagent; it is always easy to conduct, and when properly executed is accompanied by the loss of a very small quantity of arsenic; and even this loss may be avoided by carrying on the carbonization in a glass retort connected with a recipient.

8. It is of the greatest importance that the carbonization of the organic matter should be complete; without this one obtains not only a liquid which grows mouldy in the Marsh's apparatus, but this liquid may produce spots which sometimes present much resemblance to arsenical spots. These spots, which were first observed by M. Orfila, are often produced in great abundance when the organic matter has been only partially destroyed. They proceed from the carbonaceous gas, partly decomposed in the flame, but are very easily distinguished by chemical reagents from the true arsenical spots. The latter dissolve instantaneously and in the cold in a few drops of nitric acid; the solution, evaporated to drive off the excess of nitric acid, and then treated by neutral nitrate of silver, gives a brick red deposit of arseniate of silver: while the non-arsenical spots dissolve with more difficulty in nitric acid, and there always remain some particles of brown matter which do not disappear till the acid is heated; and when all is dissolved, the solution, evaporated to dryness, and treated by nitrate of silver, gives a yellow deposit of phosphate of silver. Thus nothing is more easy than to distinguish these from the true arsenical spots. If the latter are themselves mixed with foreign matters, as happens when the carbonization of poisoned food has been imperfect, then these characters become less striking; but these spots may give rise to very serious mistakes if the experimenter contents himself with physical characters which resemble one another.

9. As to the arsenic which has been announced to exist in the healthy human body, all the experiments which we have made, both on the muscles and on the bones (and on soup) *have given us negative results.*

10. The commission, therefore, thinks that Marsh's method, applied with all the precautions that have been pointed out, is sufficient for medico-legal researches, in which the quantities of arsenic which it is required to detect are almost always much greater than those of which the sensibility of this apparatus enables us to determine the existence. Understanding clearly, however, that it must always be employed as a *means of concentrating* the metal for the purpose of studying its chemical characters, and that one must consider as *null, or at least as very doubtful*, the indications which it will furnish, if the deposit which is formed in the anterior part of the tube does not permit the examiner, on account of its slight thickness, to verify, in a precise manner, the chemical characters of arsenic.

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns, Aug. 10, 1841.)

	PRICE.						DUTY and 5 per cent.	DUTY PAID.	
	£	s.	d.	£	s.	d.		In 1841, to last week.	Same time in 1840.
Aloes, Barbadoes, D.P. c	15	0	0	to 30	0	0	{ B.P. lb 0 2 } F. lb 0 8 }	92,985	63,172
Hepatic (dry) BD..... c	5	0	0	10	0	0			
Cape, BD. c	2	10	0	3	10	0	F. lb 1 4	—	521
Anise, Oil of, German, D.P. lb	0	5	0	0	5	6	E. I. 1 4	698	574
E. I. lb	1	10	0	3	10	0	c 6 0	18	54
Asafoetida, B.D. c	0	1	0	0	1	3	lb. 0 1	989	2,973
Balsam, Canada, D.P. lb	0	1	6	—	—	—	c 4 0	303	456
Copaiba, BD. lb	0	4	6	—	—	—	lb 1 0	613	309
Peru, BD. lb	25	0	0	50	0	0	c 4 0	63	26
Benzoin (best) BD..... c	13	0	0	—	—	—	c 1 0	423	189
Camphor, unrefined, BD..... c	6	3	3	—	—	—	lb 1 0	10,025	10,497
Cantharides, D.P. lb	0	8	6	0	8	9	lb 4 0	858	951
Caraway, Oil of, D.P. lb	3	10	0	—	—	—	lb 0 1	22,405	12,336
Cascarilla or Eleutheria Bark, D.P. c.	0	10	6	—	—	—	lb 1 4	2,082	1,684
Cassia, Oil of, BD..... lb	0	0	2½	0	0	6	c 1 3	4,434	4,485
Castor Oil, East India, BD..... lb	0	17	0	0	18	0	{ lb 0 6	341	425
West I. (bottle) D.P. 1½ lb	0	18	0	1	0	0			
Castoreum, American lb	0	18	6	—	—	—	{ c 1 0	28,262	36,280
D.P. Hudson's Bay lb	1	2	0	—	—	—			
Catechu, BD. Pale c	0	2	0	0	3	6	{ lb 0 1	50,485	25,055
Dark c	0	2	0	0	4	0			
Cinchona Bark, Pale (Crown).... lb	0	2	4	0	3	0	{ lb 0 2	5,282	5,626
BD. Red lb	0	1	6	0	2	9			
Yellow lb	0	1	0	—	—	—	lb 0 2	7,991	7,491
Colocynth, Turkey lb	0	12	0	1	15	0	lb 0 6	20,649	24,377
D.P. Mogadore lb	3	10	0	3	15	0	c 4 0	33	32
Calumba Root, BD. c	10	0	0	19	0	0	c 4 0	338	262
Cubebs, BD. c	1	10	0	—	—	—	c 6 0	52	1
Gamboge, BD. c	0	0	6	0	1	0	{ c 6 0	5,078	4,730
Gentian, D.P. c	0	0	0	13	0	0			
Guaiacum, D.P. lb	12	0	0	7	10	0	{ c 6 0	5,123	3,508
Gum Arabic, Turkey, fine, D.P. ... c	7	0	0	2	14	0			
Do. seconds, D.P. ... c	2	6	0	2	5	0	c 6 0	10,175	14,205
Barbary, brown, BD. c	5	10	0	—	—	—	c 6 0	47	148
Do. white, D.P. c	1	15	0	2	5	0	lb 0 1	1,995	1,925
E. I. fine yellow, BD. c	3	5	0	—	—	—	lb 1 0	7,144	4,750
Do. dark brown, B.D. c	8	0	0	12	0	0	lb 0 6	36,428	27,213
— Senegal garblings, D.P. c	0	0	2½	0	0	3	{ lb 0 3	7,969	9,219
— Tragacanth, D.P. c	0	1	6	0	2	6			
Iceland Moss (Lichen), D.P. lb	0	2	3	0	2	6	oz 6 0	615	1,257
Ipecacuanha Root, B.D. lb	1	0	0	3	10	0	{ c 6 0	67	115
Jalap, BD. lb	5	0	0	14	0	0			
Manna, flaky, BD. lb	2	0	0	11	10	0	lb 2 6	554	272
Sicilian, BD. lb	0	8	0	0	9	0	lb 1 0	18,635	27,291
Musk, China, BD. oz	0	10	0	—	—	—	lb 4 0	1,488	3,655
Myrrh, East India, BD. c	0	8	9	0	9	0	lb 0 1	188,005	210,915
Turkey, BD. c	0	3	11	—	—	—	lb 1 0	16,798	8,178
Nux Vomica, BD. lb	0	5	0	0	7	0	{ F. lb 1 0	12,856	15,968
Opium, Turkey, BD. lb	0	8	0	0	9	0			
Peppermint, Oil of, F. BD. lb	0	7	6	0	8	6	lb 1 0	1,100	1,672
Quicksilver, BD. lb	2	10	0	—	—	—	lb 0 6	77,649	77,690
Rhubarb, East India, BD. lb	0	1	0	0	1	9	{ lb 2 6	4,213	7,495
Dutch, trimmed, D.P. lb	0	2	0	—	—	—			
Russian, BD. lb	0	18	0	1	0	0	E. I. lb 0 6	59,335	54,636
Saffron, French, BD. lb	0	0	4	0	0	5	{ Other sorts 0 6	48,832	43,005
Spanish lb	0	1	6	0	1	8			
Sarsaparilla, Honduras, BD. lb	0	1	0	0	1	3			
Lisbon, BD. lb	0	1	0	0	1	3			
Scammony, Smyrna, D.P. lb	0	1	0	0	1	3			
Aleppo lb	0	1	0	0	1	3			
Senna, East India, BD. lb	0	1	0	0	1	3			
Alexandria, D.P. lb	0	1	0	0	1	3			
Smyrna, D.P. lb	0	1	0	0	1	3			
Tripoli, D.P. lb	0	1	0	0	1	3			

‡§ B D. In Bond. — c. Cwt. — B. P. British Possessions. — F. Foreign. — D. P. Duty paid.

ERECTION OF A NEW HOSPITAL
IN PARIS.

It is said that the Municipal Council has determined on the erection of a new hospital for chronic diseases in the northern part of Paris, near the church of St. Vincent de Paul. It is to contain 600 beds: 300 for men, and 300 for women, in separate buildings. — *L'Examineur Medical*, a new weekly Sunday journal, edited by MM. Dechambre et Aug. Mercier.

APOTHECARIES' HALL.

THE Court of Examiners of the Society of Apothecaries, on Wednesday the 4th inst. made their Annual Report of their proceedings to the Court of Assistants, from which it appeared that during the past year, 429 candidates had presented themselves for examination. That of that number 363 had received a certificate of their qualification to practise, and 13 had been thought deserving of the especial commendation of the Court for general proficiency in the subjects of examination.

LIST OF GENTLEMEN WHO HAVE RECEIVED
CERTIFICATES.

Thursday, August 12, 1841.

J. T. Jackson, Oxford. — J. Todd, Hartfield Sussex. — E. E. Tucker, Exeter. — J. Lord, Bury, Lancashire. — L. Pugsley, Wilvelescombe. — J. Hough, Richmond, Surrey. — W. P. Hodgson, Stockton, Warwickshire. — E. Roberts, Sydenham, Kent.

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Monday August 9, 1841.

T. W. Jones. — W. H. Kater. — C. H. Dobson. — H. A. Lee. — E. Chesshire. — R. S. Leggatt. — R. H. Williams. — J. C. Wells. — C. White.

Friday, August 13.

T. Coates. — J. Rogers. — G. J. Gates. — John Cockin. — F. W. R. Smith. — W. G. Gregory. — T. G. Dixon. — J. L. Paterson. — R. T. Whitehead. — G. E. Dunsterville. — E. H. Derriman. — G. R. Irons. — C. L. Leete. — H. E. Beck.

RECEIVED FOR REVIEW.

Mr. Brett's Practical Essay on some of the Principal Surgical Diseases of India. Calcutta, 1840.

On the Construction and Management of Hospitals for the Insane; with a particular Notice of the Institution at Siegburg. By Dr. Maximilian Jacobi. Translated by John Kitching. With Introductory Observations, &c. by Samuel Tuke.

A TABLE OF MORTALITY FOR THE
METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 7th Aug. 1841.

Small Pox	9
Measles	15
Scarlatina	20
Whooping Cough	37
Croup	4
Thrush	6
Diarrhoea	7
Dysentery	2
Cholera	1
Influenza	1
Typhus	23
Erysipelas	4
Syphilis	1
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	130
Diseases of the Lungs, and other Organs of Respiration	219
Diseases of the Heart and Blood-vessels	11
Diseases of the Stomach, Liver, and other Organs of Digestion	93
Diseases of the Kidneys, &c.	4
Childbed	2
Ovarian Dropsy	0
Diseases of Uterus, &c.	5
Rheumatism	4
Diseases of Joints, &c.	2
Ulcer	0
Fistula	0
Diseases of Skin, &c.	1
Diseases of Uncertain Seat	89
Old Age or Natural Decay	47
Deaths by Violence, Privation, or Intemperance	19
Causes not specified	3
Deaths from all Causes	759

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

August.	THERMOMETER.		BAROMETER.	
Wednesday 11	from 54 to 64		29.45 to 29.60	
Thursday . 12	46	62	29.80	29.88
Friday . . . 13	42	65	29.84	29.76
Saturday . 14	45	67	29.58	29.67
Sunday . . 15	42	66	29.67	29.76
Monday . . 16	52	70	29.78	29.90
Tuesday . 17	56	72	29.90	29.98

Winds, W. and S W.

On the 11th, morning overcast, with rain; otherwise clear. The 12th, clear. The 13th, cloudy; rain in the morning and afternoon. The 14th, morning overcast, with heavy showers; otherwise clear. The 15th, generally clear; a few drops of rain in the afternoon. The 16th, clear, except the evening. The 17th, generally cloudy.

Rain fallen, .98 of an inch.

NOTICES.

We thank Mr. Blake for his paper on the Mesmeric imposture; but we think that the proceedings of M. Lafontaine require no further exposure; they have already ceased to attract the public attention.

J. B. B.—The papers are not of a kind of which we have any present need. They are left at our office.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

FRIDAY, AUGUST 27, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XLVII.

Diseases of the Thorax. General observations. Dyspnæa. Cough. Methods of exploring the physical conditions of the chest, by the senses of sight, touch, and hearing.

FROM the throat—and especially from that part of it with which we were last occupied—the transition is natural and immediate to the *thorax*. Now the interior of the chest is the theatre of numerous and most important morbid changes. In that cavity are lodged two out of the three organs most essential to life. The heart, and lungs, and brain, have been said, by a bold figure of speech, to constitute the tripod of life : and the two former are planted in the thorax. In the same division of the body lie also the great blood-vessels, and many other parts of scarcely less consequence. With respect to the heart, its alternate swinging movement cannot long be suspended, and the patient continue to live : and three minutes' total interruption to the play of the lungs would in most cases be irremediably fatal. And lesser impediments to the free working of either of these two vital organs are productive of great distress, and lead often to consecutive changes of a very serious nature in various other portions of the body.

These parts, of which the function is so indispensable, and of which the disorders are so grave and perilous, are encased in such a manner by the ribs and other boundaries of the chest, that they can neither be seen nor

handled. And until a very late period in the history of medicine—until our own times, in fact—the diagnosis of the formidable maladies that befall the viscera of the thorax was exceedingly uncertain and imperfect. Physicians were able indeed, by the observance of indirect symptoms that declared themselves through the system at large—by the presence of inflammatory fever, I mean—to infer that inflammation had been somewhere lighted up : and symptoms that denoted disturbed function of the respiratory apparatus—cough, difficult breathing, or local pain—might suffice to apprize them that the inflammation was situated in the chest. But what tissue it affected, where was its exact place, what was its extent, or what were its physical effects—these were points concerning which they had no means of obtaining any precise knowledge. “Under the title of pneumonia or pneumonic inflammation,” says Cullen, “I mean to comprehend the whole of the inflammations affecting either the viscera of the thorax, or the membrane lining the interior surface of that cavity : for neither do our diagnostics serve to ascertain exactly the seat of the disease, nor does the difference in the seat of the disease exhibit any considerable variation in the state of the symptoms, nor lead to any difference in the method of cure.” You will see, as we proceed, how very inaccurate this last statement would be, if it were made under our present mode of investigating these diseases. “Pneumonic inflammation, (he continues) however various in its seat, seems to me to be always known and distinguished by the following symptoms:—Pyrexia, difficult breathing, cough, and pain in some part of the thorax.”

I state these things to you—who do not recollect the time, as I do, when no medical man in this country could, with truth and candour, say more of his knowledge of diseases of the chest than Cullen said—that you may the better estimate the exceeding

value of the discovery of what is called the method of auscultation, in the detection and discrimination of disease; and most particularly of thoracic disease. In the present day we are able to obtain more accurate information respecting the disorders of the parts contained in the chest, than of any other internal, and therefore invisible parts of the body. Indeed, in a vast number of instances, we can tell, as positively as if we saw them, the actual condition of the thoracic viscera: can follow, step by step, the successive processes of disease or of repair, in which they are involved. We can penetrate beyond the symptoms which denote deranged function, and detect and understand those much less fallible symptoms which arise from alterations of structure. And this vast addition to our pathological knowledge has been given us by the simple application of one of our five senses to the investigation of disease, the sense of hearing, which for so many centuries had been (strangely as it now seems) neglected, or but little used. By the assistance of the ear it has come to pass that those diseases which, besides being the most common and the most destructive, were also the most obscure, are now better understood than the diseases of any other internal part whatever.

The direct symptoms which arise out of the changed conditions of the parts affected in thoracic complaints, are so mixed up with all that we know or can learn of such complaints, that what in all other cases is called the morbid anatomy of a disease, becomes, here, a part of its history. I shall not say, therefore,—as in regard to many other maladies I am obliged or I find it convenient to say—so and so are the symptoms; and afterwards, so and so are the morbid appearances: but I shall describe the morbid conditions in the outset, as the only way of rendering the symptoms which flow out of them intelligible.

But before I enter upon the subject of auscultation, it may be useful to make a few remarks upon those symptoms of thoracic disease which were previously known, and which arise out of, or rather which express, derangement of the pulmonary functions.

One of the most constant, and obvious, and distressing, and instructive of these symptoms, is embarrassed or laborious breathing: what is technically called *dyspnoea*. You know that in a healthy adult, under ordinary circumstances, the act of respiration is performed, unconsciously almost, about eighteen times in a minute. There is about one act of respiration for every four beats of the heart. In various diseases this proportion is materially altered. The measured succession of reciprocal movements, by means of which air is drawn into and again let out of the lungs, are performed

with hurry, or effort, or unusual slowness. *Dyspnoea* implies some deviation from the natural manner or rate of alternately expanding the thorax, and suffering it to collapse again; of inspiring and expiring: in one word, of breathing. The patient himself may, or may not, be conscious of this deviation. In most of the cases in which *dyspnoea* claims to be regarded as a symptom, he is conscious of it. Now upon what does this symptom depend? It may ultimately be referred to an altered proportion between the quantity of atmospheric air that reaches the lungs, and the quantity of blood that is sent into them, to be converted from venous to arterial, from the right side of the heart. That, doubtless, is at the bottom of almost every case of *dyspnoea*. Let me remind you that respiration is an automatic movement; subject nevertheless to the occasional control of the will. The pulmonary branches of the par vagum constitute the principal and constant *excitor*, as the nerves that supply the muscles of respiration are the *motor* links of the nervous chain by which the automatic movements are governed. It is believed that the presence of venous blood in the capillary vessels of the lungs forms the natural stimulus to the pulmonary part of the par vagum. In the ordinary breathing of a healthy person, this stimulus or impression is not felt: perhaps because being slight and habitual, and exactly apportioned to the need of the individual, it is not attended to: or it is at once appeased by the admission of air, and the corresponding change in the blood. But when the change is not immediately or perfectly accomplished, then arises the distressful sensation which every body has felt, but which our own language has no one word to express. The French call it the *besoin de respirer*. The English phrase, *want of breath*, denotes the peculiar sensation equally well. It calls into exercise, frequently, the voluntary power of performing the mechanical acts of breathing—a power which is superadded to the automatic process.

Various are the ways in which the natural manner and frequency of the respiratory movement may be deranged. They were fully considered when I was on the subject of death by *apnoea*. They all operate, ultimately, by destroying the just equilibrium between the blood and air which meet to undergo chemical changes in the lungs. We have lately been considering certain diseases in which the difficulty and distress of breathing is often extreme. In croup, and laryngitis, the only inlet for the air is narrowed at its very entrance: there is more blood passing through the lungs than can be arterialized under the ordinary motions of respiration: instinctive efforts take place to increase the quantity of air; to make up by more numerous acts of inspiration for the

diminished amount of air introduced by each single act. For a time these compensatory efforts may suffice. But if the access of air be still impeded, the blood begins to circulate in the arteries, but half decarbonized; and to linger and stagnate in the lungs: the lips become livid, and the skin dusky. Make, however, a free opening in the pipe that should conduct air to the lungs, and the balance between the blood in the lungs and the air that reaches them being restored, the dyspnoea is soon at an end. The quantity of blood being the *same* then, but the air inspired *too little*, there will be dyspnoea. And the very same thing occurs whenever a portion of lung from being spongy is rapidly rendered solid. No air can then penetrate it; nor *perhaps* any blood: but the same quantity of blood as before arrives at the right side of the heart, and is transmitted thence through the pulmonary artery: and consequently those portions of the lungs, which are pervious to blood and air, are supplied with blood in excess, and require air in excess: *i. e.* dyspnoea is necessitated. And you will perceive that similar consequences may arise from any pressure made upon the lung obliterating in a certain degree its cellular structure; as by fluid collected in the pleura; by enlargement of the heart; by aneurism of the great vessels; by tumors, of whatever kind, within the chest; or by pressure upwards against the diaphragm by reason of a distended abdomen, whether the distension be occasioned by disease, such as ascites, or by obesity, or by a full stomach, or by a gravid uterus. A like disproportion will ensue, if the free expansion of the thoracic cavity be prevented by pain, by disease or rigidity of its boundaries, or by palsy of its muscles through interruption of the nervous circle whereon their contractions depend.

But on the other hand the balance may be destroyed from the opposite quarter: the air admitted during a single ordinary inspiration being the same, the quantity of blood requiring to be converted from purple to scarlet may be augmented; and in that case also, in order to maintain the due equilibrium, more numerous acts of respiration must be performed: in other words, dyspnoea will arise. This is the case under strong exercise: the pressure of the muscles upon the veins propel their contents with greater velocity towards the right side of the heart; the heart contracts more frequently in proportion; it is more rapidly filled with blood; a greater quantity than usual is sent through the pulmonary artery to the lungs; and the individual breathes more quickly, to supply this augmented quantity of blood with air: he is out of breath, in a state of dyspnoea. But this is not disease. Disease, however, will often have the same effect. The quickened

circulation in fevers, any obstacle to the free passage of the blood from the heart into the arteries, will tend to gorge the lungs with blood, to destroy the requisite equilibrium between the air and the blood in those organs, and so give rise to dyspnoea.

Other conditions still may be mentioned, as predisposing to hurry of the breathing—a peculiar state of the nervous system; certain qualities of the blood:—but I need not dwell on these at present.

There are two important corollaries derivable from what I have now been stating. In the first place you must perceive how intimately the functions of the heart and lungs are dependent upon each other; and that disease originating in either of these vital organs may readily be the cause of consecutive disease in the other. We shall have many examples of this before us as we proceed. It would afford materials for an interesting essay, this mutual interdependency of cardiac and pulmonary disease. At present I merely glance at it in passing.

In the second place, what I have said of dyspnoea must have sufficed to show you that, taken by itself, it has not much value as a diagnostic symptom. All that it tells us is, that the healthy and natural relation between the quantity of blood and of air in the lungs is disturbed: but to determine the cause of that disturbance—to decide whether the heart be in fault, or the lungs, or both, or neither—we must have recourse to other sources of information.

Cough is another of the symptoms, mentioned by Cullen, as denoting disordered function of the breathing apparatus. I need scarcely tell you that it is produced by closing the glottis, and then making a sudden and strong expiration. Its purpose is the dislodgement of mucus which may have collected in excess in the air passages, or any other source of irritation to the membrane lining those parts. To be effectual it requires the admission of a certain quantity of air, and the possession of a certain degree of muscular strength. I pointed out to you, in the last lecture, a remarkable exemplification of this: the boy whose case is described by Mr. Chevalier in the *Medico-Chirurgical Transactions*, was dying of croup; was on the brink of being suffocated by the collection of mucus in his windpipe and bronchi, which mucus he was unable to expel: and he was unable, not from deficient muscular strength, but because he could not inflate his lungs *beyond* the collected mucus, in a sufficient degree. When an ample opening was made in his trachea, he drew in a strong breath, and coughed the mucus up *through the rima glottidis*. In old and feeble persons labouring under chronic bronchitis with profuse secre-

tion from the mucous surface, strength is often wanting to cough the phlegm up; and they die suffocated.

But the sensation which prompts to the act of coughing may arise from many other causes besides the accumulation of mucus in the air passages. Any slight irritation about the glottis; a long and trailing and tickling uvula; the inspiration of irritating vapours; pressure of any kind upon the respiratory organs; may any of them produce cough. Nay, it sometimes is provoked by sympathy with other parts; an instance of which we have in what is called a stomach cough. Some morbid condition, some irritation of the stomach exists, which being appeased, the cough ceases. You will recollect the name and the functions of the *pneumo-gastric* nerves. We have, in the fact just mentioned, another example, in addition to those which I glanced at in a former lecture, of irritation of the sentient extremities of one branch of a nerve, declaring itself by uneasy sensations excited in other branches of the same nerve. For these reasons, therefore, cough is not more diagnostic of particular diseases situated in the thorax, than is dyspnoea. There are, indeed, certain varieties of cough, as there are certain modifications of the breathing, from which we may obtain very useful information even in respect to the nature and seat of *some* diseases: and these varieties and modifications I will point out as I go along.

Let me admonish you, also, before we come to auscultation, not to fall into an error which has been too common; that of trusting entirely to the ear in the investigation of thoracic disease, to the neglect or exclusion of those phænomena which are discoverable by the eye, or the hand; or of those indirect revelations which are furnished by the condition of other parts and functions, or by the previous history of the patient. Even before the discoveries of Avenbrugger and Laennec, physicians were too remiss (if we may judge from their writings) in what may be called the mechanical exploration and observance of the actions of respiration. A good deal may be learned, sometimes, by merely placing one's hand upon the chest, or belly, as I shall explain more particularly by and by: and a great deal, also, may be made out, in some cases, by the simple inspection of those parts, when they are uncovered. You may see, for example, that the ribs, in respiration, scarcely move at all, while the belly rises and falls alternately with the descent and ascent of the diaphragm. This is called *abdominal respiration*. It may arise from a painful condition of the intercostal muscles, or of the pleuræ, rendering the patient *unwilling* to elevate his ribs; or it may arise from disease

of the spinal cord, between the origins of the phrenic nerve and of the intercostal nerves, rendering the patient *unable* to raise them; or the same *inability* may result from disease of the lungs themselves. The symptom may guide us at once to the seat of the malady. Again, the breathing may be entirely *thoracic*, no motion of the abdomen taking place; and this may depend upon an affection of the diaphragm, or of the pleura which is reflected over it; or upon disease, accompanied with tenderness, within the abdomen—upon peritonitis for example; or upon mere distension of the abdomen. Or by looking at the naked chest, you may see that one side of it moves, and that the other moves less, or does not move at all: and the motionless side may be of the natural size as compared with the other, or it may be pinched up and contracted, or it may be round and bulging; and most important conclusions, and most important indications of treatment, will flow from a knowledge of these circumstances. The general form of the chest is also instructive. Never neglect, then, to examine the thorax, in cases where it is supposable that the disease is seated in that part of the body, by your *eye*, as well as by your *ear*. The eye needs but little training to enable it to perceive and comprehend those signs which are within its sphere: the ear, unfortunately, requires to be carefully educated. I will just remark, farther, that in the case of females no indelicate exposure of the person need be made. In most cases the morbid conditions I have been adverting to may be recognized through a thin linen covering.

Auscultation signifies the investigation of internal diseases through the sense of hearing: and it is especially applicable, for reasons which I either have stated or will state, to the study of *thoracic* diseases. In its full meaning it includes all that we learn by listening to a cough, and all that we gather by striking the chest, and listening to the resulting sound. But in general, the term *percussion* is used to express this last mode of eliciting information, although the information is conveyed through the medium of the ear; and the word auscultation is applied to the art of distinguishing diseases by listening to internal sounds, by means of the ear placed in apposition with the surface of the body, or by means of some conductor of sound interposed between the ear of the listener and the person of the patient. In the first of these two modes the auscultation is said to be *immediate*; in the second, *mediate*. By percussion we ascertain the degree of resonance, or want of resonance, of the part struck: by auscultation we learn the qualities and modifications of the voice, as reflected through the chest; and of the

breathing; and of the sounds of the heart. The invention of the method of percussion we owe to a German, Avenbrugger, who wrote an excellent treatise upon it, which was brought into notice by Corvisart, who translated it. For the more brilliant discovery of auscultation we are indebted, as every body knows, to Laennec.

Now it will save us much trouble, and conduce, I hope, to your future progress as practical auscultators, if, before I speak of any of the diseases of the chest, I premise some general observations respecting these modern methods of examining the human body, with the view of detecting and discriminating its diseases. Indeed, I could not make myself intelligible unless I did so.

And first, with respect to percussion, which you will please to recollect is nothing else than auscultation of, or listening to, sounds which we ourselves artificially and purposely produce.

You know, every child knows, by daily experience, that different substances, when struck, give out very different modifications of sound. If you strike a drum, you get one kind of sound; if you strike a brick wall, you get another. The one is loud, trembling as it were, and prolonged; the other dull, short, and flat. But I need not attempt to describe in words things which are familiar to you already. Bodies that are solid, or inelastic, give the dull flat sound in proportion to their solid thickness, or their want of elasticity. On the other hand, hollow vessels, *i. e.* vessels containing air, with thin, firm, elastic boundaries, give out a sound more or less approaching in its qualities to that of a drum: the sound is called a *hollow* sound from that circumstance. If you have a wooden cask containing air only, it is resonant when struck; fill it half full of water, and the lower part will render a flat sound, the upper empty portion a hollow sound; less hollow, however, than when the vessel contained no water: fill it up with water, and the *whole* is dull on percussion: pour out the water, and fill it loosely with wool—it will still be resonant, though in a different and less clear note than when it held air alone.

Now this experiment may be transferred to the human chest, which is a cavity, bounded by firm, thin, tense, and elastic walls, and containing, in its natural state, the spongy lungs, which are full of air, and other parts that are solid; of which the heart is the chief. If you strike the surface of the chest (it requires a little knack to do it properly), and if the blow falls over a portion of healthy lung, you will produce a resonant or hollow sound. If the lung be not there, if it be pushed aside, and its place supplied by some more solid or inelastic substance,

by fluid for example, you will hear a dead sound. So you will if the lung *be* there, but has lost its spongy character, is void of air, and somehow or other solidified. But you may have a resonant sound, though the lung is in a state of disease; nay though the lung is not there: so that percussion alone cannot always be depended upon. I shall tell you, hereafter, how to guard against being misled by it in such cases. Again, if you strike over the region of the heart, you will get a positively dull sound, or at any rate a much duller sound than in most other parts of the chest.

It is really a singular thing, that this method of searching for indications of disease, and of health, should have been so long neglected or overlooked in our profession. I am sure that I had a practical acquaintance with the principle of percussion long before I knew any thing of physic; and so, I make no doubt, have most of you. Many a time, when wishing to know whereabouts I might drive a nail firmly into a wall, I have tried with the hammer to find which was brickwork, and which was wooden joist; and percussion is an art in daily use for similar purposes, with carpenters and bricklayers. Yet it does not appear to have been thought of by physicians till the middle of the last century, when Avenbrugger, after studying its results for seven years, as he tells us *inter tædia et labores*, published at Vienna his "*Inventum novum, ex percussione thoracis humani, ut signo, abstrusos interni pectoris morbos detegendi.*" This was almost totally neglected however, until, as I stated before, Corvisart's celebrated work on diseases of the heart brought it into general notice.

Avenbrugger and Corvisart, and indeed every body who used percussion at all, until a very few years ago, employed *direct* percussion: that is, they struck the chest with the extremities of their fingers. More recently, *mediate* percussion has been introduced into practice, by M. Piorry. In mediate percussion some solid substance is placed upon the spot, the resonance of which is about to be explored, and the blow is made upon that substance, which is called a *pleximeter*—a blow-measurer. A round thin plate of ivory, laid flat upon the surface, is the most common sort of pleximeter; or metal, or wood, or leather, or India rubber, may be employed. Many persons, and I am one of them, use no other pleximeter than the fingers of the left hand.

I shall explain, as briefly as I can, the method of employing percussion, and the cautions requisite to render it an effectual and a true interpreter of the state of the parts beneath the stricken surface.

The position of the patient is of some

consequence. It should be one that is convenient to the examiner, and not inconvenient to himself; and it should be one calculated to render the part struck as firm and tense as possible. The best position of all is a sitting position, on a firm chair. But you may percuss a patient very effectually as he sits up in bed, or while he stands, or some parts even when he is lying down. A good deal is said,—more, in my opinion, than is necessary,—about the effect of curtains, and so forth, in deadening the sound. I do not believe they will ever interfere with your conclusions, especially as we learn more from comparing the sounds given out upon percussing the corresponding parts of the opposite sides of the chest, than from the absolute resonance or want of resonance of any single part. But there are some exceptions to this; and if your patient can be made to sit on a chair in the middle of the room, so much the better.

Then, if you are about to percuss the front of his chest, make him hook his arms over the corners of the back of the chair, and throw his head back. If you desire to explore in this way the lateral portion of the thorax, he must place the hand of that side upon his head, and lean a little to the opposite side. If you would know how the posterior part of the chest sounds, he must lean forwards, fold his arms across his breast, and bend down his head.

Next as to the mode of percussing. For direct percussion, the ends of the fingers of the right hand should be brought together, and into a line with each other, so that no one of them projects beyond the rest; and care should be taken, first, to compare the sound produced by striking any part of the chest on one side, with that produced by striking the corresponding part on the other side. It follows from this rule that we should not examine all the points on one side before passing to the other, because we should thus lose the remembrance of, and the power of accurately comparing, the sounds obtained from corresponding points. It is best to strike first on one side of the body, and then on the corresponding spot of the other. It follows also that we are not to compare the result of percussion on one of the *ribs*, with the result of percussion on one of the *intercostal spaces*. The blow should fall *upon* the rib, and *parallel* to it.

A second point requiring attention, is the state of the chest in respect to the act of breathing. If one side be percussed after the movement of inspiration, and the other after that of expiration, some little difference in the resulting sounds will be manifest, even in the healthy condition of the thorax. And this might mislead. Let corresponding

spots on the two sides be therefore both struck, either while the chest is expanded, or while it is collapsed, or while the patient holds his breath.

Thirdly, you must take care to strike the corresponding parts at the same angle, and not with the fingers perpendicular to the surface on one side, and inclined obliquely to it on the other: also to strike corresponding parts with the same degree of force. And the blow should not be hard enough to give the patient pain; indeed such a blow would not produce a good sound. It should be smart and quick; the ends of the fingers should not *remain* on the chest. Under some circumstances, however, the patient cannot bear to be percussed at all.

These latter cautions are, most necessary when *direct* percussion is employed; over which *mediate* percussion has, however, many advantages. Some of these are obvious. In the first place, the space examined by mediate percussion is very exactly defined and limited. Secondly, you may strike the pleximeter much more forcibly than you could strike the unprotected body, and so produce a more decided sound. Even when the surface is morbidly sensible, or the patient unusually irritable, so that percussion in the ordinary way cannot be performed at all, it may generally be done through the pleximeter. A third and very great advantage is, that mediate percussion is available when made over certain parts where, even although there may be no pain occasioned by it, ordinary or *immediate* percussion is attended with no useful result. Parts, I mean, where there is much fat, and parts which are fleshy, or oedematous. If the pleximeter be pressed firmly upon these parts, even upon the mamma in females, the hollow sound is attainable; whereas, if they were struck by the fingers, the sound would be perfectly dull. Mediate percussion may be applied also, with effect, through the clothes.

I say that a very convenient way, and one which I find quite sufficient, of employing mediate percussion, is by making a pleximeter of the finger, or fingers, of the left hand; taking care that they are closely in contact with the subjacent parts, and *similarly* applied to corresponding spots; and that the backs of the fingers be outwards. Piorry declares, indeed, that the resonance produced by this mode is scarcely one-tenth part so great as that elicited by using a thin, solid, and elastic plate. For all practical purposes, however, I am certain that the finger, as it is the readiest, so also is it a very satisfactory and sufficient pleximeter. It has, moreover, this positive advantage, that the sound made by striking

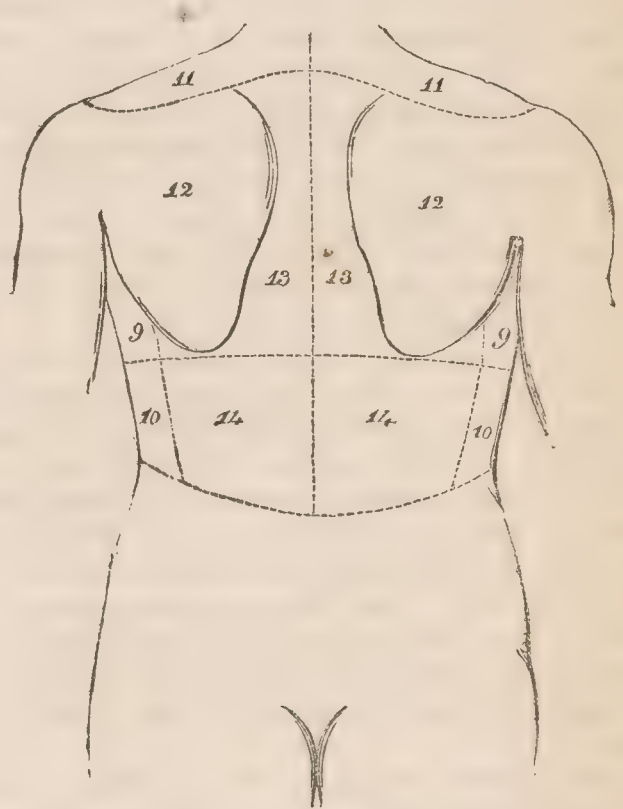
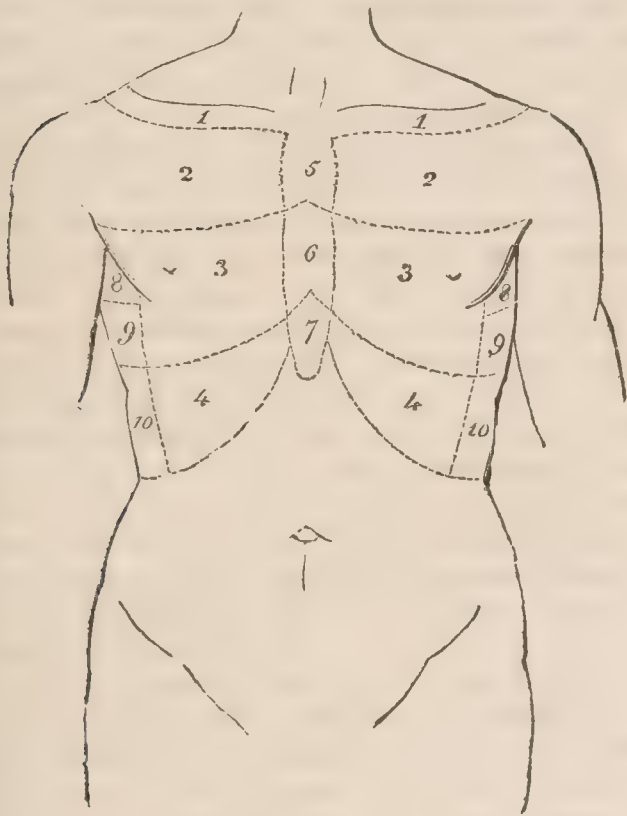
it is not loud, and does not obscure or interfere with that which depends upon the condition of the chest.

It is right that you should be aware of differences of sound which belong to the individual. *Cæteris paribus*, the sound given out on percussion is more resonant during inspiration than during expiration; in childhood and youth than in middle age; in middle age than in old age; in females than in males; in thin persons than in fat; and, *they say*, in nervous irritable people than in those of a contrary temperament.

And it is still more necessary that you should be aware of differences of sound given

out, in health, by different parts of the surface of the thorax, *in the same individual*. And in order to explain this more distinctly, and for the sake of reference hereafter, let me here describe to you the *regions* of the thorax, as they have been artificially mapped out for the purposes of auscultation.

It is unnecessary for me to describe particularly the method followed in this arbitrary division of the thorax into regions. I will state the names that have been commonly applied to them, and the sounds which in health they respectively yield, according to their number in the diagram before you.



The *first* region is that of the *clavicles*; one of course on each side. Upon these bones it is immaterial whether direct or mediate percussion be made. The sound given out should be very clear at their sternal extremities, dull at their humeral extremities, and clear at their middles. The resonance diminishes from the sternal towards the acromial end of the clavicle. These parts correspond to the summits of the lungs.

The *second* region is the *subclavian*. It lies between the clavicle and the fourth rib on each side. Beneath this superficial region lies the upper lobe of the lungs; and towards the sternum large bronchi are situated. You will understand, therefore, that the sound educed by striking this part ought to be very clear.

A little lower down, number *three*, is the *mammary* region, extending from the fourth to the seventh rib on each side, and answering to the middle lobe of the lungs. Here

also the resonance afforded by percussion is clear; but in the lower part of this region, on the left side, we find the heart, which is more or less covered by lung; and on the right side the liver begins to mount. The sound is somewhat modified and deadened by these deeper seated viscera. In women we can determine the degree of resonance of this region through the *mammæ*, by *mediate* percussion only.

The *fourth* is the *infra mammary* region. It comprehends that part of the bony compages of the thorax on each side which lies between the seventh rib and the edge of the cartilages of the false ribs. Into this region, on either side, may descend the thin anterior margins of the lower lobes of the lungs; but in the same region, on the right side, lies the liver, over which the sound of percussion is dull; and on the left side is placed the stomach. Hence, when the stomach is tympanitic, a preternaturally resonant sound will proceed from this part; and when the

stomach contains no gas, the sound will be irregularly dull.

All the regions hitherto described are double. There is still the mesial part of the front of the chest to be subdivided, and we may call the three regions there situated, and numbered 5, 6, and 7, the *upper sternal* region, the *middle sternal*, and the *lower sternal*. In all these the sound on percussion ought to be clear, except perhaps in the inferior portion of the last, which may be rather dull, or which, from its vicinity to the stomach, may be tympanitic.

The *eighth* region is the *axillary*; the axilla above the fourth rib on each side. The *ninth* is the *lateral* region, between the fourth and seventh ribs. In both these regions the resonance should be distinct and clear. The *tenth*, which may be called the *lower lateral* region, below the seventh rib at the sides, gives the same sounds as the *infra mammary*, namely, on the right side a dull sound, on the left a sound which at times is preternaturally hollow: on account of the presence of the liver in the former case, and of the stomach in the latter.

But we have yet to look at the hind part of the thorax. Here we have the space (region *eleven*) which forms the top of the shoulder, and lies between the clavicle in front, and the superior spine of the scapula behind; the *acromial* region. Very little can be made out by percussion in this spot; the sound is dull. But the depression immediately above the scapula, the *supra-spinal fossa*, as it is called, is a space which affords valuable information to the *ear* applied over it.

The *twelfth* region is the *scapular*. It comprises the part covered by the scapula on either side. It corresponds to the posterior part of the middle lobe of the lungs, but yields a dead sound, unless a pleximeter be used.

The *thirteenth* region is the *intra scapular*. It lies between the inner edge of the scapula and the spine on each side. It corresponds to the roots of the lungs, and to the mesial parts of the lower lobes. You may get a good clear sound here, if the patient's arms be crossed, and his head bent forwards, and his back arched, so as to stretch and tighten, and make as thin as possible, the superincumbent muscles; you will find also that the ridge of the spinal column in this part gives a hollow sound.

The last region to be mentioned, number *fourteen*, the *dorsal*, is the lowermost part of the ribs behind. It answers to the base of the lungs, and gives a clear sound: but the liver on the right side is apt to render its inferior portion dull; and the stomach on the left to make it unduly resonant.

Now, knowing these circumstances, if you find that a dull sound is yielded on percussing a part that should render a hollow sound, you conclude that beneath the part struck there is not the natural quantity of air. But whether this arises from consolidation of the lung itself, and the obliteration of its cells, or from fluid effused into the cavity of the pleura, you cannot, by mere percussion, determine. So, again, if the sound be unnaturally tympanitic, you cannot say whether that results from what is called pneumothorax, or from emphysema of the lung. Percussion testifies that *air* is there; but further this deponent sayeth not.

The information which the exercise of percussion may leave thus imperfect, auscultation of the sounds produced by the acts of breathing, speaking, or coughing, will in most cases supply.

If the ear be laid close to the surface of a healthy thorax (or if the instrument called the stethoscope be interposed between that surface and the ear), the ear will hear the air enter and fill the lungs, and then withdraw and leave them again, in perpetual succession. The sound produced by this ingress and egress of air has been called the *respiratory murmur*. I might tell you that this sound, in my judgment, is rather a rustle than a murmur: that it is like the sighing of the wind in the branches of a tree. I might say, with Laennec, that it resembles the sound made in the deep inspiration of a sleeping person; or, with Dr. Davies, that it reminds one of the soft murmur of a pair of bellows, of which the valve does not click. But one minute's appliance of your ear to the subclavian region in a child, or even in an adult, will give you a clearer notion of the nature and character of this sound than any verbal description could convey. Yet, respecting this natural respiratory murmur, there are some things of which it is desirable that you should be previously informed.

In the first place, the *entrance* of the air is much more noisy than the *exit*: which, sometimes can scarcely be heard. You will see, by and by, the importance of noticing this fact.

In the next place, the murmur of respiration is not equally audible in all persons. It may differ much in intensity, though not in kind, in two persons, each of whom is in perfect health. Thus it is much more loud and distinct in children than in grown persons. So remarkably is this the case, that when we meet with unusually noisy respiratory murmur in an adult, we say that his breathing is *puerile*; it has the character of the respiration of a child. Now, I tell you before hand, that puerile respiration

in the lung of an adult is generally a sign of disease; and it is mostly partial; heard in certain parts only of the chest.

Again, *cæteris paribus*, the natural murmur of respiration is more clearly audible in lean and spare persons than in such as are muscular and fat. Fat and muscle are bad conductors of sound, and act as dampers. Listening to the breathing through a thick layer of adipous tissue, is like listening (as Dr. Latham says) to the respiration of a person through his clothes.

But if you take two healthy men who are alike in respect to leanness or fatness, you may often find that the respiratory murmur is very loud in the one, and very feeble, or almost inaudible, in the other: but in this last it *becomes* audible when he takes a deep and forced inspiration.

The reason of this difference is not very well understood. The breathing may be very indistinct, though the thorax be capacious, and well formed, and healthy. Some persons seem to require less effort than others to maintain the due equilibrium between the air in the lungs, and the blood in the lungs: as if they had not only pulmonary space enough, but to spare. So that the difference in the manner of breathing, and in the sound occasioned by the passage of the air in and out, depends, in all probability, upon individual peculiarities of the circulation. At any rate, it is very important that you should be aware of the existence of these differences.

But the sounds which reach the ear applied to the chest of a breathing person will differ in different parts, and under different circumstances. The sound given out by the air as it passes through the trachea and larger bronchi, differs from that which results from its passage into and out of the smaller bronchi and air passages. I am anxious not to refine too much on these points; and, therefore, I pass by minuter points of difference. Place the stethoscope over the trachea, and you will hear just such a sound as you might expect to hear: the sound of air rushing through a tube of considerable size, a blowing noise. We will call that sound, which you will recognise again when you have once heard it, *bronchial* breathing. It accompanies the outward as well as the inward passage of the breath. Again, place your ear or your stethoscope upon the right mammary region; there you shall hear that rustling sound, which I propose to call *vesicular* breathing, and which is chiefly audible during inspiration. We shall find these distinctions of much use in the discrimination of disease. We shall find *bronchial* breathing, for example, where we should find *vesicular*.

Now if in any part of the chest where we

ought to hear breathing, we hear none, this may result from solidification of the subjacent portion of lung; or from some obstacle in a large bronchus, preventing the air from entering that part; or from air being in any way shut up and stagnant in that part; or from an effusion of liquid into the pleura at that part. And here again percussion comes into play, and determines for us which of these possible circumstances is the one really present. If the part when struck emits a hollow sound, there is stagnant air beneath it, either in the healthy lung, or in the lung altered by disease, or in the cavity of the pleura. If a flat solid sound be given out, there is solid lung beneath, or liquid in the pleura, between the ear and the lung. All these points, and the conclusion to be deduced from them, will become clear to you, I trust, as we proceed. Minuter analysis of the diagnostic signs would be premature.

A word or two preliminary I must say about the *voice*.

The voice passes outwardly through the mouth and nostrils into the surrounding air; it passes also inwardly, through the trachea and bronchi into the lungs, and it may be heard through the lungs, by the ear laid flat against the chest. But it gives quite a different sensation to the ear in different places. If you place the stethoscope on the trachea, the voice will articulate itself into your ear as if it came from and through the instrument. This sound, which is natural here, would be unnatural, and a mark of disease, if heard beneath the clavicles. When heard beyond its natural situation, it is called *pectoriloquy*. A less degree of this, a sound like that of a person talking into a tube, and whose words, for that reason, are muffled and indistinct, is called bronchial voice, or *bronchophony*. When to this modification of the voice there is added a twanging vibration, a cracked discordant tremor, resembling the squeak of Puncinello, or (as some think) the bleating of a goat, *agophony* is said to exist. All these unnatural modifications of the voice are indicative of most important changes in the thorax. I merely introduce them to you now: you will become better acquainted with them in due time.

What is true of the natural voice, is true, *mutatis mutandis*, of that unnatural vocal sound, *cough*. The cough may be so modified by the condition of the internal parts, as to reveal that condition.

There are yet other ways in which some information can be gathered respecting the interior of the chest. If you give certain patients a sudden smart jog while your ear is applied to their thorax, you may hear a splashing sound; like that produced by shaking a barrel or a bottle partly full of

water. From this expedient you in fact determine that the cavity of the pleura, or perhaps a large excavation in the lung, does contain both air and a liquid. This is called the method of *succussion*. It was employed long before the other forms of auscultation were thought of. Hippocrates mentions it.

Again (but that is not auscultation), you may sometimes collect useful information respecting the state of the chest by simply laying your open hand upon it. In most persons there is a distinct vibration or thrill produced by speaking, which thrill is very sensibly felt by the hand. Now this thrill may be felt on one side of the chest and not on the other. And such a difference is an unequivocal sign of disease.

The positions which I described before as being the most convenient for the performance of percussion, are the most convenient also for the purposes of auscultation. You may listen with the unassisted ear, or you may listen through the stethoscope. This, as you know, is a perforated piece of wood, of which one end is adapted to the ear, and the other, which is to be applied to the chest, is hollowed out, or expanded, into the shape of a bell, or funnel. The object of the instrument is to collect and convey to the ear of the observers the vibrating impulse of the air, or of the solid walls of the thorax, occasioned by the motions within. The stethoscope is sometimes useful for examining a circumscribed spot in the thoracic cavity. We *must* use it also when we would investigate the breathing in parts to which the ear itself cannot easily be applied; and in cases where, from the filth of the patient, or because he has some infectious disorder, we desire to avoid any immediate contact with his clothes or person; and in cases of young or old ladies, to whose breasts it might not be seemly or delicate to be putting our faces. But, with these exceptions, the stethoscope, in my experience, is rather a hindrance than a help. Much, however depends upon custom. I can best distinguish sounds within the thorax by my unassisted—perhaps I should say my unimpeded—ear: and I therefore employ the stethoscope or cylinder in such cases only as I have just adverted to. I cannot do without the instrument, but I do without it whenever I can. Care should be taken that the end of the stethoscope next to the ear be closely and comfortably fitted to that organ: and that its other extremity be blunt, so as not to hurt the patient; and, farther, that it be uniformly and evenly applied to the surface of his chest. If these precautions be attended to, the shape and fashion of the instrument are quite unimportant.

CONTRIBUTIONS

TO THE

CHEMICAL PATHOLOGY OF SOME FORMS OF MORBID DIGESTION.

BY GOLDING BIRD,

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[Continued from p. 726.]

IN continuing our investigations into the chemical characters of the fluids vomited in the diseases to which the terms pyrosis, water-brash, gastrorrhœa, stomach-gleet, follicular gastric dyspepsia, &c. have been applied, we meet with a very interesting subject of inquiry at every step we proceed; viz. do the fluids vomited in these affections, themselves frequently but symptomatic of other and more important diseases, always possess the same chemical characters? or do they present those distinctions which the accounts given by patients of their own sensations would lead us to suppose as really existent? We have already seen that such is not the case as a general rule; and it becomes important to trace the exceptions, if any exist, and connect them, if possible, with particular states of disease.

The distinction between pyrosis and gastrorrhœa, although very generally admitted, does not appear to be defined by any very distinct line of demarcation. The distinction practically made, and to which I shall adhere in the few remarks I have occasion to make on these subjects, appears to depend upon the comparative amount of irritation present in each case: thus, when the colourless vomited fluid is preceded by a sense of burning heat at the scrobiculus cordis, the disease is generally termed pyrosis; and gastrorrhœa, when the vomiting is scarcely preceded by more than a sense of uneasiness in the scrobiculus cordis, or more frequently by a feeling of tightness and constriction over the whole epigastric region, often compared to a cord drawn tightly round the abdomen.

In examining the chemical properties of the vomited fluids, it will, perhaps, be best, in the first instance, to take a case of pyrosis and one of gastrorrhœa, as simple and as much unconnected with any serious organic lesion as possible, and then proceed to those in

which the gastric discharges are accompanied by or depend upon evident structural disease of the stomach or other organ. As all these cases have been *en masse* referred to subacute inflammation of the stomach, by some authors, and by others, in my own humble opinion, upon much more philosophical grounds, to a state of morbid irritability, it will become necessary briefly to refer to the curative treatment adopted, as this in many diseases affords an approach to an *experimentum crucis* in demonstrating the true nature of the affection.

CASE VIII.—*Pyrosis, apparently idiopathic.*

Joseph Miles, æt. 64, came under my care at the dispensary on Nov. 21, 1840. He is by trade a watchmaker; was employed as a sailor until 1815, and has enjoyed through life a tolerably good state of health, especially since he left the navy. He has visited both the East and West Indies, and whilst there suffered from fever. For the last twenty years he has worked twelve hours a day generally before a bench in a half-bent position, frequently at the same time turning a lathe with his foot. His habits have of late years been extremely temperate, scarcely ever having been intoxicated, never drinking more than a pint of beer a day, and a little gin and water occasionally. His parents died of old age.

Nine months ago, this man first experienced a gnawing pain at the pit of the stomach after taking food: this appeared to continue during digestion, and was relieved by brisk eructations, or more generally by a gush of limpid fluid into the mouth. These symptoms, with considerable and rapidly increasing emaciation, continued up to the time of his admission. At that period he stated that he slept well, undisturbed by dreams; rises at six, and works until half-past eight, and during this time always feels well; he then breakfasts, generally merely on bread and butter, with weak tea. About ten or fifteen minutes after this meal a fixed gnawing pain appears at the scrobiculus cordis, with a sense of constriction compared to a cord drawn tightly round the chest. In addition to this, violent lancinating pains dart to the right side, back, and loins, and he complains bitterly of a fixed sore pain between the scapulæ,

confined to a spot the size of a crown-piece. All these symptoms increase for half an hour, when the flatulent distension of the abdomen becomes considerable; a throbbing sensation is experienced over the region of the transverse colon, dyspnœa comes on, and every attempt to breathe is attended with a severe pain of the left side. At last the stomach appears as though filled with burning coals; eructations, with gushes of a watery fluid, rush off from the stomach, and relief is obtained. The fluid in rising gives no particular sensation until it reaches the mouth, when a brackish saline taste is perceptible. This man then remains easy until tea-time, when, after this meal, he is seized with another, although much milder attack. It is remarkable that they scarcely ever follow his dinner, which he attributes to his not taking any fluid during the meal. His tongue is free from fur, vividly red at the tip and edges; has no disagreeable taste in the mouth on waking from sleep; perspires freely; bowels act daily, evacuations generally healthy; pulse 80, tolerably soft; skin cool. Complains of tenderness on pressing the scrobiculus cordis. Saliva examined half an hour after taking food, faintly alkaline.

The fluids vomited in gushes, after breakfast and tea, differed only in that collected in the evening being neutral, whilst that in the morning was generally alkaline: a specimen of each was submitted to examination.

Nov. 21st.—R Argenti Oxydi, Hyd. c. Cretâ aa. gr. vj.; Ext. Hyosciami, ðij. M. ft. Massa in pilulis xij. dividenda, e quibus sumet unam ter in die. Diet—For breakfast dry toast, thin slice of broiled bacon, coffee without sugar: dinner; mutton, old bread: tea—dry toast, and tea without sugar. All fermented liquors interdicted.

24th.—Bowels daily open; evacuations brown and rather offensive: states that the paroxysms of pain are less severe, and the gushes of fluid much decreased. His expression is certainly much improved.—Pergat.

Dec. 1st.—States himself to have been improving daily; scarcely any pyrosis left; bowels act daily; motions still dark, but not offensive; no headache; tongue completely clean, rather vividly red; appetite good. Has rigidly adhered to his diet.

Pergat.

3d.—Had a slight return of gastrodynia yesterday for a few minutes before dinner: this disappeared after the meal: no tenderness about the scrobiculus cordis.

R Infusi Chiryatæ, f ʒj.; Magnès. Carbo gr. iv.; Acidi Hydrocyanici, ℥ij. M. ter in die sumendus.

7th.—No return of pyrosis or gastrodynia: convalescing.

Pergat.

15th.—Discharged free from ailment.

CASE IX.—*Gastrorrhœa, apparently unconnected with any structural lesion.*

Elizabeth Simmons, æt. 38, admitted at the dispensary on Nov. 19, 1840. A tall woman, of dark complexion, engaged as a tobacconist: she has borne seven children, the last one eighteen months ago: has been a widow for a twelvemonth. Her previous health is described as excellent; menstruating regularly, without pain, and free from leucorrhœa. From the time of her husband's death her circumstances altered, and she suffered extreme distress. Three months prior to her admission the bowels began to be constipated, although previously always regular in their action; she then suffered severely from griping pain in the abdomen: soon afterwards an extreme sense of uneasiness after taking food appeared: this was generally relieved by partaking of warm fluids. Gushes of limpid colourless fluids, giving an icy cold sensation on rising from the stomach, made their appearance with tolerable regularity between the meals. She describes the sensation of a mass of ice rising from the stomach to the mouth with great accuracy. About an hour after each meal, the pain reaches from midway between the umbilical region and scrobiculus cordis to the left breast, and thence to the loin and back. These pains occasionally disappeared, as digestion became complete; sometimes, however, continuing all day, especially after partaking of indigestible food. A dinner of hard beef or of meat-pudding will at any time bring on these symptoms. She has lost flesh lately, and declares herself worn out by the irritation of the disease.

When the patient came under my care her aspect was remarkably anxious: she complained bitterly of frequent blinding

headaches. The tongue was slightly covered with a whitish fur, and serrated at the edges from the pressure of the teeth; the papillæ projecting and rather red. Pulse 80, weak; extremities almost always cold; frequent chilliness, succeeded by flushes of heat; urine apparently healthy. The quantity of fluid rising each time was rather more than half an ounce, and this was repeated four or five times during a paroxysm, so that the whole quantity varied from two to three ounces. It was generally neutral; sometimes very faintly acid. Bowels confined for three days.

November 19th.—The fluid vomited this day was submitted to examination: it was colourless and slightly opaline: by repose it let fall a few mucous flocculi, and the supernatant liquid was left transparent.

R Hydrarg. Chlorid., gr. iv.; Opii, gr. j. M. fiat. pilula hac nocte sumenda. Haustus sennæ cras mane sumendus.

20th.—Bowels several times relieved; motions dark, mixed with mucous slime, and very offensive. She expresses herself as much relieved, the usual griping pains not having appeared this morning. Severe sickness, and vomiting of bitter matter.

R Ext. Conii, gr. iv.; Pil. Hydrarg., gr. j. M. ft. pilula ter in die sumenda.

22d.—Bowels daily open; motions light brown, not offensive: no headache, nor any return of pain or vomiting since yesterday: the quantity of fluid from the stomach very much less profuse.

Rep. pilulæ.

27th.—Has been rapidly improving since the last report. Had a slight accession of pain between the shoulders this morning. No return of gastrorrhœa. Bowels act daily, with some griping.

R Infusi Chiryati, f ʒiss.; Sodæ Carbonatis, gr. xv.; Tinct. Hyosciami, ℥xx. ter in die. Sumat Pil. Hyd. Ext. Aloes Saponis, aa. gr. iij. omni nocte.

Dec. 1st.—No uneasiness; bowels act well; tongue clean: complains of a disagreeable sweet clammy taste in the mouth, especially on awaking from sleep.

Rep. Mistura et pilulæ sine Hydrargyro. Garg. Sodæ Chlorinata.

4th.—Convalescing. Pergat.

14th.—Discharged cured.

During the treatment of this case, the diet was rigidly restricted to bread and milk for breakfast; boiled mutton, bread, and occasionally light pudding, for dinner; and dry toast, with weak tea without sugar, in the evening. The

fluids poured out from the stomach in these two cases were submitted to the action of the agents used in the examination of saliva and ordinary gastric mucus, and the results are shown in the following table.

Case.	Joseph Miles.	Joseph Miles.	Elizabeth Simmons.
When fluid obtained	Evening	Morning	Morning.
Ebullition	No change	Slight opacity	No change.
Chlorine	Copious albuminous deposit	Copious albuminous deposit	Copious albuminous precipitate.
Nitric acid	No change	No change	No change.
Sol. potass	No change in cold; by boiling, a few flocculi	On boiling, a few brown flocculi	Renders it more limpid.
Sol. alum	Troubling	Opalescence	Dense precipitate.
Chlor. tin	Dense troubling	Precipitate	Dense troubling.
Chlor. iron	Deep orange tint	Pale orange	Pale orange tint.
Bichlor. mercury	Thick opacity	Precipitate	White troubling.
Nitrate lead	Dense precipitate	Dense precipitate	Slight precipitate.
Nitrate silver	Copious caseous precipitate	Copious caseous precipitate	Dense precipitate.
Sulphate copper	Green troubling	Copious green troubling	Slightest troubling.
Tinct. Galls	Troubling	Troubling	Copious precipitate.
Action on litmus paper	Neutral	Alkaline	Faintly acid.
Specific gravity	1.0091	1.0080	1.0058.

If these two cases be regarded as sufficiently defined in the character of the symptoms they present, to be considered as types of the two affections, pyrosis and gastrorrhœa, we cannot avoid drawing some interesting conclusions. In the first case we find symptoms of irritation so marked as to give rise to an impression that a state of erethism, not to say inflammation, of the gastric mucous membrane existed; and the distress of the patient increases to a point which at last becomes nearly intolerable. A sense of a burning mass (the *fer chaud* of the French) is experienced in the throat, and at last relief is obtained by a burst of limpid fluid. In the second case, the constitutional disturbance is smaller, the pains are rather dull, and more of a mere griping character, and resemble those experienced in ordinary dyspepsia; the fluid rises without much distress, and produces an icy cold sensation; the gastric inconvenience is diminished by warm drinks, whilst, in the first case, these increased it. The patient, it is true, is,

as she expressed it, worn out with the ailment; but rather, as is obvious from the history, from the constant interference with the function of digestion, and the evils attendant upon constipation, than from any high amount of constitutional irritation. It is in the class of cases of which that of Miles may be regarded as a type, that the remedy proposed by Dr. Johnson, the nitrate of silver, is generally so valuable: the oxide of that metal, lately introduced to the notice of the profession by Mr. Lane, appears still more fitted for administration in these cases, as it scarcely seems to produce any inconvenience to the patient, and, so far as is yet known, does not possess the inconvenient property of darkening the skin of the patients taking it; and I believe the experience of Dr. Johnson has sanctioned its general substitution for the nitrate.

On looking at the chemical evidence afforded by these cases, we are struck at once with the nearly identical characters of the fluids vomited in each,

notwithstanding the greater intensity of the irritation present in the first, as compared with the second case. The differences that do exist appear rather to depend upon the varying quantity than quality of the constituents of the fluids. The circumstance of the variation in the acid and alkaline state of the fluid observed in the case of Miles, is important, from its serving to show how futile and unphilosophical must be all attempts at dividing cases of dyspepsia, accompanied with gushes of fluid from the stomach, into different species, each requiring a peculiar treatment, merely from the acidity or alkalinity of the vomited liquids. We learn, moreover, that the fluid of pyrosis has no more right to be regarded as serous than that of gastrorrhœa, and that the hypothesis lately advanced by a very ingenious writer, that "pyrosis is a disease of serous vessels destined to secrete a fluid, which, when healthily acted upon by the other fluids, viz. the salivary, the oral, the pharyngeal, and the glandular mucoid juices of the stomach, becomes gastric juice*," is totally unsupported by the chemical evidence furnished by an examination of the vomited fluid. It is true that, occasionally, a tendency to the development of opacity on the application of heat is observed in some specimens of the vomited fluids, but as this as will appear hereafter, is to be regarded the exception (having occurred in only four out of twenty-four specimens I submitted to examination), it gives no support whatever to the idea that the serous exhalants have any thing to do with the secretion of the pyrotic fluid; indeed, the very existence of these vessels is at best extremely problematical, and even denied by the majority of observers.

It hence appears probable that in pyrosis and gastrorrhœa, the only real distinction exists in their exciting causes, and in the amount of local and constitutional disturbance present; and excepting that the specific gravity of the fluid is generally less in gastrorrhœa, or, in other words, appears to diminish with the decrease of local irritation or erethism of the secreting organs, we can scarcely point out a definite distinction between them. We find this view supported by analogy, for in as-

cites the result of peritonitis, and in that depending upon a contracted liver, no chemical distinction has been observed between the effused fluids, although in the former case they resulted from inflammation; and in the latter, at least in many instances, from exudation, the result of obstructed portal circulation.

The next question is attended with some difficulty, viz. what is the source of the vomited fluids? Until lately they have been regarded as strictly of gastric origin, but a late writer has stated his belief that they are "furnished by the salivary glands, and muciparous follicles of the mouth and throat;" and further, by "questioning the patient closely, he will be informed that the fluid runs from the mouth when nausea supervenes, but is not eructated or vomited, as Cullen, Mason Good, Abercrombie, Andral, and others, describe*." In support of this there has been started the difficulty of conceiving how the stomach could throw up several ounces of limpid fluid, and not part with the food it contained, when the supposed vomiting takes place during digestion. This argument is by no means a valid one, for the food may have passed the macerating and reached the digesting portion of the organ, and may be too closely grasped to readily escape. As this is, however, hypothetical, I may allude to two cases which appear opposed to the theory alluded to; first, in schirrus pylorus, as I shall have occasion to shew hereafter, in which disease no one doubts the light brown sour fluid loaded with hydrochloric acid, which is often so abundantly vomited, to come from the stomach; yet we constantly find this appears in copious gushes not only during digestion, but even a few minutes after food has been swallowed, and yet very frequently no portion of it is found in the vomited fluid—a circumstance which I have repeatedly heard patients themselves mention with surprise; so that the argument above alluded to falls to the ground. Next, I have repeatedly questioned patients regarding their sensations during the act of the supposed vomiting, and in every case of true pyrosis or gastrorrhœa, I have repeatedly heard them distinctly describe the feeling of the rising of a mass from the scro-

* Dr. West on Water-brash: *supra cit.*

* Dr. Burne on Habitual Constipation, 1840, p. 77.

biculus cordis as most distinct. A great difficulty is opposed to Dr. Burne's ingenious hypothesis of the source of pyrotic fluids, from the quantity in which they are occasionally secreted—a quantity for the secretion of which the small muciparous glands of the mouth and fauces appear inadequate; for the salivary glands must not be taken into account, as every observer must have noticed the great amount of local irritation and tumefaction which exists whenever salivation is present, and these organs pour out a large quantity of fluid. Again, in preternatural secretion of saliva, at least when the quantity is considerable, it appears nearly in a continuous flow, and not in those sudden paroxysmal gushes which characterise the appearance of fluid in the diseases under consideration. I shall have occasion to allude in another part of these papers to an unhappy case of ulceration of the œsophagus, in which upwards of eight pints of colourless fluid, differing very slightly from that of mere pyrosis, were poured out in paroxysms in the course of five or six hours—a quantity which we can hardly conceive the minute muciparous glands of the mouth and fauces to have yielded, even if obvious salivation, which was totally absent, had existed.

Lastly, chemical evidence, which unfortunately has not in my own investigations thrown much light on the matter, to say the least, does not support the view advocated by Dr. Burne. Unfortunately all our discriminating evidence rests simply upon the presence or absence of saliva, in any considerable proportion, in the fluid; and we have seen how little dependence can be placed on this circumstance, from the change which saliva itself undergoes when secreted in excess, as in ordinary salivation. Two or three chemical facts may, however, be alluded to, as tending to shew the improbability of pyrotic fluids consisting chiefly of saliva. Thus, the precipitate by nitrate of silver is infinitely more considerable in the most dilute specimen of fluid of pyrosis than in the densest saliva I ever examined; also the action of heat and nitric acid are very different. Thus, in a table published in a former paper, it appears that out of seven specimens of saliva, nitric acid produced an opacity in six, all of which were distinct cases of ptyalism; whilst in twenty-

three specimens of colourless gastric fluids, taken consecutively, this re-agent rendered only four opaque*. Ebullition again, which produced a troubling in the six specimens of saliva of ptyalism before referred to, affected only four out of twenty-four specimens of gastric fluids. Hence it may be assumed that, so far as observation can serve as a guide, both in relation to the patient's statements, and to the large quantity of fluid often poured out, and the little aid furnished by chemistry, that the fluids of pyrosis and gastrorrhœa, although necessarily mingled with a certain quantity of saliva in passing through the mouth, are yet strictly and essentially gastric in their origin. Still it must be owned that very considerable difficulty obscures this curious and important question.

[To be continued.]

OPERATION FOR STRANGULATED HERNIA.

To the Editor of the Medical Gazette.

SIR,

THERE is an important point connected with the operation for the relief of strangulated hernia by Petit's method, with which, at the London Hospital and in private practice, in numerous cases, I have found it extremely useful to be acquainted, and which I am desirous of bringing before your readers.

The object of Petit's operation is the division of the stricture which causes strangulation and the superjacent structures only, whereby the dangers supposed to be consequent upon the ordinary mode of operating by opening the sac are obviated; and the intention of the present communication is to detail the point alluded to, consisting of a method by which the exact seat and extent of a stricture may be accurately ascertained previously to the commencement of the required incisions. The advantages which such knowledge imparts to an

* I cannot too much regret, that a serious error should have crept into the last paper, in which the experiments on healthy gastric mucus are correctly detailed, but from the transposition of the words "gastric mucus" and "healthy saliva," in the remark that followed, the whole inductions are rendered unintelligible.

operator must be obvious, since it substitutes certainty for uncertainty in a matter where the former is necessary to direct and give precision to the steps of proceeding, and removes all embarrassment arising from the latter. Simple and easy of attainment as the information is, I am not aware that the method of attainment has been alluded to by authors on the subject of hernia, and there are various reasons which induce me to think that its publication will be attended with benefit; the principal of which is, the conviction that the increased facilities which it affords to the performance of an operation against which the difficulties of its execution has been urged as a great if not a chief objection, will eventually tend to cause its more extensive adoption in practice.

At present it is not my intention to canvass the merits of Petit's operation, yet, in reference generally to the operation for the relief of strangulated hernia, it might be asserted that, exclusive of the fear and dread of the knife which usually acts upon the patient's mind, there are two causes which influence the mind of the surgeon, tending to create on his part a temporizing and procrastinating practice, highly prejudicial, in most cases, to the patient's real interest. These causes are, the general knowledge which he has acquired of the unfrequent fatal termination of hernia when returned by the taxis, and the frequent fatal sequence of operations. The almost necessary effect of such knowledge is to stimulate every endeavour to succeed by the taxis alone; and the hope of ultimate success induces a continuance of the endeavour often beyond the period which a due regard to the precarious condition of the hernial contents justifies. It would not be very difficult to shew that the frequently fatal sequence of operations just alluded to, is more fairly attributable to lesions of viscera consequent upon this long-continued exposure to a strangulating cause, previous to the performance of an operation for their relief, than to the direct effects of the operation itself, even when conducted in the least safe, but ordinary way, by opening the sac, and that the *propter hoc quia post hoc* argument is not generally more applicable, as referable to the operation, than it is in many

other cases in medicine, in which it is so extensively abused. The certainty of the fact, however, of the frequent fatal sequence of operations, from whatever cause it arises, tends to the continuance of the practice of procrastinating, until the time when the operation imperatively must be adopted, too often as a remote and last chance for the salvation of the patient. Of the two causes mentioned as influencing the surgeon's mind, it may be fairly presumed that the last, or the dread induced by the frequency of death after operation, is the most cogent in its effects, for it cannot be supposed that he would willingly allow an often intense and protracted suffering to continue unaided by some certain certain means of relief, unless under the impression that those means were attended with great danger to life. If, therefore, such presumption be correct, the best and most useful endeavour will be to strip the operation of its terrors, by substituting in its place one simple in its execution, and devoid of the ascertained dangers to which it exposes the patient. By the adoption of Petit's operation in the place of the ordinary operation, I think this beneficial substitution is attained; an opinion which, if equally impressed upon the minds of surgeons in general, will doubtless remove the chief obstacle preventive of the early recommendation of operation, after the usual aids to the taxis have been tried, but before those changes of structure have taken place which militate so often and so powerfully against its successful result.

It is an impression pervading the minds of most experienced surgeons, that the general amount of mortality from strangulated hernia would be far less, provided there was generally less delay in giving relief from strangulation. The diminution of mortality would probably be still more evident, provided such relief was early and generally afforded, without exposure of the viscera, after the manner of the ordinary taxis; the assimilation to which is the chief recommendation of Petit's operation. In bringing before the profession the simple means of diagnosis of the seat of stricture stated below, I trust that by facilitating it will tend to increase the frequency of the application of an operation destined

for so beneficial a purpose. The operation itself admits of a very brief general description. It consists of an incision of the integuments over the seat of stricture, followed by a division of the subjacent cellular texture and fascia, to which succeeds the cautious division of the stricture itself; and afterwards the hernial contents are returned into the abdomen, as by the taxis, without exposure.

In femoral and in umbilical hernia, for all practical purposes of the operation, the seat of stricture may be assumed to be at the respective abdominal apertures, although in the former hernia there is occasionally some slight variation upon that point. Thus in one case it was above Poupart's ligament, produced by a transverse band of fascia crossing the entrance of the funnel-shaped canal into which the hernia descends in its passage from the abdomen, and required the almost complete division of that ligament to enable the finger to be carried under it as a guide for the bistoury with which it was divided. In another, the stricture was situated at least half an inch below the ligament, but easily detected and divided.

In inguinal hernia the seat of stricture is far more variable, and the range of its variation extends from the internal abdominal ring to the scrotum, in the male; consequently, without some previous indication to guide the operator, the external incision may be made over one extremity of the range, when the stricture is far away at the other, and a fruitless search may be the probable result. It is therefore apparent that the rings in inguinal hernia cannot generally be assumed to be the seat of stricture; or if this assumption was admissible, it would still remain to be determined which of the two rings was implicated in any given case. There would consequently not be any defined and ascertained point to which to direct the incisions, even when, for a successful performance of the operation, the greatest precision is desirable in that respect.

There are several ways of conducting the necessary examination to obtain the desired diagnosis, all of which depend for their success upon the stoppage of the communication of impulse from one part to another by the

stricture. Thus, if the body of a hernial tumor be compressed by the hand, an impulse is communicated to all its parts below the seat of stricture, but if the neck of the hernia be grasped between the finger and thumb of the other hand, above the stricture, while such compression is made, there will not be any impulse felt. When in the commencement of the examination the neck of the tumor is first grasped, we may be always assured that if an impulse is felt on compression of the tumor itself, the seat of stricture is nearer to the abdomen; and by gradually withdrawing the finger and thumb in that direction, while renewed compression of the tumor is made, a point will be soon reached at which impulse ceases to be felt. The point at which impulse first ceases to be felt is the seat of stricture. In like manner, if an impulse is not felt when the neck of the tumor is first grasped, we may be equally assured that the stricture is situated nearer to the body of the hernia, and by a like gradual approximation to it with the finger and thumb, an impulse shortly commences to be felt. That point is the uppermost part of the strangulated contents, which implies that the stricture is immediately above it, and on inquiry it will be found to correspond with the indications of an examination commenced from below, as just mentioned; and thus the two modes of examination will tend to the correction of errors, to which each separately is liable.

The same information may be obtained by attending to the point of cessation of impulse when the patient coughs; but this method is irksome and painful under circumstances of acute peritoneal inflammation, and on that account is not so generally desirable as that detailed. Yet much valuable information, not otherwise attainable, may be afforded by using this method in conjunction with that furnished by compression of the tumor. In some cases the stricture occupies a considerable portion of the neck of the hernia, but in most it is confined to a limited space. In the former case, before performing Petit's operation, it is desirable to be acquainted with those limits, in order that, when extensive, suitable provision may be made for its complete division. The combination

of the two examinations has this knowledge for its object, which is easily attained by attending to the points of cessation of impulse.

The point of cessation of impulse on coughing indicates the upper boundary of the stricture, and the point of cessation of impulse on compression of the hernia indicates the lower boundary of the stricture; and consequently the boundaries are the limits of its extent. Additional evidence may often be derived from a careful examination of the part thus indicated, by the detection of a firm band felt through the integuments, and apparently encircling the space occupied by the stricture, and affording satisfactory confirmation of the accuracy of previous conclusions.

By these various evidences the seat of stricture, and consequently the object of search in an operation, is clearly defined, while its incisions, having now a point, the seat of which is determined, assume a precision and certainty which they could not otherwise possess. Yet various causes, which I pass over without particular notice, prevent the uniformly successful performance of Petit's operation, and the ulterior necessity of opening the sac, either partially or entirely, occasionally arises, to secure the return of its contents into the abdomen.

It does not, however, appear that the previous and unsuccessful attempts at all enhance the dangers of the patient's situation, or diminish the value of the prospects of the proceeding subsequently adopted. The greatest evil arising from the attempt is, that it has been attended with some pain; and the chief misfortune is, that it has not succeeded.

The probability of the necessity to open the sac, to effect a return, is much increased, when the stricture is caused by its thick and indurated neck. This condition, be it observed, is asserted by some surgeons to render Petit's operation impracticable. The assertion is much too absolute, and moreover is far from being correct; which a little experience in the following method of proceeding will demonstrate to be so. When the stricture is situated exteriorly to the sac, a director may generally be easily introduced under it, and it may be as easily divided

with a bistoury; but when the neck of the sac itself forms the stricture, no such method can be adopted. In such cases its division should be accomplished by cutting the indurated and thick substance on its exterior surface only, taking the greatest care that the knife does not penetrate to the interior of the sac. If this step has been properly performed, the division is only partial, and little amount of relief will ensue. To render the relief effective, the partial division of the indurated neck should be repeated in one or two other situations on the circumference of the stricture, by which at length the stricturing substance is so far weakened in its power of resistance, especially when unsupported by surrounding structures, that it becomes susceptible of dilatation by the very moderate interior pressure of the hernial contents during the efforts of the taxis. Success does not frequently attend the first efforts, so that renewed partial divisions, and renewed efforts, are mostly required; and however unpromising such cases may be, experience enables me to state that in this way they often admit of relief without the necessity of opening the sac.

The proportion of failures in the attempt is, from its nature, greater than that which is experienced in herniæ strictured by the margins of the abdominal apertures; yet inclusive of these failures I have not any reason to consider their general amount large, having sustained only five failures out of thirty-two cases on which I have attempted to leave the sac unopened;—a proportion which does not by any means justify the general condemnation of the operation, from its very frequent impracticability, while the nearly uniformly favourable termination of those cases in which the operation was successfully completed, almost justifies the assertion that, when adopted in suitable cases, before incurable visceral mischief has been implanted, it affords a chance of recovery to the patient little if at all inferior to that afforded by the taxis itself.—I am, sir,

Your obedient servant,

J. LUKE.

39, Broad Street Buildings,
Aug. 16, 1841.

FAILURE OF THE OPERATION FOR SQUINTING.

To the Editor of the Medical Gazette.

SIR,

THE operation of dividing the muscles of the eye, which is alike remarkable for its apparent hazard yet beautiful simplicity and perfect safety, has now been before the public nearly two years, and it is to elicit the results of this operative procedure in the hands of others, and to ascertain how far they accord with my own, that I am induced to address this brief communication to you.

There might possibly be some little merit in being the first provincial surgeon to practise the operation after its announcement to English readers by Mr. Bennett Lucas; but, without quibbling with dates, I would merely state that immediately on seeing that gentleman's cases in the *Lancet*, I wrote to him concerning some particulars connected with the instruments, and I take the opportunity of publicly thanking him for his kind and attentive communication.

My first case was that of a female, who had squinted for nearly thirty years. The operation appeared to be quite satisfactory for about ten days, when the eye began to resume its wonted obliquity—from what cause I know not; but thinking that possibly it might arise from imperfect division of the muscle (for the poor woman was persuaded with difficulty to submit to the completion of the operation after the division of the conjunctiva) I resolved in future to remove that doubt by a free and decided division of every fibre of the muscle. It would be as well to state, not that the accident had any share in the production of it, that the eye was not observed to alter its direction from perfect straightness until after a fall from a cart, from which my patient sustained considerable bruising of the whole of that side of the face, and of other parts of the body.

In every subsequent operation great care has been taken effectually to divide the muscle at fault, and in no case that I have since seen have I observed the eye to regain its *former* obliquity, but in all the cases the progress of which I have watched (some from distant parts of the country have been lost sight

of) there has been a gradual turning of the eye in the *opposite* direction. The muscle divided in my cases has invariably been the internal rectus, and the eye has assumed an obliquity externally. This has not been observed to take place immediately, or in a few weeks, or even months. A little boy, upon whom I operated in August last, has presented himself to my notice, from time to time, since the operation. For ten months the eye remained quite straight to the satisfaction, and, in fact, delight of his relatives; but at that time I noticed a slight inclination of the eye outwards, and during the last two months considerable deformity has been thus produced.

Other cases have terminated in a similar manner. The operation has been successful; the cure has been considered perfect for weeks, and even several months; and then we have had the mortification of witnessing all our efforts frustrated by the gradual accession of a deformity nearly as great as the one which originally called for surgical interference.

It would seem that the antagonizing muscle having been destroyed, its opponent becomes gradually and permanently contracted and shortened; but the reason of its occurrence, after having been kept in a relaxed or even stretched condition for years, even by the contraction on the other side—also the reason of its occurring at so lengthened a period after the operation—must be left to the physiologist to decide.

In the first place, it would be desirable to know the result of the operation on the whole—to ascertain the condition of those eyes operated upon immediately after its introduction. It would be well for those who have had their scores and hundreds of cases, to beat up for their early patients, and give to the profession a faithful report of the present condition of the organ.

Certain it is that the periodicals of the day present a vast difference with regard to this operation within the short space of twelve months: about that time ago every journal teemed with successful cases, peculiar plans of operating, or descriptions of instruments; and one was almost inclined to regard it as a happy discovery of the age; but alas! how has the scene changed! there is scarcely a straggling communication on the subject in six months; and, as

though there were a wish to hush up past performances, there is now not even an allusion to it.

I here give the result of the operation (which is far from satisfactory to my own mind) as performed in my necessarily limited sphere of country practice, and call upon those who have practised the operation so much more frequently than myself to give a candid and impartial statement of the present condition of the organ operated upon within six months after the introduction of the operation, that we may judge of the propriety of having recourse to the operation in future,—of adopting it as generally successful, or of banishing it, as one followed, in the majority of cases, by consequences nearly as little to be desired as the previous deformity. I would make the same appeal equally to our own surgeons, amongst whom I would mention Bennett Lucas, Liston, Guthrie, and Radclyffe Hall, as having published the greater number of cases; and to Dieffenbach, Phillips, and others, on the continent, where the operation first originated.

Hundreds of cases have been published as perfectly successful; but I have observed in some few that the remaining muscle (formerly the antagonist of the one divided) has drawn the eye permanently into the opposite direction; that this has not taken place immediately, but after a longer or shorter interval, and in one case not even until ten months afterwards, so that up to such period these cases might have been represented as successful; and I would fairly ask how it is with the numerous cases referred to? At the same time attention may be directed to another circumstance which has occurred to require a second trifling interference. In one case of my own, granulations about the size of a pea formed upon the conjunctiva at the point divided by the knife, which were readily removed by the scissors. I have also been applied to by a gentleman who had similar exuberant granulations after being operated upon for a strabism by an eminent surgeon in London. In no case, however, have I noticed a like formation after the opening had been cleanly made into the conjunctiva by means of the forceps and scissors. In my first cases I used the small knife and sharp hook; with

both of which I subsequently dispensed as unnecessary, attributing the granulations above referred to to a somewhat greater degree of laceration of the conjunctiva by their use. Latterly, the only instruments I have used are Pellier's speculum, dissecting forceps with accurately fitting points, blunt hook, and curved scissors. Children I have found it necessary to roll up in a large sheet, to prevent the least movement of their limbs.

In conclusion, I trust that it will not be thought that I am raising unnecessary or uncalled-for objections to the operation. Mine are not the objections of a prejudiced veteran, but of one who hails with pleasure every sound suggestion towards the amelioration of his fellow creatures; who, in this particular instance, has given it what he considers a fair trial, but has ~~not~~ been satisfied with the results thereof.—I am, sir,

Yours most respectfully,
T. HERBERT BARKER.

Bedford, August 16, 1841.

ANALYSES AND NOTICES OF BOOKS.

“L'Auteur se tue à alonger ce que le lecteur se tue à abrégé.”—D'ALEMBERT.

The Physiology of Vision. By W. MACKENZIE, M.D. &c. 1 vol. 8vo. 1841.

THIS work is designed to supply to the medical student a succinct account of the physiology of the eye, for his study previously to, and coincidently with, the study of its diseases. The author states that in his lectures on ophthalmic surgery he had constantly felt a difficulty in recommending any work in which this subject was treated of clearly and briefly, and at the same time, in a more complete manner than it is possible to teach it in surgical lectures. To remove this difficulty for himself and others, he has published the present work, which is certainly better fitted to accomplish the purpose than any with which we are acquainted. It does not pretend to any novelty; but it possesses the rare one of treating of a difficult subject with distinctness. Its materials are culled from all good sources; it therefore combines most of the advantages which are separated in the best works

that treat exclusively either of optics as a collection of geometrical problems, or of the eye as one of the organs of sense. It can be confidently recommended for the study of a medical pupil in the second or third year of his school career.

Three Memoirs on the Development and Structure of the Teeth and Epithelium, read at the Ninth Annual Meeting of the British Association for the Encouragement of Science, held at Birmingham in August, 1839: with Diagrams. By ALEXANDER NASMYTH, F.L.S., &c. 8vo. 1841.

NEARLY at this time in last year we took so prominent a part in the discussion respecting the claims of Mr. Nasmyth and Mr. Owen to discoveries in odontography, that we do not think it necessary again to enter on a subject alike unpleasing and unprofitable. We shall, therefore, only state that the present work includes the original and hitherto unpublished materials of the memoirs which were the foundation of that dispute, with notes in defence of the author's rights and of the charges brought against him, and a series of neatly lithographed diagrams. The preface contains also an account of the treatment which Mr. Nasmyth received at the hands of the Council of the British Association; after reading which it is difficult to believe that the proceedings of that body are not marked by illiberality, as well as by the frivolity and inefficiency for the advancement of science which has already rendered them ridiculous.

MEDICAL GAZETTE.

Friday, August 27, 1841.

“Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”
CICERO.

ON THE MANAGEMENT OF THE INSANE.

We have just received, with very great pleasure, a translation of Dr. Maximilian

Jacobi's work “On the Construction and Management of Hospitals for the Insane*,” and we desire to recommend it strongly to our readers, not merely because the arrangements of the Institution at Siegburg, to which it especially relates, appear to us to be *in general* excellent, but because the whole work is composed with a rare practical knowledge of the subject, and with a regard for science, which have seldom marked the recent publications of the same class.

In the present state of the therapeutics of insanity, the construction of the Hospitals for the patients is unfortunately one of the most important circumstances to be considered: we say, unfortunately, for it is surely to be lamented that the powers of strictly medical treatment are at present so unavailing, even when the disease depends on no unchangeable structural disorder, that the proper construction of the abode of the patient is almost essential to the success of his treatment. In all that relates to this, the reader may find much that is valuable in the work before us; the observations both of the author and of Mr. Tuke are such as may suggest improvements in every institution for lunatics in the country; and we regret that we can afford no space for an abstract of either of them, and must confine ourselves to some general observations which they suggest.

The management of the insane in this country is, at present, it must be confessed, in a most disordered state; it is altogether systemless and inefficient. Considering that insanity is a disease which it is scarcely possible to treat efficiently at the patient's home, and in which, therefore, all of all ranks are reduced to the condition of hospital

* Translated by John Kitching, with Introductory Observations, by Samuel Tuke, Treasurer of the “Retreat.” London, 1841. Churchill.

patients, it is obvious that the most perfect scheme would be that in which, with such a diligent surveillance as would prevent oppression, all lunatics who are not manageable at home should be confided to persons authorised for their fitness' sake to their exclusive care. But what is now the case? Of the lunatics among the poor, and even of some of those from among the middle classes, the great majority are committed to almost pauper asylums, where the efficacy of treatment, and the fitness of management, are, with few exceptions, marred by the necessity for economy. Excluded by the very nature of their disorder from hospitals whose ample funds might provide them all that they need, lunatics from these classes are consigned to asylums supported not by the liberal hand of private charity, but by the slowly-opening public purse. And they live the lives of paupers: in most cases, the buildings provided for them, and all their domestic arrangements (in which are, in a great measure, involved their therapeutic management), are ill appointed, and for want, or for economy of funds, cannot be mended.

Yet it may be questioned whether, on the whole, the condition of the lunatics from among the poor is not better than that of patients of the same kind in a higher class. In the public asylums there are at least the advantages of experienced attendants, of generally appropriate diet and medical treatment, and still more of the regular superintendence of licensed and competent medical practitioners, including, in some instances, a physician, a surgeon, and a resident apothecary. But the lunatics from the class whose friends can afford to pay for attendance, by no means generally fare so well; and in this consists the greatest anomaly of our system. There are around the metropolis and, in gene-

ral, in the neighbourhood of each large town, lunatic asylums superintended, or, more properly speaking, *kept* by medical practitioners, who, devoting the greatest part, or the whole of their attention, to this department of their profession, must be regarded as competent to the charge of the insane; and some of them are undoubtedly eminently fitted for the office. But, the proportion of lunatics who are thus provided for is unhappily extremely small; the terms of maintenance in such institutions as these are either absolutely more than the friends of patients can afford, or they are more than they regard as equivalent to the difference of the advantages between such an establishment and one of a lower grade in value and price; and so, the comfort, or, it may be, the health and sanity of the patient, is made to wait upon the economy of his friends; and that, though the money they expend upon him be his own.

In these minor and cheap establishments, the superintendent has rarely any knowledge at all of the proper medical, or even of the moral, management of the insane. Some of them are kept by gentlemen, whom we suppose we may call amateurs, and who take this mode of earning an income through an accidental preference for it before the pursuit of some profession or trade to which they would be better suited. Others of these asylums are managed by ladies, who of course cannot be expected to be qualified for more than the duties of matrons or nurses under proper government. Others, again, are superintended by retired keepers, who, having raised some money in their subordinate offices, lay it out to good interest in an establishment of this kind, the low terms of which seldom fail to bring them patients. And, in addition to all these, a number more of lunatics are committed singly to the

charge of keepers, who, for various reasons, (their own profit, misconduct, or any other), have left authorized lunatic asylums, and taken up business on their own account. Now surely in all these cases there is grievous error. Till the condition of a lunatic is absolutely hopeless he ought never to be removed from the charge of one who, by some test of his knowledge, has been proved competent to treat his case: and, even when the incurable state is really set in, surely it is not too much to require that the guardians of those who cannot have safe charge of themselves should have some proved judgment to guide them in the management of their patients' health and comfort. Such a condition must be demanded, unless it is to be admitted that the life of a lunatic is not worth prolonging.

We have no intention of entering at present into the details of a measure by which these evils might be remedied. We will only express the opinion that the main feature in it should be, that no lunatic should be lodged in an establishment kept by any but a licensed medical practitioner, till, on sufficient testimony, he can be regarded as positively incurable. Unless it can be proved that there is no hope of applying the knowledge of medicine beneficially in the treatment of insanity, nothing can be more flagrantly wrong than that it should pass into unauthorized hands. In the case of every other speciality of medical practice, nothing is more certain than that the heaviest clog upon its progress, and therefore the greatest injury to the public, has resulted from its falling into the hands of those who have not been educated for medicine. Look at aural, and ophthalmic, and dental surgery! What amount of good has ever come from mere aurists, or oculists, or dentists, to compensate for the thousands of eyes,

and ears, and teeth, that they have destroyed by their ignorant quackery? The whole science has in each of these cases been established by the labours of surgeons and physicians, who have cultivated the one or the other as their favourite branch on the main trunk of medical knowledge; but in no instance has the branch flourished when stolen off from the tree and planted by itself. And just so must it be in every other case; but in none more than in the treatment of insanity. If either the present, or any future generation of lunatics, is to be benefitted, it can be done only by practitioners of medicine, who will make their cases their especial study; and who, for their reward, as well as for the public good, deserve to be protected against the competition of self-licensed men and women.

But it has happened unfortunately for lunatics that of late they have become too much objects for public curiosity; for, mistaking the softness of their feelings for the strength of their intellects, too many from among the public have lately interfered in this matter. Newspaper discussions, anonymous letters, magisterial suggestions, and books by authors of all grades but the right, have abounded far too much for the advantage of the patients. A certain amount of openness to the public is undoubtedly a necessary safeguard for the inmates of asylums; but such a safeguard is amply provided by the visits of authorized persons to see that no oppression is exercised; and it is altogether beyond the right of the public to force their opinions on any part of the therapeutic management. The profession has been degraded by this recent clamour about a non-restraint system: if, as we have said once before, it be the best adapted for the patients' good (and, on the whole, we believe that it generally

is), let it be proved so by the honest evidence of facts; but let not medical men competent to judge have the system forced upon them by a mere popular cry—by anonymous and senseless appeals to feelings—by pathetic books by ladies—or even by the ambiguous solemnities of magistrates. No good can come of these: to every one who would study and practise his profession as a science, a scheme thus supported must appear, *primâ facie*, a bad one; and no one can, without danger to his professional character, embark in a common cause with those that will sail with the wind of popular expressions of this kind. Next to its falling into the hands of designing quacks, nothing is more dangerous to the progress of a science than its becoming a subject of popular curiosity.

It is this interference of the public in the management of lunatics which has led to the degenerate character of modern publications on the subject; but we hope that Dr. Jacobi's work will assist in calling medical practitioners to better thoughts. Much time and much energy are being now squandered in the discussion of a matter of detail, on which, if there be any doubt, it is only because at present the facts necessary either to establish or to overthrow the supposed value of the new method are insufficient. But in the midst of this discussion, the spirit of which is maintained only because it is carried on in the view of the public, the much more important general questions of the construction of asylums, and the competency of their superintendents—of medical treatment—of morbid anatomy; and, in a word, of all the real science of insanity, are almost utterly lost sight of. Yet on these, much more than on the kind and quantity of restraint to be employed, does the patients' welfare depend. Nor is his alone concerned;

for while these things are publicly discussed, the medical man is being gradually more and more supplanted from that for which he is alone fit, and which should therefore be his exclusive privilege—the charge of cases of curable insanity.

SUBSTANCE OF A CLINICAL LECTURE

ON

CANCER OF CICATRICES,

Given at St. George's Hospital, May 18th,

By MR. CÆSAR HAWKINS.

THE subject for to-day's consideration is suggested by the case of a man in Wright's ward, named William Ward, whose limb I amputated three or four weeks ago, as well as by some other cases which have recently been under your notice, as they are instances of a disease that is not commonly understood, and erroneous practice, founded on misunderstanding of its nature, may lead to the cases being trifled with, till it is too late to adopt a successful line of treatment.

This man, Ward, thirty years of age, was admitted into the hospital February 24th, with extensive ulceration reaching from the upper edge of the patella to below the middle of the leg, the surface being full eight inches by five in diameter; in one part, just below the head of the tibia, the ulcer is deeply excavated, from destruction of the anterior part of the bone. A little exposed bone can be felt with the probe, and the leg bends easily at this part; it does not appear, however, that the bone is entirely destroyed, as lateral movement only bends the bone slightly, in comparison with its flexibility in the other direction. The joint does not appear to be at all implicated in the disease. His health has lately declined, and the countenance is pale, but the bowels are regular, and the appetite good; pulse rather quick. He is kept awake by pain, but does not appear to suffer much when the limb is moved or bent.

He states that, twenty years ago, his leg was much injured by being caught in some machinery, after which several pieces of bone came away, but it remained well till the last year, and he was able to use the limb till six months ago, previous to which, ten months before the present time, the skin became ulcerated over the patella, and gradually spread, downwards chiefly, to its present size, the extension below the skin into the bone having only recently taken place. The skin round the ulcer is thickened, and the margin is much raised and hardened, and

irregular; and here and there on the ulcer is an imperfect warty appearance, the surface of the ulcer being very red and florid, but being much more irregular and more condensed than common granulations.

The limb was at first placed in a fracture box, and lettuce and opium given to quiet the pain, and a sedative lotion of prussic acid used, which gave much relief, as it usually does in malignant sores. There being in a few days no doubt of the nature of the case, amputation was proposed to him, but declined.

On the 2nd of April some suspicious enlargement of the glands in the groin took place, but I believe it was only owing to some stimulant having been applied to the sore at one part, the pain from which perhaps contributed to make him at last consent to the only means of cure.

I accordingly amputated the thigh on April 10th, and the stump is now nearly healed, and he has been walking about for some days. The appearance of the disease is seen in the preparations before us; but I will return to that part of the subject presently.

The nature of this case has been described by me in the nineteenth volume of the Medico-Chirurgical Transactions, in a paper in which I have related the results of several cases, the preparations from some of which are on the table before us, together with several others which have occurred since, and which will serve to show you every stage of the disease, which is *cancer* in the imperfect skin of a *cicatrix*.

There takes place, then, an ulceration or morbid growth in old scars of any character, such as those arising from burns, gun-shot wounds, floggings, or common ulcers.

1. Here is a preparation of an ulcer of this character that I took, three or four years ago, from the back of the hand of a man who had had a sore in this situation for twenty years in the cicatrix of a burn, which extended for some distance beyond the ulcer. It was of the size of half a crown, hard and uneven, with a very tender surface and irregular figure, and in part warty in appearance, with a hot pricking or smarting pain running up the arm. Till the last year it had healed from time to time, but since then it has resisted all applications, and has spread. After trying arsenic and other remedies for a little while, I felt satisfied of its nature, and dissected it off from the extensor tendons, which preserved very fair motion after the wound healed. In this case, then, what had for some time been common ulceration at last assumed a new action; and you may see that, although very little warty on the surface, it yet consists of a great number of tough hard fibres, perpendicular to the surface, which may easily be torn asunder;

and the tumor is in fact exactly like the hard base of many cases of cancer of the lips.

2. In a further stage, or else anterior to the formation of the hard base of this case, you may see an exuberant growth of firm red warty granulations or fungus, elevated an inch or more above the skin, such as some of you may remember in a man named Gale, who was under my care two years ago with a tumor about three inches long, and nearly two broad, situated on the outside of the leg in the cicatrix of an old and large varicose ulcer, the tumor having begun about nine months previously, while the ulcer had first formed eleven years before. The tumor was attached to the fibula, but I believed the bone was not diseased below the periosteum, and that amputation consequently was not necessary. I therefore removed the tumor, and scraped off the surface of the bone with a chisel, and after some trouble, in consequence of the hard cicatrix which extended all round the leg, the wound healed.

3. The disease forms a very considerable sized tumor in some cases, as in these two preparations in which the new growth originated from flogging on the shoulders; or in these preparations from a leg, which I amputated for the disease a few years ago. The tumor in this case extended quite round the leg, and was two inches high, and about four inches broad, and gave excessive pain, having come on a few months before in the cicatrix of a burn, which the patient had had from childhood. Instead of the warty appearance before described, the disease is now, as you may perceive, composed of a firm vascular substance, with round elevations of some size, still capable, to a certain degree, of being torn down in fibres, but bearing more resemblance to a medullary tumor when very vascular. You may see that it only goes down to the fascia and periosteum; even when of this size, the bone, although thickened from inflammation, being still otherwise healthy. This patient at first refused to submit to amputation, but on coming to the hospital one day, I found she would have the operation performed if I would do it immediately; of course I did not refuse, and I saw her some time afterwards without any return of disease.

When I wrote the paper I have alluded to, I expressed my opinion that the disease was one of a malignant nature, but that it was purely a locally malignant disease, as far as I had seen it, and that if it was of the same nature as scirrhus cancer, it was so in the very lowest degree; that it consisted of a new structure, with the power of contaminating the adjoining soft tissues, and converting them into the same disease, and consequently that its entire removal by the knife, or total destruction by caustic, was necessary to effect a cure; but that the low

degree in which it was malignant was evinced by a fatal case I had examined after death, in which, although it had spread so as to occupy a space of eighteen inches by ten on the back, it had even then caused no affection of the absorbent glands, nor occasioned the formation of any morbid structure in other parts of the body.

I remember, when this paper was read, that some gentlemen present expressed an opinion that the warty ulcer in question was curable; but in fact it is not so; the new growth is incapable, as it would seem, of cicatrising: destroy it as you will to the level of the rest of the skin, yet if any portion whatever of the morbid substance remain, it will soon show its malignant nature again by fresh growth, as in this beautiful preparation, which I made when I was House-Surgeon here, from a patient whose leg was finally amputated, after all sorts of applications had been tried without success, including several escharotics, and the actual cautery. The peculiar nature of the disease may be sometimes seen in an old ulcer of long standing, in such a manner that only one part of it, or one ulcer out of several on the same limb, may be cancerous, while the rest has the same appearance which it has possessed for years previously.

Rayer, in his excellent work on the Skin, alluding to my paper, or rather to an abstract of it, which he had seen in some journal, supposes the new growth to be an hypertrophy of the papillæ of the skin, whence it derives its warty form; but this is by no means the case, for it affects, as you may see, not the papillæ only, but the whole texture of the cutis, and the preparations show you how different the tumors are from others of common warts, even when very large and numerous, as in this great mass, which I removed from the labium.

Neither is this disease in a cicatrix like the exuberant fungus you may sometimes see, as in this cast, rising out of the cancelli of a bone when it is carious; the fungous granulations in such a case being the result of irritation only, and ceasing when the local condition or the state of the constitution are altered for the better.

Subsequent experience in a good many cases has confirmed me in most of what I then stated, but has shewn me also that the growth of this kind of cancer in cicatrices is more malignant in its influence than I was at that time inclined to believe, and that it bears much resemblance to ordinary cancer of the skin, of which it is evidently a variety, though still it is less virulent than any other variety of cancer which I have seen: its mildness depending probably on its being produced at an earlier period in the imperfect structure of an ulcer than it would be in healthy skin, with a corresponding constitu-

tional tendency to the development of cancer on the application of the necessary excitant, and being therefore also less virulent in its contaminating properties on the parts around, or on the glands or system at large.

Cancer, developed even in original sound skin, instead of a cicatrix, is, however, milder in its usual form than is generally imagined, because we are more familiar with it in other textures, in which its rapid growth and fatal influence are strikingly evinced. Cancer of the skin, for instance, is much less violent than cancer of mucous surfaces, although the two textures are so much alike in their structure and properties. In the penis, or clitoris, or labia, the violent pain, and early influence on the glands, and the frightful sufferings and death of the patient, are very different from cancer, as it usually appears in the skin. You have lately had an excellent example of this in a poor woman named Gaylor, in whom the disease originated in the vagina and labia, and you saw how the glands were enlarged and ulcerated in the groin, and what numerous cancerous tubercles of a secondary form were developed in the skin of the thighs and adjacent parts. It is curious that cancer of the cutaneous structure should be so mild in the majority of cases, since the skin seems to present so active an absorbing surface for various purposes; yet such is doubtless the fact. Here is a preparation of a cancerous tumor, which I removed two years ago from the skin of the sternum in a patient in this hospital, which shews you the same warty appearance of solid texture formed by the disease in common skin, which our other cases present in cicatrices. In this case the tumor was supposed by the man, who was between forty and fifty, to be a common mole, which he observed twenty years before he came here: ten years afterwards it ulcerated, and did not heal again, and within a few months of the operation it began to be painful, and spread more rapidly. He has remained well, I believe, since I removed the diseased part. In another case an elderly man came under my care with a large cancerous mass of warts in the same situation on the sternum, which had ulcerated about four years before, and on his admission was about six inches long, two broad, and one and a half high, with great pain, and an exceedingly foetid secretion. I wished to have excised it, and to have scraped off the surface of the sternum, which was superficially affected, but he would not consent to it, though I dare say it would have succeeded, as notwithstanding the size of the tumor and its long continuance there was no apparent contamination of the glands in either case. You have recently seen this tumor removed by Mr. Tatum from the back, which was apparently not in a cicatrix, but yet looks

like the later stage of cancer in that variety of disease when it forms the large tumor I have spoken of; but unfortunately in this case the glands were much affected, and the patient has died since the operation.

The similarity of the local appearance of the two forms of cancer is therefore very evident; but when I wrote the paper I have referred you to, I had not seen any cases which shewed me, as subsequent experience has done, that there was also so much similarity in their course in other points connected with their malignant properties. First, as to the contamination of adjacent parts. I had seen the disease affect the fascia of periosteum, and then cause enlargement, and thickening, and consolidation, of bone, or some degree of ulceration in the cancelli; but it may do more than this, as Ward's case has shewn you, and there may be therefore occasionally some difficulty in ascertaining how much disease in any part is really cancerous, and how much is simple inflammation. You have a few days ago seen the thigh amputated by Mr. Babington, and the case is in many respects so instructive that I will venture to speak to you about it, although not under my own care.

Richard Webb, æt. 54, admitted April 28th, with a scirrhus ulcer of the right leg. The ulcer is situated a little below the middle of the leg, over the tibia, irregular on its surface, with several prominent warty granulations; a probe passes freely into a cavity in the bone, which is felt exposed, and in one part destroyed entirely through its substance, so that the probe passes to the soft parts posterior to the tibia. The integuments for some distance around the ulcer are slightly thickened, indurated, and of a purple colour. At present there is not much discharge from the ulcer, but when it becomes profuse it is very offensive. Suffers much from sharp shooting pains up the thigh; a gland in the groin is very slightly enlarged and hardened; superficial veins of leg and foot enlarged. States that twenty-one months ago he struck the part (which is now ulcerated) with a hook: the skin was not broken, and he continued to exercise the limb: it soon became inflamed, and extremely painful, and he expresses having felt a "knot growing from the bone." It continued to increase, and six months after the accident it became ulcerated. Since the ulceration has commenced several spiculæ of bone have come away. Ten weeks ago caustic was applied to it, which impeded its growth, but ineffectually. He is unable to stand. The foot is much benumbed. The tibia appears enlarged in the neighbourhood of the ulcer, and rather irregular to within a short distance of the head. Tongue coated. Pulse quiet. General health good. States that forty-three years ago he had an ulcer on

the same spot from the kick of a horse, which remained open for two years, after which he remained quite well till the blow on the leg.

May 6th.—The leg was amputated above the knee.

Now in this case the cancerous nature of the surface was evident, and with so much disease of the bone of any kind added to the cancer, amputation was necessary, and with the experience of Ward's case just before, it seemed probable to all of us that the bone in this man also was affected with cancer; but then amputation above the knee is much more dangerous than below it, and it was an important question to determine whether there was room to perform the operation with safety against a return of the disease, if the bone itself was cut across below the knee. Our notes remark that the bone was enlarged and irregular in shape and feeling much above the opening into its interior, and it seemed therefore probable to Mr. Keate and myself that the malignant disease was very likely to extend high within the cancelli: Mr. Babington was less afraid, I believe. Nevertheless you may perceive that in fact the operation might have been done below the knee, as the disease in the interior was in all probability only inflammation ending in abscess and necrosis, with enlargement of the shaft; and the cancerous disease was confined, as it would seem, to the cutaneous texture, where the warty and fungous appearance and its vascularity are as well seen now as when the blood circulated in it. The disease, in fact, is only a little further extension of healthy inflammation below the cancerous ulcer, like that which, in the person from whom I removed this limb by amputation, had only caused the deposit of an increased quantity of osseous structure. It is indeed doubtful, when all the man's history is considered, whether the abscess and necrosis did not precede the cancerous action, and whether this did not come on in the skin, after the abscess in the bone ulcerated, the malignity remaining throughout confined to the skin. The disease is in fact, in either supposition, a mixed case: and in another patient, Porter, now in the hospital, with malignant disease of the ankle, you may see that in a cancerous disease beginning below the skin, the ulcer formed in the skin may assume a warty appearance, like that which originates in the cutis.

But now let us return to my own case, that of Ward, and see in our notes and in the preparations what extensive disease may be caused in the bone by cancerous ulceration manifestly commencing in the cicatrix in the skin.

The longitudinal section of the leg shews you how much the skin and subjacent textures are converted into the usual hard

fibrous structure of cancer, but you may perceive also at the part where the bone was flexible, that the anterior part of the bone is destroyed, and that the whole cancellous structure is changed into a dense white lardaceous substance, which may be cut with a knife, a few spiculæ only of osseous matter being found in it. The whole head of the bone is changed into this new growth, leaving the cartilage however in its normal condition, and there is no sign of disease in the articulation. You may perceive that the cancelli, as low as where the bone was sawn across about the middle of the leg, have undergone this alteration into a cancerous tumor; in fact, although several inches were allowed below the part where the bone was flexible, yet even lower than this the bone has become diseased, leaving only a few inches from the ankle free from it. In this transverse slice of the lower end of the bone the change is very well seen, as one portion (its posterior surface) is still composed of healthy osseous tissue, while the other two surfaces of the outer shell, and the greater part of the cancelli, are lardaceous and white, and softer than usual, looking like what you may see in a tumor unconnected with bone.

The appearance of the new growth is not however like brain; it is not like a medullary or hæmatoid variety of malignant structure, but is much more like the scirrhus form of carcinoma, using this term in its genuine sense.

In this next preparation cancerous disease of the tibia has gone to a further stage in a man whose leg was amputated by Mr. Walker, and you may perceive that besides the infiltration of the cancelli with cancerous matter, and the absorption of the old bone to make way for the new deposit, there has been extensive destruction in several parts by deep ulceration, forming excavations in the interior of the bone, in a much more marked degree than in Ward. There was some doubt whether the disease might not have originated in the bone, and ulcerated outwards into an old cicatrix, the disease then presenting the usual warty growth and fungous appearance of cancer of cicatrices; but if it were so, it equally shews that cancer of a cicatrix is possessed of certain characters, in whatever way it begins, and that both the skin and the bone may be extensively destroyed by ulceration. The patient died of secondary abscesses, I believe, without morbid deposits of cancerous kind in the rest of the body.

Secondly, with regard to the absorption of the poison of the cancerous ulcers of cicatrices. When this patient, Ward, was unwilling to submit to amputation, you will remember that we had reason to fear that the glands in the groin were becoming contaminated, but our apprehensions were

probably excited by simple irritation and enlargement, without malignant infection.

The correspondence between cancer of cicatrices and that of healthy skin, as to its effects on the glands, had not been witnessed by me when my paper was written, but I have since then had an opportunity of seeing that absorption does take place, and the case is an example at once of this fact, and of the mildness of the disease, on account of the length of time it had existed before the glands were affected. It happened in one of the patients whose cases were detailed in the paper, a man of the name of Callcott, who was operated on by Sir B. Brodie in 1828, the disease appearing to be connected with the tibia, which had been injured twenty-seven years previously, and having then existed fourteen months. A portion of bone was removed by the trephine along with the cancerous fungus, but it was found to be only vascular and inflamed, and not changed into cancer, as in Ward. The limb continued quite well for nine years, and then broke out in the old cicatrix in the early part of 1837, and he was admitted into the hospital in the following December, under my care, when 60 years of age, with a cancerous fungus of about three inches and a half in diameter, attended with much pain. Amputation was proposed to him, as the bone was felt at the bottom of the fungus, but he preferred trying the effect of caustic first, and chloride of zinc was applied to half of the sore. This was followed by inflammation of the absorbents and cellular texture, and much disturbance of the health, and some weeks afterwards I amputated the leg below the knee. The bone even now was only superficially affected, but on the man's death, from inflammation of the veins, I found several of the glands in the groin enlarged with hard firm white substance, with some of the yellowish deposit in it, which is often found in cancerous glands, and which left no doubt in my mind of their being contaminated by the disease of the leg below them; one of them was as large as a walnut, the others were smaller.

This is, however, the only instance I have seen of the absorbent glands being affected, out of perhaps some twenty-five cases of the disease, of which I have either notes or a distinct recollection; and therefore surgical operation for the cure of this kind of cancer can be undertaken with tolerable confidence. The occurrence shews the similarity of the cancer of cicatrices, and of the skin; but at the same time it demonstrates their dissimilarity in degree, as infection in the common cancer of the skin, especially when of such long standing, is undoubtedly in a greater proportion of cases than this. How many glands may be affected by common cancer of the skin, you have seen in Mr. Tatum's case,

recently operated on, many of the glands of the neck being in that case enlarged, besides some tubercles in the skin, separate from this fungous ulcer which was removed.

Thirdly, as to the general state of the system in cancer of cicatrices. There was no disease found in any part of the body in Callcott, nor in another patient whom I carefully examined after death, with the very large and fatal ulcer which I before mentioned to you; nor, I believe, in Mr. Walker's patient, to whom this limb belonged; nor was there any evidence of local disease, nor of cancerous deterioration of the health, in any of the other patients whom I have operated on, or seen under other surgeons' care. It is probable, therefore, considering the number of cases I have seen, that such an occurrence is extremely rare, and particularly as, even in the more virulent cancerous ulcerations of common skin, the system in very many cases appears not to be contaminated by the local disease. We lost the opportunity, in Mr. Tatum's case, of seeing whether there was any cancerous deposit in the viscera, as, owing to some cause or other, only the head was examined; but I understood there were no symptoms of disease before his death, except in the head, where no morbid deposit was found.

Thus, then, if cancer of cicatrices is so mild in its effects as to be nearly always local—if the glands are only affected in some rare case, and the system still more rarely becomes contaminated, although the disease has extended deeply and largely even into the cancelli of the bones—a cure may reasonably be anticipated in almost every case. But then, again, as the disease is not merely an ulcer of unhealthy character, but is essentially a morbid deposit, capable of spreading into the adjacent parts, nothing short of the entire removal or destruction of every portion of the new growth can produce this cure.

There are but two methods of effecting this object, namely, the use of caustics, or of the knife of the surgeon. Each of these may be applicable in some cases; or where both may be employed effectually, the patient may sometimes have his choice of the two plans. For small portions of the disease, or superficial ulcers, caustic may be employed as safely as the knife, although perhaps with more pain to the patient, whose fears often lead him to choose the caustic in preference to the other mode. If the thickness is great, the knife must be much less painful, and probably safer, than the use of caustic.

As a general rule, I think caustic is less likely to be followed by inflammation of the cellular tissue, with risk of absorption of

matter, and the formation of secondary abscesses, than an operation by the knife. But then, on the other hand, I think you will much more frequently have inflammation of the absorbent vessels, and erysipelatous inflammation of the skin, after the use of strong caustics, than when a cutting instrument is used; so that I scarcely know which is really attended with more risk to the patient from these accidents. When you employ caustic, it must be one active enough to destroy the whole thickness of the disease; so that the actual cautery, or lunar caustic, are useless; even nitric acid will often extend with difficulty through the new structure. Kali purum, or the chlorides of zinc or antimony, may be employed without the risk which attends the use of arsenic; but I think the best of all is the chloride of zinc, which may be proportioned, with a little experience, to the exact thickness of the part to be destroyed in different cases. Sometimes, then, the tumor or ulcer may thus be taken away completely; sometimes, when a bone is exposed, and rough or carious, a chisel will scrape off the surface, without, in its inflamed state, causing the exposed layer to exfoliate; or, if it is too hard for this, the surface of the bone may be touched by nitric acid, when the bleeding has ceased after the operation, exfoliation of course then taking place. Sometimes, again, the disease is too extensive, or too deep, to admit of removal without amputation of the limb in which it is situated.

SENECIO JACOBÆA

IN GONORRHŒA, AND A METHOD OF MAKING
BALSAM OF COPAIBA INTO PILLS.

To the Editor of the Medical Gazette.

SIR,

You did me the favour on the 23d ult. to call the attention of the profession to the *Senecio jacobœa*, which I have reason to believe possesses properties which make it a useful remedy in gonorrhœa. Several gentlemen have kindly responded to my request, and are making experiments with it. From some of them I have already received most hopeful accounts, and my own experience seems to warrant an increased confidence in its usefulness. Still I should not be justified in speaking too confidently upon the result of a few cases. I wish to collect the history of many, from various practitioners; and I pledge myself to tender a faithful account of all I receive properly authenticated. It is desirable to know in what stage of the disease it may be useful, and what effects, direct or indirect (if any), it exerts on the system.

In this stage of the inquiry, I sincerely hope that the letter of Mr. Bentley (chemist) will not prevent any one from giving the remedy a fair and impartial trial. I shall, notwithstanding his opinion, take leave to consider the *Senecio jacobcea* to be a new remedy for gonorrhœa, until it is pointed out to me where, or by whom, any trial of it in this disease is recorded. I trust that my letter conveyed to no other reader the impression that I claimed for it to be altogether new to the *materia medica*. In common with every indigenous plant, so conspicuous in our hedge rows, all sorts of properties have been attributed to it by the older herbalists. The latest account of this plant which I have seen is in the *Dictionnaire Universel de Matière Médicale*, par F. V. Mérat and A. J. De Lens. 1834. Bailliére.

“Ce végétal vivace est réputé émollient, résolutif, apéritif, expectorant, détersif, et surtout vulnérable. Sa decoction a été conseillée dans l’angine, l’inflammation des amygdales, la dysenterie, &c. On l’a appliqué en cataplasmes sur le bas-ventre dans les douleurs de cette région, sur les contusions, les ulcérés sordides, &c. On n’en fait aujourd’hui nul usage dans la médecine urbaine ; sans doute la villageoise, si riche en prescriptions végétales, l’emploie encore, mais probablement sans beaucoup d’efficacité.”

To another plant of the same family—the *Senecio vulgaris* (groundsel)—the power of acting as an emetic, when its bruised leaves are applied to the pit of the stomach, is attributed by Mr. Stedman (*Edin. Med. Essays*, vol. ii.); and it has found a place, probably from this property, in the *Pharmacopée Universelle*.

But the question is, who has used this species (*jacobcea*) in this disease? I shall be most happy to receive from the practitioners alluded to by Mr. Bentley an account of their experiments, and will give due weight to their testimony. In the meantime, I would remark, that, in my opinion, the juice of the plant deserves no confidence, as every one knows that the active principles of herbs exist in the expressed juices in very variable proportions; and, besides, it is necessary to add to them some foreign substances, alcohol, &c., to prevent fermentation, which may seriously impair, and, in some cases, entirely destroy, their therapeutic virtues. For these reasons, probably, the liquid juices have been long banished from the British pharmacopœias.

Perhaps, as Mr. Bentley’s extract of the *Senecio jacobcea* was made “some time past,” its efficacy may be diminished, as the results, he says, were not positively against its use in gonorrhœa, but merely “doubtful.”

We are giving and recommending the extract made by Mr. Bullock, in doses of

15 grains, three times a day, in the first stages of the disease; and I ought to correct any assertion of its nauseousness, which applies to the decoction, as patients can take the extract mixed in water.

Mr. Winstanley, of the Poultry, also, has prepared the extract this season; and I do not doubt the extent of its usefulness (for that it is useful in the disease I cannot entertain a doubt) will be shortly settled, for the herbalists in Covent Garden have, since the appearance of my letter, had a great demand for the plant.

The entertaining authors of the “Rejected Addresses,” in their imitation of Cobbett, make him say of some extraordinary event, “I prophesied that, *only* I never told any body.” So, whatever is advanced in physic, some one starts up with—“I knew that before!!” I do heartily wish that every practitioner who knows any real truth relative to diseases and remedies which is not recorded would hasten to make it known. It is surely *an honour* to advance our science, if it be only an hair’s-breadth, and

“*Palmarum qui meruit ferat.*”

Might I suggest that a small corner of the *GAZETTE* allotted to negative information would convey useful information, as, for example, thus:—

“I tried the common black pitch in six cases of gonorrhœa, in doses of half a drachm to a drachm, three times a day, and could not see that it produced any effect. I had seen, somewhere, that it possessed the power of affecting all mucous membranes. — JOHN GARDNER.”

By adding to such short statements the authority, an opinion may be formed by subsequent inquirers of the degree of weight due to them.

While on the subject of gonorrhœa, I should like to communicate some useful fact, to repay the readers of this long letter. There is one which, although known to some chemists, is not known to many practitioners: it is but a trifle, and there are those who, with myself, are not despisers of trifles.

How to make Copaiba into Pills.

Take—Balsam of Copaiba, 5 parts; Bicarbonate of Magnesia, 3 parts. Rub them together, and a mass is made exactly adapted for pills.

This affords a very convenient means of administering this drug; the pills agree with the stomach better than the gelatinous capsules, or any other form that I am acquainted with (and they are not more than one-eighth to one-tenth of their price), probably from the minute subdivision of the balsam, the efficacy of which is not at all impaired. Dr. A. T. Thomson has published a method of making copaiba into pills with calcined magnesia,

which is tedious, and not so effectual as the above.—I am, sir,

Your obedient servant,

JOHN GARDNER.

49, Great Portland Street,

August 11, 1841.

MEDICAL ASSOCIATION OF IRELAND.

PROCEEDINGS OF COUNCIL.

Report of the Deputies appointed to attend the anniversary meeting of the Provincial Medical and Surgical Association.

PURSUANT to a resolution passed by the council on the 22d ult., we attended the anniversary meeting of the Provincial Medical and Surgical Association held at York on the 3d, 4th, and 5th inst.; and, as a deputation from this Association, were received by our English brethren with the utmost kindness and courtesy. There were also present at the meeting deputations from the North of England, and from the British Medical Associations, in conjunction with whom we took part in the proceedings of the meeting held on the 4th inst. On that day a report upon the subject of medical reform (published at length in the Medical Press of the 11th inst.) was read and adopted with the general concurrence of the deputations. It appeared to us, however, as well as to the gentlemen representing the North of England, and the British Medical Associations, that the grievances of the profession required something more than the mere enunciation of the abstract truths contained in that able document, and that, for practical purposes, it was expedient that an efficient plan of communication, through official and responsible channels, should be established between the several associations, and that, thereby, an active co-operation should be set on foot, for the purpose of attending, not merely to the subject commonly known as medical reform, but also to the redress of all practical grievances which now oppress the medical practitioner. We accordingly laid our views on the matter before the Association, and, without making a specific proposition, endeavoured to point out the advantages that would result from the appointment of a committee invested with powers to co-operate, efficiently, with other bodies, for the protection of general professional interests. We were the more anxious for the appointment of a committee, specially charged with such duties, because we were aware, from the experience of the past year, that it was the opinion of Dr. Barlow, the respected chairman of the committee appointed by the Provincial Association to watch over the interests of the profession, that that committee had no power except to report annually; and that the power of acting lay with the council, which,

being composed of more than 300 members, was, of course, unsuited to discharge executive functions of any kind. We were ably supported by the other deputies, and have every reason to infer, as the result of private conversations with the members of the association, that the general feeling was in favour of a system of sustained action in support of the interests of the profession, whereby the influence and funds of the association might be made actively useful; rather than of the delivering of an annual oration on medical reform. We regret to say, however, that the secretary, Dr. Hastings, and Dr. Barlow, chairman of the reform committee, thought differently, and distinctly informed us that they did not wish any proposition for the appointment of a responsible committee to be brought publicly forward. Under these circumstances, the deputies present conferred together, and unanimously agreed to the following resolutions:—

York, August 5, 1841.

“At a conference of the delegates of the British Medical, the North of England, and the Irish Medical Associations, to the Provincial Medical Association, at its anniversary meeting, it was agreed that the delegates should report to their constituents respectively, that after having attended and taken a part in the proceedings at the general meeting of that body on the previous day, and having given their best consideration to the subject, they deemed it inadvisable to make any specific proposition for the purpose of procuring its co-operation through the medium of some official channel appointed for that purpose. Further, the delegates were informed by the secretary that it would not, in his opinion, be advisable that such a proposition should be made at the present meeting. The secretary and the chairman of the reform committee of the association also stated, that a free intercourse had hitherto existed between the reform committee of the Provincial Association and the associations represented by this conference of delegates, which, in their opinion, would be sufficient at the present time.

As it appeared essential to the best interests of the medical profession to organise a plan of co-operation between the different associations founded for medical reform, it was unanimously agreed, that the fullest and freest intercourse between their councils be recommended for the purpose of promoting measures for the good government and regulation of the medical body.”

A copy of these resolutions, bearing the signatures of Dr. Brown of Sunderland, Dr. Webster of Dulwich, and Mr. Bottomley of Croydon, we now beg leave to lay before the Council.

ROBERT C. WILLIAMS.

H. MAUNSELL.

REMARKABLE CASES OF HERNIA.

By M. DEMAUX.

At the meetings of the Anatomical Society of Paris, M. D. lately presented two remarkable preparations. The first was from a man advanced in life, of whose history nothing was known. M. D., in carefully dissecting a hernia which was situated at the right inguinal ring, and was as large as a moderately sized orange, met with some muscular fibres, which at first he knew not what to refer to, for the sac had not yet been opened. He soon found, however, that they were those of an intestine, and he then opened the abdomen, and found that the cæcum had slipped downwards, so that its posterior wall had come in contact with the orifice of the inguinal canal, through which it had formed a hernia. It was this posterior wall which presented externally. Within, a similar portion of the anterior wall formed a pouch, which was covered by peritoneum, and constituted a sac, into which a loop of small intestine had passed. On blowing into the lower extremity of the latter, the protruded portion of the anterior wall of the cæcum suddenly re-entered the abdomen, and with it the fold of small intestine which it contained. To have arrived at the latter in an operation, it would have been necessary to cut through, 1st, the skin and fasciæ, 2d, the posterior wall of the cæcum, 3d, the anterior wall and the peritoneum.

In the second case the patient was a man 56 years old, who had long had an inguinal hernia on the left side. For some time he wore a truss; but the rupture having ceased to come down he had left it off, and then, after long walking, the hernia reappeared, and at last could not be reduced. The ordinary operation was performed, and it was scarcely necessary to divide the ring: a mass of omentum could not be returned on account of adhesions, but a small loop of intestine which lay behind it was replaced without difficulty. In three days the patient died of peritonitis. The orifice of the inguinal canal was found to present rather a large infundibulum; but it soon divided into two branches. The one which was anterior and internal extended down to the testicle, and still contained the mass of omentum: this was, therefore, a congenital hernia, and in this the strangulation had taken place: the other, posterior and external, though also very deep, contained nothing; its orifice was larger than that of the anterior sac, and would have admitted the finger.—*L'Examineur Médical*, Juillet 11.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, August 19, 1841.
J. Highett, London.—J. Medlicott, Bishop's Castle.—T. Clarke, Doncaster.—C. Goddard, Norwich.—R. Hicks, Sturmer, Essex.—J. Dickson, Ronaldkirk, Yorkshire.—C. L. Allwork, Maidstone, Kent.—T. B. Johnson, Newcastle-on-Tyne.—P. B. Giles, Hope Court, Ludlow.

A TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 14th Aug. 1841.

Small Pox	10
Measles	19
Scarlatina	7
Hooping Cough	41
Croup	5
Thrush	10
Diarrhœa	16
Dysentery	0
Cholera	1
Influenza	1
Typhus	19
Erysipelas	5
Syphilis	2
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	154
Diseases of the Lungs, and other Organs of Respiration.....	233
Diseases of the Heart and Blood-vessels	20
Diseases of the Stomach, Liver, and other Organs of Digestion	64
Diseases of the Kidneys, &c.....	7
Childbed	7
Ovarian Dropsy	0
Diseases of Uterus, &c.....	4
Rheumatism	1
Diseases of Joints, &c.....	2
Ulcer	0
Fistula	0
Diseases of Skin, &c	1
Diseases of Uncertain Seat	102
Old Age or Natural Decay.....	42
Deaths by Violence, Privation, or Intemperance	27
Causes not specified	2
Deaths from all Causes	802

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N. Longitude 6° 3' 51" W. of Greenwich.

August.	THERMOMETER.	BAROMETER.
Wednesday 18	from 56 to 70	30.06 to 30.14
Thursday . 19	47 72	30.14 30.09
Friday . . 20	53 74	29.93 29.73
Saturday . 21	52 70	29.62 29.77
Sunday . . 22	49 65	29.84 29.86
Monday . . 23	52 64.5	29.78 29.85
Tuesday . 24	46 63	29.96 30.11

Prevailing wind, S.W.
On the 18th, morning overcast, with rain; otherwise clear. The 19th and following day clear. The 21st, morning cloudy, with rain; afternoon clear. The 22d, morning cloudy, with rain; afternoon clear; evening overcast. The 23d, morning overcast, with heavy rain; otherwise generally clear. The 24th, generally clear; a shower of rain between 12 and 1 P.M.
Rain fallen, .435 of an inch.

CHARLES HENRY ADAMS.
WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, SEPTEMBER 3, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XLVIII.

Catarrh; its varieties. Acute Bronchitis. Dry sounds attending the respiration; rhonchus, and sibilus: moist sounds; large and small crepitation: how these are produced, and what they denote. Treatment of acute bronchitis. Peripneumonia notha. Sudden infarction of a large bronchus.

IN the last lecture I described to you, in a general manner, the method of auscultation; or the employment of the sense of hearing in the investigation of disease; and particularly of *thoracic* disease. I explained, as well as I could, the different sounds which result from percussing various parts of the chest; and from the entrance and exit of the air during the several acts of breathing, speaking, and coughing. I mentioned certain conditions in which those natural sounds are abolished: but I did *not* speak, except incidentally and very cursorily, of the altered and new sounds to which the presence of disease within the thorax has been found to give rise. Nor do I propose to enter now upon any *formal* account of these morbid sounds. I shall endeavour to make you acquainted with their characters, and causes, and signification, as they arise in the progress of the separate thoracic diseases which I am about to consider.

Before you can possibly appreciate the *morbid* sounds, you must make yourselves familiar with the *natural and healthy*. You must have a standard, by which you may

measure the numerous deviations from the natural sound, that will reach your ear in disease. Listen to the voice and the breathing of healthy children—or of some of your friends and fellow-students—and you will soon recognize those respiratory sounds which accompany the perfect state and working of the breath-machine.

I intend to commence with those diseases of the lungs in which the *mucous membrane* lining the air passages is primarily or principally involved. This membrane is often affected *alone*. It is much exposed to known *causes* of disease: to alternations of temperature in the air which is constantly passing over it; to the irritation produced by acrid or noxious matters, solid or gaseous, which are mixed and inhaled *with* the air. And when disease commences in *other* parts of the lungs, it seldom fails to reach, sooner or later, the *mucous membrane*. In diseases also of the *heart* the same mucous membrane is very liable to be consecutively affected, by reason of those alterations in the condition of its capillary vessels which the disorder of the circulation produces.

Now I shall first point out the changes to which this mucous membrane is liable; and then describe the modifications of the natural sounds that result from such changes; and lastly, consider the different forms of disease which these changes constitute, and these altered sounds denote.

In a former part of the course I gave you some account of the peculiarities which differences of texture impress upon the phenomena and process of inflammation: and among the rest, I spoke of the behaviour of the *mucous* tissue when inflamed.

Catarrh.—The mucous membranes, in the state of health, are perpetually *moist*. The exhalation of this moisture, to a certain amount, and *not beyond* a certain amount, constitutes an essential part of their healthy functions. Now their *inflammation* (for I am about to consider

first the inflammatory affections of the membrane of the air passages ; some of them indeed I *have already* discussed), I say the inflammation of these mucous surfaces alters their ordinary secretion. An inflamed mucous membrane is in the first instance *dry* ; its secretion is suspended. But this is not the only change that takes place in it ; it becomes tumid also, swollen, thicker than before : it is redder than natural : and its sensibility undergoes a perceptible modification. *Pain*, in mucous membranes, is *not* a common phenomenon : for their texture enables them to expand or dilate freely, so that they escape much tension, and the pain which is produced by tension : but their natural sensations are blunted, and new and uneasy sensations arise in them : sensations of heat, fulness, itching. It happens that we can *see* a portion of the mucous membrane that belongs to the air passages : and by noticing the changes produced in it by inflammation, we infer those which are apt to take place in the parts we *cannot* see. We have all often experienced in our own persons an inflammatory state of the membrane lining the nasal cavities ; the schneiderian membrane. At first the nostril is preternaturally dry : yet though it is dry, you cannot breathe through it : it is stuffed up ; not with accumulated mucus, but by the mere swelling of the membrane : the sense of smell is perverted or lost ; the part is evidently red ; it is tender also and irritable ; the contact of atmospheric air a little colder or a little less pure than common, provokes sneezing. The affection extends often into the frontal sinuses ; and headache and oppression ensue : or it passes into and through the lachrymal sac, the conjunctiva participates in the inflammation, the puncta lacrymalia become impervious, and the tears flow over the cheek. And with all this there are sometimes shivering or chilliness ; and the pulse, especially in the evening, becomes a little more frequent than common. There is slight fever. After the unusual dryness, the membrane begins to secrete a thin serous fluid, having acrid properties ; for it reddens and frets the *alæ nasi* and upper lip over which it flows. By degrees, this thin serous fluid becomes thicker, and as it becomes thicker it becomes less irritating also, more viscid, opaque, and yellow : the swelling of the membrane diminishes ; it is less raw and sensitive : at length the secretion resumes its natural *quality*, and is reduced to its natural *quantity* again ; and the tumefaction of the membrane entirely disappears. This is the course of what is popularly called a *cold in the head*. When the defluxion from the nasal membrane is considerable, systematic writers call the complaint *coryza* ; when it is attended with much pain and weight about

the *frontal sinuses*, it is named *gravedo*. It is a variety of *catarrh*. In *catarrh*, sometimes one part, and sometimes the whole, of the mucous membrane of the air passages suffers inflammation. If the disorder goes down into the lungs, it is said to be a *cold in the chest* ; or, from one of the most prominent of its symptoms, a *cough* : in medical language, *bronchitis*. It sometimes travels from one part of the membrane to another. Beginning, for example, in the nose, it gradually creeps down into the windpipe and lungs. Sometimes the inflammatory condition passes from the throat into the eustachian tubes, and produces deafness ; or down the gullet and to the stomach, causing qualmish or other uneasy sensations, and a loss of appetite. And occasionally this order appears to be reversed. There are some persons who will tell you that whenever any thing disagrees with their stomach, whenever dyspepsia is produced by some error in diet, they are sure to have *catarrh*.

Now I have adverted to this *cold in the head*, or *coryza*, because the phenomena which are open to our inspection in the schneiderian membrane take place also, no doubt, in the bronchial. The membrane is first dry, and tumid, and irritable ; the uneasy sensations of which it is the seat prompt to the action of coughing. The chest feels tight, stuffed, constricted. There is some hoarseness, and a sense of roughness and soreness in the windpipe ; and a dry cough, which seems to arise from some irritation about the glottis. Sometimes, with these symptoms, pains in the limbs, like the pains of rheumatism, occur ; the appetite is impaired ; the patient is thirsty ; and a general lassitude is felt all over the body.

But what effect has the altered state of the membrane upon the sounds elicited by percussion ; or heard within the chest, by the ear, during respiration ? Why it brings us acquainted at once with two remarkable modifications of the natural sound of breathing ; and these I must describe and explain.

I will take this opportunity of again recommending you to read and study the little book of lectures recently published by Dr. Latham. It contains a very plain and clear account of the auscultatory signs of disease within the chest ; and he speaks of these signs in more easy and popular language, with less of over refinement, and a less subservient adoption of the French mode of thinking and writing on these subjects, than any other English author that I know of. I recommend his little volume the rather also, because he uses the nomenclature which is the most familiar to myself : in fact, as we some years ago saw and talked of these matters together in the wards of St. Bartholomew's, we were likely to employ the same terms.

When you listen, I repeat, to the breathing of a healthy person, you hear, as the breath goes in and out, but especially as it goes in, a smooth and gentle rustle—the *respiratory murmur*, or the *vesicular breathing*. But when the inner surface of the bronchial tubes, and of their ramifications, is preternaturally dry, and tumid, this sound is altered: you hear a hissing, or wheezing, or whistling, as the breath goes in and out; and this is technically called *sibilus*: or you hear a deeper note, a snoring noise, as the patient inspires or expires—a sound like the cooing of a pigeon, or the bass note of a violin, or the droning hum of an insect in its flight; and this is called *rhonchus*. These two, in their various modifications, constitute the *dry* sounds of respiration; and it will be worth while, once for all, to reflect upon their cause and nature, and the manner in which they are combined, and what they denote. You are aware that when air is propelled through a cylindrical tube of a certain size, and when that tube is narrowed in a particular way at one or more points, a musical note is produced. Now this is what often happens in the larger bronchi; this is what *always* happens in them when *rhonchus* is present. Rhonchus belongs to the larger divisions of the bronchi exclusively; and as these are often, for a time, exclusively affected, so rhonchus may exist *alone*. It will be grave or deep in proportion to the length and diameter of the tube in which it is produced. When the sound is grave and deep, the hand placed upon the chest may frequently perceive a trembling or thrill communicated to its parietes. I believe that rhonchus is mostly occasioned by portions of viscid half-solid mucus, which adhere to the membrane, and cause a virtual constriction of the air-tubes, and act as vibrating tongues while the air passes by them. I conclude such to be the case, because it seldom happens that the rhonchus cannot be got rid of by a vigorous cough. It will soon begin again, perhaps, or it will commence in some other part, but the effort of coughing, which detaches and removes the adhering tough mucus, dislodges also, for the time, the rhonchus. Yet, rhonchus in a given spot may be permanent: a tumor, or a tubercle, may flatten one of the air-tubes, and convert it into a musical instrument. For the most part, you will find what I have told you holds true: you may suspend the rhonchus by getting the patient to make a hearty cough. Now in the natural state of the chest, we do not, except in particular spots, hear the transit of the air through the larger bronchi. Whatever sound it makes is damped by the spongy lung, or covered by the vesicular breathing. But rhonchus, in its turn, may overpower the vesicular

murmur, and render it inaudible. It does not prevent it, but it outroars it, as it were. Yet this is seldom the case: you hear the rhonchus, and, if you listen attentively, you may in general hear, mingling with it, the vesicular murmur also. Recollect, then, that rhonchus belongs to the larger divisions of the air-tubes; that it denotes their partial narrowing; that it is a dry sound; and that the condition of which it is expressive implies usually no danger: there is no material obstacle to the passage of the air through these larger tubes to the vesicular structure beyond them.

I must further admonish you, that in your earlier essays in auscultation you will be apt to deceive yourselves in respect to the exact place in the lung in which the rhonchus which you hear is produced. It is so loud a sound, that when it proceeds from a single bronchial tube it may be plainly audible over the whole of that side of the chest; and sometimes, more obscurely, over the other side too.

When air is driven with a certain degree of velocity through a small pipe, it gives rise to a hissing noise. It is by forcing air through a cylinder perforated by a slender tube, that Professor Wheatstone obtains the sound of the letter S in the talking machine which he has constructed, after Kempelin's model. Precisely this condition we have in the smaller bronchial ramifications, when the inflammation in catarrh or bronchitis has reached *them*, and rendered the membrane lining *them* tumid. And sibilus is the result of this change. Now sibilus, like rhonchus, may exist alone; and, inasmuch as the sibilus proceeds from the smaller air-tubes, adjacent to the pulmonary vesicles, it *abolishes* the natural respiratory murmur. It does not, like simple rhonchus, merely drown it, but it takes its place. If you do hear the respiratory murmur mingling with sibilus, you may be sure that some of the lesser air-tubes are narrowed, and some free: you cannot have both sounds at once from the same ramifications of the bronchi. Sibilus is a sound of more serious import, therefore, than rhonchus; it bespeaks a condition of greater danger. It belongs to the smaller air-tubes and vesicles, and denotes that they are in the first stage of inflammation, which has diminished their natural calibre, by rendering the membrane tumid. It is a dry sound, but you cannot cough it away.

I say rhonchus may occur alone, and sibilus may occur alone; but very often indeed they both occur together; and may be heard in various parts in different degrees: causing a strange medley of groaning, and cooing, and chirping, and whistling, and hissing, mixed, it may be, here and there, with the natural respiratory murmur.

When you hear sibilus over the entire surface of the chest, the mucous membrane is universally affected, and the case is a severe one, and attended with considerable hazard.

It is just possible that a sibilant sound may proceed from a large air-tube, when its bore has been narrowed to a very minute slit or orifice; but this possibility does not interfere with the general distinctions that I have been endeavouring to point out.

Now in these cases we neither obtain nor require any information from percussion, except of a negative kind. Supposing the inflammation confined to the mucous membrane, the resonance on percussion will not be sensibly diminished; the lung is every where spongy still, and air reaches every part of it, though not with the usual freedom.

There is one exception to this. Occasionally, though rarely, a piece of tough phlegm may seal up, as it were, the very entrance of one of the principal bronchial tubes, and so prevent the air from passing to or from the portion of lung to which that tube conducts. When this happens, it is very likely to puzzle the auscultator for a time. There is air in the sealed-up portion of lung, therefore percussion gives a natural sound; but the air is at rest, therefore no sound of respiration is audible. An effort of coughing unstops, perhaps, the bronchial tube; and then the air is again heard to enter and to depart from that portion of lung. I shall advert to this sort of accident again.

Finally, I may remark, that these dry sounds, rhonchus and sibilus, are heard during the *breathing*; they have no relation to the voice or to the cough.

After a while, the inflamed membrane begins again to pour out fluid; but it is not the thin, bland, moderate exhalation of health; it is a glairy, saltish, transparent liquid, like white of egg somewhat; and if it be expectorated only after much coughing, it will be frothy also, *i. e.* it will contain many bubbles of air entangled in it. It is a stringy tenacious fluid, and the more so in proportion to the intensity of the inflammation. With this new condition of the membrane, we have new sounds—sounds which result from the passage of air through a liquid; sounds which are occasioned by the formation and bursting, in rapid succession, of numerous little air-bubbles. These sounds are called *crepitations*. This process may take place in the larger air-tubes, or it may take place in the smaller, or in both. In the larger tubes the *bubbles* will be larger, and the ear can readily distinguish this; we have *large crepitation*. In the smaller air-tubes we have, in the same way, *small crepitation*. There is no difference between these sounds, except in degree;

and they graduate insensibly into each other. But there is a considerable difference in the nature of the intimations which their well-marked varieties convey. If there be merely large crepitation, without any other morbid sound, it is produced in the larger tubes. Air passes, notwithstanding, into the vesicular structure *beyond* the accumulated liquid; and vesicular breathing *exists*, though perhaps it *cannot be heard*, on account of the crepitation. But the state of the patient is not a state of peril. On the other hand, small crepitation has its seat in the smaller air-tubes and cells; it supersedes the vesicular breathing, and, if extensive, it bespeaks considerable danger.

Rhonchus and large crepitation are respectively the dry and moist sounds that belong to the larger bronchi; sibilus and small crepitation the dry and moist sounds of the smaller branches. When the latter sounds are heard over a considerable part of the chest, there is, I say, usually a good deal of distress, dyspnoea, and cough; and the fever which attends the local inflammation is at its height. By and by the expectoration becomes opaque, and more consistent, and of a greenish or yellowish colour; it is brought up with more ease; the crepitation, great and small, diminishes; perhaps rhonchus reappears: but at last the parts return to their original condition; and the natural, smooth, equable rustle of the breathing is again every where audible.

These are all the morbid sounds to which active and recent inflammation of the mucous membrane of the air-passages ever gives rise: rhonchus and sibilus; large and small crepitation. Having once described their nature and causes, I need not repeat the description if we find them accompanying other diseases: but their *import* may be different. I may mention here, that as crepitation results from the passage of air amongst and through liquid, from the rupture of the little air-bubbles so produced, the *kind* of liquid may vary. If the air, in going and returning, meets with serum, or with pus, or with blood, it will occasion exactly the same bubbling noise. Hence the French term for what I have been calling crepitation, *viz. mucous rattle*, is very objectionable. From the sound itself, we cannot tell whether it proceeds from *mucus* or from some other liquid present in the air-passages; and from this objection the word crepitation, whatever exception may be taken against it on other accounts, is free.

Having thus embraced the occasion of explaining these auscultatory signs, I will now resume the history of catarrh. It implies inflammation of the mucous membrane of the air passages; and it receives different appellations, according to the district of that membrane which it chiefly plagues: *gravedo*,

in the frontal sinuses ; coryza, in the Schneiderian membrane of the nose ; bronchitis in the trachea and lungs.

Catarrh is the commonest of all disorders. Not one man in ten thousand passes a winter without having a *cold* of some sort. And this name points to its ordinary cause : cold somehow applied to the body. It does not always or often result, I apprehend, from cold air brought into contact with the membrane itself, in the process of breathing ; but from cold, and especially from cold and wet, applied to the external integument. It is unnecessary for me again to go over the circumstances under which the application of cold is most likely to prove injurious. Catarrh is usually a mild disorder, and runs its course in a few days, if abstinence be observed with respect to animal food and stimulating liquor, and if the patient remains in an equable temperature, and avoids re-exposure to the cause of his malady. I am now speaking of the milder forms of catarrh. We are not often consulted for this complaint. Every man, in regard to a cold, thinks himself qualified to be his own doctor. But if you *are* consulted, keep your patient in the house, or even in bed ; let him live upon slops ; give him a gentle aperient, and then some of those medicines which are esteemed to be diaphoretic : small doses of James's powder ; three drachms of the liquor ammoniæ acetatis, with a drachm of the spiritus ætheris nitricus, and an ounce of camphor mixture ; or a saline draught with an excess of alkali, and a few grains of nitre, or a little antimonial wine ; three or four times a day : and let him take four or five grains of Dover's powder, and put his feet and legs into a warm bath, just before he goes to bed. In this way you may conduce to his *recovery* ; and he may be simple enough to believe that you have *cured* him.

Yet I believe catarrhs *may* sometimes be *cured* ; and the natural recovery from them *may* be, *sometimes*, accelerated. If you practise the old maxim, which says, "*veniēti occurrere morbo*," you may occasionally stop a cold on the threshold, as it were, by an opiate. And to persons who are habitually troubled with slight catarrhs, this piece of practice may prove of the greatest value. A medical man who resides in this neighbourhood, and with whom I was a fellow-student, is exceedingly subject to what he calls a snivelling cold. For many years he used to bear this as he best might ; and that, to say the truth, was very ill and impatiently. On one occasion, almost by accident, he took twenty drops of laudanum just as one of his colds was beginning to torment him ; and he found that the initiatory symptoms ceased. Since that time he has constantly had recourse to the opiate under similar circumstances ; and whereas

he used formerly to be very miserable for three or four days, he now is quite well and comfortable in the course of half an hour. And this is not a solitary case. It is worth trying, if you experience the feelings of an incipient catarrh, to go to bed, and to take a beaker of hot wine negus, with a table spoonful of the syrup of poppies in it. This will not suit every person ; but if it fails on the first trial, it need not be repeated, and no great harm, beyond an increase of headache, will be done by it. I would not recommend this plan, however, to a plethoric person ; nor to any one having a tendency to inflammatory disease ; for when it does not cure, it makes the complaint worse.

There is also a period in catarrh which has gone on unchecked, when you may accelerate its departure—"speed the going guest"—by a good dinner, and an extra glass or two of wine. But this pleasant method is scarcely to be advised for persons of delicate habits ; or in whom any phthisical tendency is suspected to exist ; or who are prone to inflammation. And it is not to be tried with any one till the fever is over, and the expectoration thick and loose.

I must not omit to mention the *dry* plan of cure ; although (I confess it with some shame) I have never yet tried it either upon myself or upon others. Dr. C. J. B. Williams, who invented it I believe, has a high opinion of its efficacy. It certainly has the merit of simplicity, for it consists merely in abstinence from every kind of drink. No liquid, or next to none, is to be swallowed until the disorder is gone. The principle here concerned is that of cutting off the supply of watery materials to the blood. The wants of the system exhaust, from the circulating fluid, all that can be spared for the natural evacuations ; and there is nothing left to feed the unnatural secretion from the inflamed mucous membrane. Its capillary vessels cease to be congested ; the morbid flux is diverted, and the inflammation starved away. Such is the theory. Habitual toppers might hold the remedy to be worse than the disease ; but Dr. Williams assures us that the necessary privation is not very hard to bear : and that it achieves a cure, upon an average, in forty-eight hours. He allows, without advising, a table spoonful of tea or milk for the morning and evening meals, and a wine glass of water at bed time.

One great advantage of this plan is, that it does not require confinement to bed, or to the house. The man whose business calls him abroad, may, with appropriate clothing, pursue his customary employment, and his cure is all the while going on. In fact, exercise, inasmuch as it promotes perspiration, helps the recovery ; whereas the system of

warm drinks and diaphoretics renders the body more susceptible to atmospheric vicissitudes; and, to be effectual, implies restrictions which are oftentimes extremely inconvenient.

Dr. Williams observes, that while this dry treatment is serviceable in catarrhal bronchitis, it is *most* successful in coryza, the snivelling cold in the head. It must be put in force in the very commencement of the disorder.

You may often do much by way of *prevention*, for persons who are unusually liable to take colds. I have remarked before upon the great value of the *shower bath* for that purpose. I could mention several instances in which persons have got rid of the tendency to catch cold by the habitual adoption of this measure. It should be begun in the summer, and used tepid at first; but in a short time quite cold water may be employed; and being once begun, the practice may be continued through the winter. I stated formerly, that the effect of exposure to cold was, *ceteris paribus*, in proportion to the intensity of the duration of the *sensation* of cold that it produced. The intensity of the sensation of cold under the shower bath is considerable, but the duration of it is momentary. It operates as a prophylactic in this way: it inures the surface to a lower temperature than it is likely to be subjected to at any other part of the day. The lesser degrees of cold have then no injurious effect, unless they are long protracted. For those who cannot procure a shower bath, or who cannot bear its shock, cold sponging will be found exceedingly salutary.

Acute bronchitis.—But inflammation of the membrane lining the air-passages may be, and often is, a very acute and dangerous disorder, *i. e.* the inflammation may be both intense and extensive; it may descend into the vesicular texture, and occupy the whole surface of the membrane on one side of the chest, and then it may be a very grave disease; or it may involve the whole lining membrane of both lungs, and then it is always attended with considerable peril.

This inflammation will sometimes, when it is thus *general* over the whole membrane, linger for a considerable time in its first stage; and it may even, after so lingering, subside again without ever passing beyond the first stage. By the first stage I mean the stage of dryness. Very little notice of this modification of bronchitis has been taken by authors. Dr. Latham has given a distinct and graphic description of it, to the accuracy of which I can testify from my own experience. You will find cases of it detailed in his book. Since they were published, some striking instances of this form of the disease have occurred to myself.

One, which happened lately. I will relate by way of example. I was asked by an old pupil of the hospital to see a lady, his patient, in Gordon Square. I found her feverish and in a state of extreme dyspnoea, sitting up in bed from inability to lie down, labouring for her breath; her face turgid and rather livid, her nostrils working, her shoulders elevated; she could scarcely speak, but expressed, in what she did say, a dread of immediate suffocation. She had been in nearly the same state for a day or two. On listening at her back I could hear the air slowly wheezing and whistling into her lungs every where, and then leaving them still more slowly, with a prolonged growl, something like that of an angry cat. There was no true vesicular breathing; there was no crepitation; and there was no part into which the air did not, although with difficulty, find its way. The chest was every where resonant on percussion. There could be no doubt that the membrane throughout was tumid and dry, and in the earliest stage of inflammation. Depletion had already been employed in this case, and we had recourse to the tartar emetic. This was given in free and repeated doses, till it produced nausea and sickness. Whenever it did so, the pulse diminished in force, the face became blanched, and the breathing much easier; and the medicine was then suspended until these effects had gone off, when it was repeated in the same manner. The disease was not put a sudden stop to however, by this treatment; it was kept at bay for a day or two longer, and then a copious secretion from the membrane took place, with great relief to all the distressing symptoms. Then, of course, crepitation became universally audible. Except the debility which it left behind, the patient soon recovered of the bronchial inflammation.

But in the great majority of instances the inflammation does not thus linger in its first stage: the membrane soon begins to pour out glairy mucus; so that we do not often meet with *sibilus*, without finding at the same time, in some part of the same lung, that there is also small and large crepitation. It is of some importance to attend to the characters of the mucus that is expectorated. It is transparent and viscid. If you pour it from one vessel into another, it flows out in one mass of extreme tenacity; it will draw out sometimes like melted glass; and the degree of viscosity is a tolerably accurate measure of the degree of the existing inflammation. Upon the surface of the viscid mucus there is usually more or less froth, the *quantity* of it depending on the facility or the difficulty with which the sputa are brought up. If the patient does not expectorate till after a long fit of coughing, during which air has been

many times inspired and expired, and has thus got intimately mingled with the mucus that fills the air passages, the expectoration will contain numerous little air bubbles: will be very frothy. Sometimes also, during this stage of the complaint, the sputa are marked with streaks of blood.

While the expectoration possesses the characters I have been describing the inflammation is still intense, and the fever and dyspnoea considerable. This correspondence between the general symptoms and the matters spat up was well known to the ancients, who said that such expectoration was still *crude*. But in proportion as the inflammation approaches to resolution, the appearance and qualities of the sputa are changed: the mucus loses by degrees its transparency, is mixed with masses that are opaque, and of a yellow, white, or greenish colour: and these masses, few at first, increase more and more in number, until they constitute the whole of the sputa. Such expectoration as this is commonly accompanied by a marked remission in the different symptoms of the bronchial inflammation: it announces that the inflammation is terminating in resolution. It is such as the ancients spoke of as being *concocted* or *ripe*. However, the characters of the opaque sputa expectorated towards the end of an attack of acute bronchitis are liable to great variation.

It will often happen that the expectoration after having thus become opaque, and particoloured, will go back again to its former condition of transparency, and stickiness, and froth: and that is a very certain index of a return or increase or extension of the inflammation: so that an observance of the characters of the matter expectorated teaches us, in a certain degree, the progress of the inflammation; and consequently constitutes one point of guidance to our treatment. The nature of the expectoration forms also an important particular in the means of distinguishing bronchitis from pneumonia; as I shall farther explain when I speak of the latter disease.

I have described acute bronchitis as it appears when it terminates favourably: in such cases the inflammation generally begins to abate, some where from the fourth to the eighth day of the disease. But acute bronchitis may terminate *unfavourably*. When the inflammation is universal and intense, the fever high, and the labour of respiration great—if the symptoms do not yield to the treatment employed, or if judicious treatment has not been adopted, or has been too long delayed, signs of impending suffocation begin to shew themselves: the lips and cheeks, and tongue, assume a purplish colour; a livid paleness takes the place of the former red flush; the expression becomes more and more anxious; delirium comes on, and rapid

sinking. These indicate, you know, the circulation of blood that is in a great measure venous through the arteries; and the venous blood acts as a poison when it so circulates. Profuse, cold, clammy sweats ensue; and the patient dies of apnoea. His breathing is choked by the morbid secretion which occupies the bronchial tubes, small as well as large, and which he has not strength enough left to cough up.

Accordingly, when we examine the thorax after death so produced, we find, in the first place, that the lungs do not collapse upon the admission of the pressure of the atmosphere to their external surface. We next find the trachea, and bronchi, and their ramifications, blocked up by a frothy adhesive mucus, resembling that which during life had been expectorated: and the membrane which lines them is red and thickened.

The treatment proper for these acute and dangerous forms of bronchitis is a matter of some nicety. Blood-letting, as I formerly stated to you, has not that decided power over inflammation of the *mucous* tissues which it possesses over the adhesive inflammation which takes place in the serous membranes. If there be much fever, a hard pulse, and great oppression of the breathing, and particularly if these symptoms present themselves in a young, strong, and robust individual, we must bleed him from the arm. And you will always find blood-letting *relieve* the symptoms; even when its ultimate effect may be injurious. The patient's distress arises from his inability to supply air enough to arterialize the venous blood which is transmitted to his lungs; and by diminishing the quantity of blood sent to those organs, you will, *pro tanto*, mitigate his uneasiness. But a great part of the danger to be apprehended in the advanced periods of the disease, is that the patient may not have muscular power enough to disembarass his air-passages of the phlegm that overloads them; to draw a strong breath, and to accomplish a vigorous cough. We must not bleed therefore to syncope, and again and again, as we are often justified in doing in cases of pneumonia. Sixteen ounces will be a moderate bleeding at first for an adult, but more or less than that must be taken, and the bleeding must be repeated or not, according to the condition of the pulse: for the pulse is a better measure of the propriety of pushing the abstraction of blood, than the local symptoms.

Great relief is often obtainable by *topical* blood-letting; by cupping over the surface of the chest, or between the scapulæ. If you distinguish sibilus in one portion of the lung more than in another, take the blood rather from that part of the surface which corresponds to the place of the sibilus.

After the bowels have been cleared by a

mercurial purgative, calomel and jalap for example, you will find the tartar emetic a very valuable medicine in these acute cases of bronchitis. It should be given in such doses as will excite nausea: and if vomiting be occasioned, you may still go on with the medicine after the sickness has subsided. The depression which this substance produces is great, but it is temporary, and it is effected without expending blood. With the antimony—I mean during the same period—mercury may and ought to be given: to this combination I should be inclined to trust more than to any other internal treatment.

If symptoms of sinking and debility have begun to show themselves, it will be necessary to administer stimulating expectorants. I presume that the carbonate of ammonia, which is often extremely useful in such cases, acts as an expectorant, by giving a fillip to the muscular power. But it is supposed by some persons to exercise some specific influence upon the bronchial membrane. However this may be, five or six grains of it, given in solution every four or six hours, are often followed by free expectoration and a marked improvement.

One of the circumstances of which patients are much disposed to complain, is the violent or importunate cough; and another is, the want of sleep and of rest: indeed, the one of these is often, in a great measure, the cause of the other:—the urgency or frequency of the cough prevents the patient from sleeping. Now there is nothing so well calculated to allay cough, and to procure sleep, as opium; and you will be strongly tempted to give these patients opiates, and you will probably be encouraged to do so by the success which will follow that practice in many cases. The good effects of a full narcotic at bed-time are sometimes very striking. Patients who for previous nights have been perpetually harassed by cough, and who are worn out by the disturbance of their rest, will sleep tranquilly, and in the morning expectorate largely and freely, and declare themselves wonderfully the better for their opiate. Yet opium is a ticklish remedy in these cases. Many a patient—some within my own knowledge—labouring under general or extensive bronchitis, have been put so soundly to sleep by a dose of opium on going to bed, that they have never waked again. I believe you may receive it as a golden rule in these cases, not to give opium—I mean in a full dose, so as to force sleep—if you see any venous blood mingling in the general circulation,—if the complexion be dusky, and the lips in any degree blue. The circulation of half-arterialized blood through the brain is in itself a powerful cause of coma; and if you add the influence of an opiate, the coma

may easily be made fatal. While the cheeks and lips remain florid, and when the first violence of the disease has abated, an opiate will do capital service. It is a common practice to combine it with antimony or some other expectorant. Twenty minims of laudanum, with the same quantity of the liquor antimonii tartarizati; or a third of a grain of the acetate of morphia, with a drachm of oxymel of squills; are convenient forms.

Counter-irritation is frequently of great use, as an auxiliary measure, in the treatment of acute bronchitis. Sensible relief of the cough, and of the oppressed breathing, often follows the rising of a large blister laid across the front of the chest. When the dyspnoea is extreme, and a more speedy counter-irritant is required, you may have recourse to the mustard poultice. Inhalation of the steam of hot water is also very soothing and useful. It is one of the best expectorants I know of when it answers at all; but to some persons it proves irritating, and they derive no comfort from it.

I have been speaking of acute bronchitis, uncombined with any other pulmonary disease; and it is curious how little disposed the inflammation often seems to be to extend itself from the mucous membrane to the neighbouring tissues. The reason, doubtless, is, that this membrane is furnished with a distinct set of blood-vessels, the bronchial arteries, and veins; while the substance of the lungs is supplied by the pulmonary. We could not tell, merely by attending to the general symptoms, whether the inflammation was limited to the inner membrane or not; but by making use of the sense of hearing, we *are* able to determine this. If the inflammation should spread to the parenchymatous texture of the lungs,—*i. e.* if the bronchitis should pass into pneumonia,—this circumstance would be disclosed by physical signs, which I shall in due time describe and explain; and it would demand certain modifications of our plan of treatment.

Peripneumonia notha.—I shall have to speak of some varieties of chronic bronchitis; but there is a mixed form of pulmonary disease that requires to be noticed, in which acute or subacute inflammation engrafts itself upon changes that are chronic and abiding. Sydenham calls the disorder to which I now refer *peripneumonia notha*—bastard peripneumony. *Catarrhus senilis* is another of its names. It may be considered as chronic bronchitis, occurring in old persons, and very apt to be converted into pneumonia, or to be greatly aggravated in degree during winter, or upon any accidental exposure. This is the common complaint of persons advanced in life. I mention it here in compliance with the usual custom, and because this is as convenient a place for its introduction as any. But it would be an

error to regard it as exclusively a disease of the mucous membrane of the lungs. An habitually congested state of that membrane, marked by some shortness of breath, and some expectoration, and by the constant presence of some degree of crepitation in the lower parts of the lungs,—these are circumstances which are of daily occurrence as consequences of *disease of the heart*; and it is in persons whose habitual health is of this kind, that what is called peripneumonia notha, which implies a diffused inflammation of the pulmonary mucous membrane, with sometimes an enormous secretion from its surface, is most apt to supervene. Almost all such persons will tell you that there are periods at which they experience slight febrile attacks, and exacerbations of their complaints: they have pain in the breast or side, headache, heat, and thirst; and at these periods the cough and expectoration are always aggravated, and continue for some time to be more than commonly severe. “The disease (says Cullen, who, following Sydenham, has given a good description of its general symptoms) has often the appearance only of a more violent catarrh; and after the employment of some remedies, is entirely relieved by a free and copious expectoration. In other cases, however, the feverish and catarrhal symptoms are at first very moderate, and even slight; *but* after a few days these symptoms suddenly become considerable, and put an end to the patient’s life, when the indications of danger were before very little evident.”

The truth is (and we learn the truth by the evidence of auscultation) that in these cases *pneumonic* inflammation is often suddenly set up. There is no security, as Dr. Latham observes, that the portions of lung which yield small crepitation to-day may not be solid and impervious to-morrow. Dr. Latham is of opinion that in this disease the inflammation is apt to travel over the bronchial membrane from place to place, as erysipelas is seen sometimes to wander over the surface of the body. I know not how this may be; but certainly death is often produced in these persons by the sudden spoiling of even a moderate portion of lung. In their ordinary condition, the patients have just enough, and no more, of the respiratory apparatus in an effective state, whereby to subsist; and when a fresh part of it is rapidly rendered solid, they quickly perish. But they die also from another cause. The nicety of treatment which I spoke of as being required in certain stages of acute bronchitis, is still more apparent and necessary here. We are placed in this dilemma. If we do not take blood in these attacks, we run a risk of losing our patient from the effects of the unchecked inflammation; and if we do

bleed, we are in danger of losing him by producing a degree of weakness which will render him unable to expectorate the effused mucus, and so liable to perish by suffocation. Leeches and blisters, and what are called expectorants, are the remedies to which we must chiefly trust. Medicines which are at the same time diuretic are also serviceable—the spiritus ætheris nitricus, the preparations of squill and of digitalis.

Even in younger patients, in whom the bronchitis is idiopathic, not engrafted upon any previous disease of the chest, and in whom the disorder had not appeared severe, extreme difficulty of breathing will sometimes most unexpectedly arise, and sometimes it will rapidly lead to the extinction of life. Now both Andral and Laennec have pointed out one cause (to which, indeed, I have already adverted in the present lecture) of this sudden change for the worse. It is a cause which was not likely to be even guessed at before the discovery of the method of auscultation. Occasionally it happens, they say, that during the course of an attack of bronchitis, we cease altogether to hear, in a certain extent of the lung, either the natural respiratory murmur, or any of the modifications of rhonchus, sibilus, or crepitation, that have been mentioned; yet over this same portion of lung, in which no sound, healthy or morbid, is heard by the ear applied to the thorax, percussion gives the natural hollow sound. At the same time the patient becomes affected with urgent dyspnœa. This sudden suspension of all sound of respiration they attribute to the temporary, or, in some cases, the permanent, obstruction of one of the bronchi, the ramifications of which are distributed to that portion of lung in which the respiration is no longer audible. In such cases it generally happens that at the end of a strong fit of coughing—the effect of which is to expel, or at least to displace the tenacious plug of mucus obstructing the bronchus—the sound of respiration is re-established as suddenly as it had previously disappeared; and the dyspnœa also ceases. In some rarer cases, however, the noise of the pulmonary expansion does not return, the difficulty of breathing increases, suffocation becomes imminent, and death by apnœa rapidly takes place. The slightest attack of bronchitis may in this way be suddenly transformed into a most serious and quickly fatal malady.

Andral relates two instances of death from this cause; one of which was the following. A coachman, fifty years old, had been several times a patient in La Charité, for obstinate pulmonary catarrh, with slight dyspnœa, and puriform expectoration. Every time he went away relieved, but not cured. On both sides of his chest could be heard all the

varieties of rhonchus. In one spot the column of air which penetrated the bronchi imitated the snoring of one in a deep sleep; in another spot it was like a dull and prolonged groan; in a third, a sound resembling that made by bellows; and in a fourth the cooing of a turtle-dove was exactly simulated. On the last occasion of his entering the hospital, his respiration was still tolerably free. One morning he was found in a state of unusual anxiety. In the middle of the night, after a violent paroxysm of cough, his breathing (he said) had suddenly become very much oppressed. It was discovered, on auscultation of his chest, that no air penetrated into the upper lobe of the right lung; yet that part sounded well on percussion, even louder than the corresponding part of the other side, which was morbidly dull. The difficulty of breathing went on augmenting, and the man was soon dead.

Besides other marks of disease in the lungs, the primary bronchus leading to the upper lobe on the right side was closed up completely by tough mucus, and exhibited the appearance of a full cylinder.

In the other case, also, the obstructed bronchial tube supplied the upper lobe of the right lung.

It may seem strange that the interruption of the access of air to so small a portion of the lungs should be attended with such serious consequences, when we know that the greater part of each lung may be impermeable by air, and yet the patient live a long time, and often without any great dyspnoea. The explanation of the apparent difficulty seems to be, that in the one case the prevention of the arrival of air in the part affected is sudden, in the other gradual. Moreover, the remaining portions of the lungs are performing their functions imperfectly.

When once attention has been awakened to the kind of accident just mentioned, the diagnosis would not seem to be difficult. We may suspect obstruction of one of the bronchi when considerable dyspnoea comes on suddenly during the continuance of simple bronchitis: and our suspicion will be confirmed if at the same time respiration ceases to be audible in a certain portion of the lung, while the sound given by percussion over the same part remains unaltered. Emphysema of the lung (which I shall explain hereafter) is the only other condition which could give rise to a similar set of symptoms.

Andral judiciously suggests the employment of emetics, and the inhalation of steam, in such cases.

LECTURES ON THE FUNCTIONS OF THE NERVOUS SYSTEM.

BY W. B. CARPENTER, M.D.

LECTURE X.

Functions of the Encephalon,—Medulla Oblongata,—Corpora Quadrigemina,—Cerebellum.

THE portion of the nervous centres contained within the cranium, and commonly designated collectively as the *encephalon*, may be regarded as consisting of three principal divisions: 1. the *cerebral hemispheres*, which, in the mammalia, and especially in man, constitute by far the largest portion of the whole; 2. the *cerebellum*, the complete separation of which from the *cerebrum*, and its distinct connections with the *medulla oblongata*, mark it out as an organ of peculiar character; and 3. the *medulla oblongata*, or cranial prolongation of the *medulla spinalis*, the upper end of which is chiefly composed of ganglia of special sensation, between which are the points whence those nerves originate, that are analogous to the ordinary spinal. It was stated, at the conclusion of the preceding lecture, that comparative anatomy furnishes us with reason to believe, that these ganglia are the peculiar seat of that *consciousness*, which is necessary for the performance of the actions ordinarily termed instinctive, and that they are the chief instruments in the performance of these actions; since we find them bearing a larger and larger proportion to the whole *encephalon*, and also to the nerves proceeding from it, as we descend the scale of intelligence. It was also stated that the correspondence between the emotional actions in man, and many of the instinctive actions of the lower animals, may lead to the belief that they are of analogous character, and are performed by the same instruments. A very simple illustration may suffice. The cuttle-fish is well known to discharge its ink, when pursued, and to tinge the water around with a colour so deep, as to enable it to escape under the cloud thus formed. Now it is not to be supposed that the cuttle-fish has any notion of the *purpose* which this act will serve; since its constancy and uniformity, and the provision for its performance immediately on the emergence of the young animal from the egg, forbid our regarding it as the result of any act of reasoning. Further, the ink is an excretion which corresponds to the urine (having been found to contain urea); and every one knows how strong an impulse to discharge

this is frequently caused by mental emotion. The same may be said of the strongly odorous secretions possessed by many mammalia, which are discharged under similar circumstances, and evidently with the same object, though of that object the animal itself be not conscious. The *emotion* of fear involuntarily opens the sphincters, and causes the contraction of the receptacle, in the one case as in the other: and the great difference in the condition of man and of the lower classes, in this respect, is simply that, in the former, the purely emotional or instinctive actions are few in comparison to the whole, whilst in the latter they constitute by far the largest part.

The emotions are concerned in man, however, in many other actions, which are in themselves strictly voluntary. Unless they be strongly excited, so as to get the better of the will, they do not operate directly through the nervous system, but are subservient to the intellectual operations, to which they supply materials, or motives. Thus, of two individuals, with differently constituted minds, one shall judge of every thing through the medium of a gloomy morose temper, which, like a darkened glass, represents to his judgment the whole world in league to injure him: and all his determinations, being based upon this erroneous view, exhibit the indications of it in his actions, which are, nevertheless, of an entirely voluntary character. On the other hand, a person of a cheerful benevolent disposition looks at the world around as through a Claude Lorraine glass, seeing every thing in its brightest and sunniest aspect; and, with intellectual faculties precisely similar to those of the former individual, he will come to opposite conclusions, because the materials which form the basis of his judgment are submitted to it in a very different form. Various forms of moral insanity exhibit the same contrast in a yet more striking light. We not unfrequently meet with individuals, still holding their place in society, who are accustomed to act so much upon *feeling*, and to be so little guided by *reason*, as to be scarcely regarded as sane; and a very little exaggeration of such a tendency causes the actions to be so injurious to the individual himself, or to those around him, that restraint is required, although the intellect is in no way disordered, nor are any of the feelings perverted. Not unfrequently we may observe similar inconsistencies, resulting from the habitual indulgence of one particular feeling, or a morbid exaggeration of it. The mother who, through weakness of will, yields to her instinctive fondness for her offspring, in allowing it gratifications which she knows to be injurious to it, is placing herself below the level of many less gifted beings. The habit of yielding to a

natural infirmity of temper often leads into paroxysms of ungovernable rage, which, in their turn, pass into a state of maniacal excitement. It is not unfrequently seen that a delusion of the intellect (constituting what is commonly known as monomania) has in reality resulted from a disordered state of the feelings, which have represented every occurrence in a wrong light to the mind of the individual. All such conditions are of extreme interest, when compared with those which are met with amongst idiots, and animals enjoying a much lower degree of intelligence; for the result is much the same in whatever way the balance between the feelings and the judgment (which is so beautifully adjusted in the well-ordered mind of man) is disturbed—whether by a diminution of the intelligence, or by an exaltation of the feelings. These views will, I think, be found correct, whatever be the truth of the speculation which I have laid before you, as to the part of the nervous system concerned in the performance of the purely emotional actions. That their channel is alike distinct, however, from that of the voluntary movements, and from that of reflex operations, will, I think, be apparent to any one who fairly weighs the evidence.

The degree in which animals high in the scale of organisation can perform the functions of life without any other centre of action than the medulla oblongata and cerebellum, appears extraordinary to those who are accustomed to regard the cerebral hemispheres as the centre of all energy. From the experiments of Flourens, Hertwig, Magendie, and others, it appears that not only Reptiles, but Birds and Mammalia, may survive for many weeks or months (if their wants be duly supplied) after the removal of the whole cerebrum. It is difficult to substantiate the existence in them of actual sensations; but their movements appear to be of a higher kind than those resulting from mere reflex action. One of the most remarkable phenomena in such a being, is the power of maintaining its equilibrium, which could scarcely exist without consciousness. If it be laid upon the back, it rises again; if pushed, it walks. If a bird thus mutilated be thrown into the air, it flies; if a frog be touched, it leaps. When violently aroused, it has all the manner of an animal waking from sleep; and it manifests just about the same degree of consciousness with a sleeping man, whose torpor is not too profound to prevent his suffering from an uneasy position, and who moves himself to amend it. The *negative* results of experiments of this kind are much more satisfactory than the positive; that is to say, if we are able to substantiate the performance of a particular function, after the removal of a certain organ, we may be sure that the

function is *not* dependent on that organ. But the converse does not hold good ; for it frequently happens that when such violent operations are practised on the nervous centres, they occasion an amount of general disturbance, which suspends or modifies functions that have no immediate connexion with the organ in question ; so that we cannot safely attribute the alteration in them to the loss of it. For example, Hertwig found that, upon removing the upper part of the hemispheres in a pigeon, the powers of sight and hearing appeared to be destroyed, and the animal sat in one spot, as if asleep ; but, being fed during a fortnight, the sensibility returned, and the bird lived for three months.

Among the ganglia of special sensation, the functions of the optic lobes, or *corpora quadrigemina*, have been chiefly examined. The researches of Flourens and Hertwig have shewn that their connexion with the visual function, which might be inferred from their anatomical relations, is substantiated by experiment. The partial loss of the ganglion on one side produces partial loss of power and temporary blindness on the opposite side of the body, without necessarily destroying the mobility of the pupil ; but the removal of a larger portion, or complete extirpation of it, occasions permanent blindness and immobility of the pupil, with temporary muscular weakness, on the opposite sides. This temporary disorder of the muscular system sometimes manifests itself in a tendency to move on the axis, as if the animal were giddy. No disturbance of con-

sciousness appears to be produced ; and Hertwig states that he never witnessed the convulsions, which Flourens mentions as a consequence of the operation, and which were probably occasioned by his incision having been carried too deeply.

Functions of the cerebellum.—In regard to the particular purposes which are served by the cerebellum, physiologists are still much in the dark ; although there are not wanting those who consider them well ascertained. That this organ has some special function distinct from that of the cerebral hemispheres, can scarcely be doubted ; since its peculiar structure and position, its independent connexions with the medulla oblongata, and its extremely variable size relatively to the remainder of the encephalon, point it out as an instrument adapted to some important purpose. We shall inquire briefly into the nature of the evidence respecting its function, which is supplied to us by comparative anatomy, by experiment, and by pathological phenomena. A cerebellum is found in all vertebrated animals, although it is in some extremely small,—looking like a little prominence on the medulla oblongata. When this is the case, it is observed that the whole mass is not a miniature (so to speak) of the large cerebellum of man, but that the central portion (termed the vermiform process) is the part most developed, the lobes not presenting themselves until the organ has acquired an increased dimension. The following table, constructed from materials contained in Serres' most valuable Comparative Anatomy

MAMMALIA.	Diam. of Spinal Cord at 2d Cervical Vertebra.	Transverse Diam. of Cerebellum.	Antero-posterior Diameter of Cerebellum.	Proportions.
Man . . .	1100	1,2000	6000	11 — 5 $\frac{1}{2}$
Simia Rubra	900	4500	2443	5 — 2 $\frac{1}{2}$
Bear . . .	1300	5900	3500	4 $\frac{1}{2}$ — 2 $\frac{1}{2}$
Dog . . .	1100	4200	2525	3 $\frac{3}{4}$ — 2 $\frac{1}{4}$
Dromedary.	1900	7100	4600	3 $\frac{3}{4}$ — 2 $\frac{1}{2}$
Kangaroo .	1200	3800	2600	3 $\frac{1}{8}$ — 2 $\frac{1}{8}$
BIRDS.				
Falcon . .	400	1350	1100	3 $\frac{1}{8}$ — 2 $\frac{3}{4}$
Swallow . .	3175	500	600	3 — 3 $\frac{1}{2}$
Turkey . .	500	1350	1600	2 $\frac{2}{3}$ — 2 $\frac{1}{3}$
Ostrich . .	700	1750	2500	2 $\frac{1}{3}$ — 3 $\frac{1}{2}$
REPTILES.				
Crocodile .	300	500	400	1 $\frac{2}{3}$ — 1 $\frac{1}{3}$
Frog . .	300	300	200	1 — $\frac{2}{3}$
FISHES.				
Shark . .	700	1700	3100	2 $\frac{1}{2}$ — 4 $\frac{1}{2}$
Cod . . .	575	1350	1700	2 $\frac{1}{3}$ — 3
Turbot . .	500	750	900	1 $\frac{1}{2}$ — 1 $\frac{4}{5}$
Lamprey .	275	225	100	$\frac{4}{5}$ — $\frac{3}{8}$

of the Brain, will afford some idea of the materials for speculating on the nature of the function of the cerebellum, which we obtain from this source. The first column gives the diameter of the spinal cord, at the second cervical vertebra; in the two succeeding columns are stated the transverse and the antero-posterior diameters of the cerebellum; these dimensions are stated in hundred-thousandths of a metre. The fourth column expresses, in round numbers, the proportion which the diameters of the cerebellum bear to that of the spinal cord; the latter being reckoned as one.

This table affords us much scope for interesting speculation, and may be applied to the correction of hypotheses erected upon other foundations. Before we proceed to these, however, a few general remarks may be made upon it. In the first place, the proportional development of the cerebellum is seen to be smallest in the vermiform Fishes, which approach most nearly to the Invertebrata; but it is much greater in the higher Fishes than it is in Reptiles. If we consider in what particular, that may be reasonably supposed to have a connexion with this organ, the former surpass the latter, we should at once be struck with their superiority in activity and *variety* of movement. Passing on to Birds, we remark that its average dimensions greatly surpass those of the organ in Reptiles; but that they do not exceed those occasionally met with in Fishes. The greater size is not found in the species which approach most nearly to the Mammalia in general conformation (such as the ostrich), but in those of most active powers of flight. Lastly, on ascending the scale of Mammiferous animals, we cannot but be struck with the rapid advance in proportional size that we observe, as we pass from the lowest, which are surpassed in this respect by many Birds, towards Man, in whom it attains a development which appears enormous when contrasted with that of the Quadrumana.

We have next to enquire what evidence can be drawn from experimental investigations on the same subject; and in reference to this it is desirable to remark, in the first place, that the experimental mode of enquiry is perhaps more applicable to this than to other parts of the encephalon, inasmuch as it can be altogether removed with little disturbance of the actions immediately essential to life; and the animals soon recover from the shock of the operation, and seem but little affected, except in some easily-recognized particulars. The principal experimenters upon this subject have been Rolando, Flourens, Magendie, and Hertwig. It is not to be expected that there should be an exact conformity among the results of all. Every one who has been engaged in physiological

experiments is aware of the amount of difference caused by very minute variations in their circumstances; in no department of enquiry is this more the case than in regard to the nervous system; and such differences are yet more likely to occur in experiments made upon the nervous centres, than in those which concern their trunks. The investigations of Flourens are the most clear and decisive in their results; and of these we shall accordingly take a general survey. He found that, when the cerebellum was mechanically injured, the animals gave no signs of sensibility, nor were they affected with convulsions. When the cerebellum was being removed by successive slices, the animals became restless, and their movements were irregular; and by the time that the last portion of the organ was cut away, the animals had entirely lost the powers of springing, flying, walking, standing, and preserving their equilibrium,—in short, of performing any combined muscular movements which were not of a simply-reflex character. When an animal in this state was laid upon the back, it could not recover its former posture; but it fluttered its wings and did not lie in a state of stupor. When placed in the erect position, it staggered and fell like a drunken man,—not, however, without making efforts to maintain its balance. When threatened with a blow, it evidently saw it, and endeavoured to avoid it. It did not seem that the animal had in any degree lost voluntary power over its several muscles; nor did sensation appear to be impaired. The faculty of combining the actions of the muscles in groups, however, was completely destroyed, except so far as those actions (as that of respiration) were dependent only upon the reflex function of the spinal cord. The experiments afforded the same results, when repeated upon all classes of vertebrated animals; and they have since been repeated, with corresponding effects, by Bouillaud and Hertwig. The latter agrees with Flourens, also, in stating that the removal of one side of the cerebellum affects the movements of the opposite side of the body; and he further mentions that, if the mutilation of the cerebellum have been partial only, its function is in great degree restored.

All these results are objected to by those who assert that the cerebellum is the seat of the sexual instinct, on the ground that the observed aberrations of the motor functions are sufficiently accounted for, by the general disturbance which an operation so severe must necessarily induce. The fallacy of this objection, however, is shown by the fact, that the much more severe operation of removing the hemispheres does not occasion such an aberration; the power of

performing the associated movements, and of maintaining the equilibrium, being remarkably preserved.

Upon comparing these results with the preceding table, a remarkable correspondence will be observed between them. The classes which have the greatest variety of movements, and which require for them the most perfect combination of a large number of separate muscular actions, have, taken collectively, the largest cerebellum. Of all classes of Vertebrata, Reptiles are the most inert; and their motions require the least co-ordination. The active predacious Fishes far surpass them in this respect; and may be compared with birds in the energy of their passage through the water, and in their facility of changing their direction during the most rapid progression. Their cerebellum, accordingly, bears to their spinal cord very much the same proportion with that of birds. On the other hand, the Flat Fish, which lie near the bottom of the ocean, and which have a much less variety of movement, have a very much smaller cerebellum; and the Vermiform Fishes, which are almost or completely destitute of fins, and whose progression is accomplished by flexion of the body, have a cerebellum so small as to be scarcely discoverable, their motion being, like that of the lower Articulata, almost entirely of a reflex character, each segment being influenced by its own ganglionic centre, and the spinal cord constituting by far the largest proportion of the nervous centres. On looking at the class of Birds, we observe that the active predacious Falcons, and the swift-winged Swallow (the perfect control possessed by which over their complicated movements every one must have observed), have a cerebellum much larger in proportion, than that of the Gallinaceous birds, whose powers of flight are small, or or than that of the Struthious tribe, in which they are altogether absent. Lastly, on comparing its proportional size, in the different orders of Mammalia, with the number and variety of muscular actions requiring combined movements, of which they are respectively capable, we observe an even more remarkable correspondence. In the hoofed quadrupeds, in which the muscular apparatus of the extremities is reduced to its greatest simplicity, and in which the movements of progression are simple, the cerebellum is proportionally smaller than it is found to be in some birds; but in proportion as the extremities acquire the power of prehension, and together with this power of application to a great variety of purposes—still more, in proportion as the animal becomes capable of maintaining the erect posture, in which a constant muscular exertion, consisting of a number of most elaborately com-

bined parts, is required,—do we find the size of the cerebellum, and the complexity of its structure, undergoing a rapid increase. Thus, even between the dog and the bear there is a marked difference, the latter being capable of remaining for some time in the erect posture, and often spontaneously assuming it, whilst to the former it is any thing but natural. In the semi-erect apes, again, there is a very great advance in the proportional size of the cerebellum; and those which most approach man in the tendency to preserve habitually the erect posture, also come nearest to him in regard to the size of the cerebellum. Now it is evident that man, although far inferior to many of the lower animals in the power of performing various particular kinds of movement, far surpasses them all in the number and variety of the combinations which he is capable of executing, and in the complexity of the combinations themselves. Thus, if we attentively consider the act of *walking* in man, we shall find that there is scarcely a muscle of the trunk or extremities which is not actually concerned in it; some being engaged in performing the necessary movements, and others in maintaining the equilibrium of the body, which is disturbed by them. On the other hand, in the horse or camel, the muscular movements are individually numerous, but they do not require nearly the same perfect co-ordination. And in the bird, the number of muscles employed in the movements of flight, and in directing the course of these, is really comparatively small; as may at once be perceived by comparing the rigidity of the skeleton of the trunk of the bird with that of man, and by remembering the complete inactivity of the lower extremities during the active condition of the upper. In fact, the motions of the wings are so simple and regular, as to suggest the idea that, as in Insects, their character is more reflex than directly voluntary;—an idea which is supported by the length of time during which they can be kept up without apparent fatigue, and also by an important fact hereafter to be mentioned, which experimental research has disclosed. It is seen, then, that comparative anatomy fully confirms the idea which experimental physiology suggests, respecting the chief functions of the cerebellum.

Some of Magendie's experiments indicate a further connexion of this organ with the motor function, the nature of which is still obscure. This physiologist asserts that, if a wound be inflicted on the cerebellum, the animal seems compelled by an inward force to retrograde movement, although making an effort to advance; and that, if the crus cerebelli on one side be injured, the animal is caused to roll over towards the same side. Sometimes (if Magendie's statements can be

relied on), the animals made sixty revolutions in a minute, and continued this movement for a week without cessation. Division of the second crus cerebelli restores the equilibrium. Hertwig observed the same phenomenon, when the pons Varolii (which is nothing more than the commissure of the cerebellum, surrounding the crura cerebri) was injured on one side; and he has also remarked that the movements of the eyes were no longer consensual.

On turning to pathology for evidence of the functions of the cerebellum, we meet with much that seems contradictory. It must be remembered that a *sudden* effusion of blood, even to a small extent, in *any* part of the encephalon, is liable to produce the phenomena of apoplexy or paralysis; and inferences founded upon the phenomena exhibited after sudden lesions of this description are, therefore, much less valid than those based on the results of more chronic affections. In regard to these last, however, it is to be observed, that we are not yet in a condition to be able to state with precision, what amount of morbid alteration in any part of the nervous centres is compatible with but slightly-disturbed performance of its function; and that cases are every now and then occurring, which would upset all our previous notions, if we were not aware that the same difficulty presents itself even in regard to the best established results in neurology. It is also to be remembered, that the results of disease occasioning *pressure*, will be peculiarly liable to affect the medulla oblongata as well as the cerebellum; and thus will occasion a greater loss of motor power than would be occasioned by the mere suspension of the function of the latter.

Pathological phenomena, when examined with these reservations, appear to coincide with the results of experiment, in supporting the conclusion that the cerebellum is not in any way the instrument of *psychical* operations. Inflammation of the membranes covering it, if confined to that part, does not produce delirium; and its almost complete destruction by gradual softening does not appear necessarily to involve loss of intellectual power. "But," remarks Andral, "whilst the changes of intelligence were variable, inconstant, and of little importance, the lesions of motion, on the contrary, were observed in all the cases (of softening, which had come under his notice) except one; and in this it is not quite certain that motion was not interfered with." In general, apoplexy of the cerebellum is accompanied by paralysis; but this is by no means usual in cases of chronic disease, in which there is rather an irregularity of movement, with a degree of restlessness resembling that described by Flourens as resulting from partial

injury of this organ. In a few cases in which both lobes of the cerebellum have been seriously affected, the tendency to retrograde movement has been observed; and instances are also on record of the occurrence of rotatory movement, which has been found to be connected with lesion of the crus cerebelli on the same side. So far as they can be relied on, therefore, the results of the three methods of investigation bear a very close correspondence; and it can scarcely be doubted that they afford us some approximation to truth.

We have now to examine, however, another doctrine regarding the functions of the cerebellum, which was propounded by Gall, and which is supported by the phrenological school of physiologists. This doctrine—that the cerebellum is the organ of the sexual instinct—is by no means incompatible with the other; and by some it has been held in combination with it. The greater number of phrenologists, however, regard this instinct as the *exclusive* function of the cerebellum, and assert that they can judge of its intensity by the degree of development of the organ. We shall now inquire what evidence in support of this position is afforded by the three methods of inquiry which have been already indicated.

I cannot but regard the results of observation as to the comparative size of the cerebellum in different animals, as very unfavourable to the doctrine in question. In the greatest number of Fishes, it is well known that no sexual congress takes place, the seminal fluid being merely effused like any other excretion, into the surrounding water, and being thus brought into accidental contact with the ova, of which a large proportion are never fertilized. On the other hand, in many Reptiles the sexual instinct appears extremely strong; and this is especially the case in the frog, the whole system of which is endowed, at the season of fertility, with an extraordinary degree of excitability, analogous to a morbid condition that sometimes presents itself in the human being. It has been remarked that, if the head of a male frog be cut off, during the congress (which lasts for some time), his embrace will not be relaxed, and will even continue until the body of the female is becoming gangrenous from the pressure; thus showing that the action is one of a purely reflex character. Now on comparing the size of the cerebellum of the frog with that of the cod (we exclude the higher cartilaginous fishes, in which the reproductive function has a more elevated type) we find that it is not above one-half the proportional size. Moreover, not only is the size much inferior, but the structure is much less complicated, in the former than in the latter. Again, in comparing

the Gallinaceous Birds, which are polygamous, with the Raptorial and Insessorial tribes, which live in pairs, we find that the former, instead of having a larger cerebellum, have one of inferior size. Further, on looking at the Mammalia, the same disproportion may be noticed. A friend who kept some kangaroos in his garden, informed me that they were the most salacious animals he ever saw; yet their cerebellum is one of the smallest to be found in the class. Every one knows, again, the salacity of monkeys; there are many which are excited to violent demonstrations by the sight even of a human female; and there are few which do not practise masturbation, when kept in solitary confinement; yet in them the cerebellum is much smaller than in man, in whom the sexual impulse is much less violent. It has been supposed that the large size of the organ in man is connected with his *constant* possession of the appetite, which is only *occasional* in others; but this does not hold good; since among domestic animals there are many which are ready to breed throughout the year—cats and rabbits, for instance; and in these we do not find any peculiar difference in the size of the cerebellum. It is asserted, however, that the results of observation in Man lead to a positive conclusion, that the size of the cerebellum is a measure of the intensity of the sexual instinct in the individual. This assertion has been met by the counter-statement of others, — that no such relation exists. It is unfortunate that here, as in many other instances, each party has registered the observations favourable to their own views, rather than those of an opposite character; so that, until some additional evidence, of a less partial nature, has been collected, we must consider the question as *sub judice*. I am by no means disposed to deny that such a correspondence *may* exist; but on contrasting the degree of support which this part of phrenology really derives from pathological evidence, with that which the upholders of this view represent it to receive, I cannot but look with much distrust at all their observations on the subject.

You will find it stated in phrenological works, as an ordinary result of disease of the cerebellum, that there is an affection of the genital organs, manifesting itself in priapism, turgescence of the testes, and sometimes in seminal emissions. Now it is quite true that, in cases of apoplexy, in which these symptoms manifest themselves, there is very commonly found to be effusion upon the cerebellum or in its substance; but it is to be remembered that in all such lesions the medulla oblongata is involved, and these symptoms, equally with paralysis, may be due to affection of that organ. Further, the converse does not by any means hold

good; for the proportion of cases of disease of the cerebellum, in which there is any manifest affection of the sexual organs, is really very small, being, according to the calculations of Burdach, not above *one in seventeen*. The same physiologist states that such affections do present themselves, although very rarely, when the cerebrum is the seat of the lesion. A large number of facts adduced by phrenologists in support of their views—such as the erections and emissions which often take place during hanging—may be explained as well, or even better, on the hypothesis that the cerebro-spinal axis (that is, the spinal cord and medulla oblongata) is the seat of this instinct. And this hypothesis is much more conformable to the results of experiment and disease, than that which locates it in the cerebellum. For it has been found that mechanical irritation of the spinal cord, and disease in its substance, much more frequently produce excitement of the genital organs, than do lesions of the cerebellum. This view is entertained by Müller, and by most physiologists who have taken a comprehensive and unbiassed survey of the phenomena in question. I am far from denying *in toto* that any peculiar connection exists between the cerebrum and the genital system; but if the evidence at present adduced in support of the phrenological position be held sufficient to establish it, in defiance of so many opposing considerations, we must bid adieu to all safe reasoning in physiology. The weight of testimony appears to me to be quite decided, in regard to the connection of the cerebellum with the regulation of the motor function. How far this invalidates the *moderate* phrenological view, which does not regard the function of the cerebellum as *exclusively* devoted to the sexual instinct, is a question well deserving of attention. There is nothing opposed to such an idea in the results of the experiments already adverted to; since there is no evidence that sexual instinct remained after the removal of the cerebellum; but, on the other hand, there is no proof that it was destroyed. A circumstance which has been several times mentioned to me, that great application to gymnastic exercises greatly diminishes for a time the sexual vigour, and even totally suspends desire,—seems worthy of consideration in reference to such a view. If the cerebellum be really connected with both kinds of functions, it does not seem unreasonable that the excessive employment of it upon one should diminish its energy in regard to the other. Further, it would seem by no means improbable that the lobes are specially connected with the regulation and co-ordination of movements; whilst the vermiform processes, which are very large in many

animals in which these scarcely present themselves, are the parts connected with the sexual function. As an additional argument in favour of the former part of this view, it may be stated, that in man the lobes bear a larger proportion to the vermiform processes than in any other animal; and that they undergo their most rapid development during the first few years of life, when a large number of complex voluntary movements are being learned by experience, and associated by means of the muscular sensations accompanying them: whilst in those animals which have, immediately after birth, the power of regulating their voluntary movements for definite objects, with the most precision, the cerebellum is more fully developed at the time of birth. In both instances it is well formed and in active operation (so far as can be judged by the amount of circulation through it) long before the sexual instinct manifests itself in any perceptible degree.

CLINICAL REPORTS

OF

DIFFICULT CASES IN MIDWIFERY.

BY ROBERT LEE, M.D. F.R.S.

[Fifth Report concluded.]

Induction of premature labour in cases of pregnancy, with malignant disease of the uterus, fibrous tumors, ovarian cysts, organic affections of the heart, dropsy of the amnion, and obstinate vomiting.

THE preceding part of this Report contained the histories of several cases of slight distortion of the pelvis, in which premature labour was successfully induced at the seventh and a half month of pregnancy. It contained, also, an account of twenty-two cases in which the advantage of inducing premature labour was not less striking, although the degree of distortion was so great that a child even of seven months could not be born alive. In the following cases the same operation was, or might have been, employed with advantage.

CASE CXXXVIII.—On the 1st of May, 1840, Mr. Cross, of Leicester Square, requested me to see Mrs. A., æt. 41, who had been twenty-four hours in labour. The os uteri was hard, irregular, and ulcerated, and so little dilated that the presentation could not be ascertained. The symptoms of malignant disease of the uterus

had commenced two years before, and the pain and discharge became aggravated when conception took place. The labour pains continued strong and regular the whole afternoon and night of the 1st of May. At 7 A.M. of the 2d, the pains were violent and incessant, with restlessness and sickness at stomach. The os uteri continued precisely in the same condition, and the presenting part could not be felt. Twenty-five ounces of blood were drawn from the arm, and one drachm of laudanum administered.

At 11 P.M., the pains still continuing violent, with severe rigors, rapid pulse, and incoherence, another effort was made to reach the presenting part, though the os uteri was still undilated. Having succeeded in touching the head, the perforator was passed up along the finger, and the skull opened, and the brain destroyed. The propriety of making incisions into the diseased os uteri had been considered, before the head could be opened, but the idea was abandoned, after consulting an eminent surgeon.

At 6 A.M. May 3, the labour pains having continued strong and regular during the night, the os uteri opened sufficiently to allow the crotchet to be introduced, and the head extracted. The placenta soon followed the child, but she continued gradually to sink, and died on May 4. The body was examined by Mr. Cross, and the neck of the uterus, extensively lacerated, presented the appearance of an irregular, dark-coloured, disorganized mass. The danger of dying undelivered, and the injury necessarily inflicted upon the uterus by the extraction of the child, would have been avoided or lessened in this case, by the induction of premature labour.

CASE CXXXIX.—Several years ago, a woman, with malignant disease of the os uteri, and who was three or four months pregnant, was admitted into St. George's Hospital. She afterwards went to Margate, but though labour came on at the end of the seventh month, the os uteri did not dilate sufficiently to allow the head of the foetus to pass, and it was extracted with the vectis. Symptoms of ruptured uterus soon followed. Mr. Price examined the body after death, and sent the uterus to me. The whole orifice and neck of the organ was

destroyed by cancerous ulceration, and the anterior part of the cervix was lacerated.

CASE CXL.—In April 1840 I was requested by Dr. James Johnson to see a patient who had a malignant fungoid disease of the os uteri. The catamenia had disappeared for several months, and she had morning sickness, and other symptoms which made her believe that she was pregnant. In May, the abdomen had enlarged, and the sound of the pulsations of the foetal heart and uterine arteries were distinctly heard, and the movements of the foetus felt with the hand. The areolæ were broad and dark, and the glands around the nipples enlarged. I recommended premature labour to be induced; but she would not consent to the operation. Delivery, however, took place spontaneously on July 14, and a dead child of seven months was expelled, without artificial assistance. The pain, discharge, and other symptoms of cancer, almost entirely disappeared for several months after her confinement, but they returned, and Mr. Rawbone, of King's Road, Chelsea, informed me that she died on January 1, 1841.

On relating these cases to Dr. Merri-man, on May 7, he informed me that he had met with three of a similar nature. In one, the labour pains were excruciating, and continued for a long time, without producing any effect upon the os uteri, but at last it gave way suddenly, and the head passed through it. The child was born alive. The mother died six weeks after.

On returning home from attending a case of labour, Dr. Merriman was informed by his uncle, the late Dr. S. Merriman, that the patient had scirrhous of the os uteri, that he thought she would never have another child, and would die from the disease. That, he said, was the result of his experience in other similar cases. She conceived again, however, and died soon after delivery. The child was born alive, and neither in this nor in the last case was any operation performed.

The history of Dr. Merriman's third case of labour, complicated with cancer uteri, is contained in the following letter :—

My dear Dr. Lee,—The following case, which I did not recollect last night, will interest you; I do not remember any one very similar.

August 12, 1824, I was desired, by my excellent friend, Mr. Clifton, of Leicester Place, to visit Mrs. ——. She told me that she had been long ill, and had consulted Dr. Bree, who thought her disease was ulcer of the womb, and treated her accordingly; she said, however, she was quite sure of being pregnant, and not liking to continue a mode of treatment which she imagined must be improper, had ceased to consult him.

On making inquiry into her symptoms, it appeared that Dr. Bree was justified in taking the view he did of her complaint; they were, extreme pain in the back, constipation, emaciation, and especially extremely offensive discharges from the vagina. These symptoms had occurred after a lying-in, two years before, and had continued ever since; indeed there was some reason to believe that they had shown themselves before the termination of that pregnancy. Upon this point, however, the evidence was not quite satisfactory.

Having learnt thus much, I inquired what were her reasons for thinking herself pregnant; her reply was, that she distinctly felt the motions of the child. As no examination per vaginam had been instituted, I obtained permission to pass my finger, and was not a little surprised to find that the os uteri, and part of the cervix, were entirely destroyed by a species of *ulcus exedens*, yet that the body of the uterus was enlarged to the size it usually attains between the fifth and sixth month, and that it contained a living foetus; so that this was a case of pregnancy in a uterus deprived by disease of a large portion of its substance. Whether the os uteri had taken on disease, before the pregnancy commenced, must remain uncertain; but at the time of my examination, which was made with as much care and exactness as was in my power, the whole of the os uteri, and a large portion of the cervix, were literally eaten away by ulceration.

On conferring with Mr. Clifton, who accompanied me, on the treatment to be adopted, I expressed an opinion that the foetus could not be much longer retained, for the distension of the uterus was already beginning to intrench upon the cervix; and as the distension proceeded, the ulcerated cervix would be pressed upon, and must

necessarily give way, and the foetus be expelled; accordingly, on the 31st of August, the foetus and placenta passed into the world, almost without pain; the child, of about six months' gestation, was born alive, and lived a few hours. The poor woman, who now began to entertain hopes of recovery, was not at all benefited by the delivery, but continued to live in a state of great suffering, sometimes mitigated by narcotics, till the month of February, when death gave her a happy release.

Believe me, dear sir,

Yours with great truth,

SAMUEL MERRIMAN.

Brook Street, Grosvenor Square,
May 8, 1840.

In Dr. F. Ramsbotham's first case, the labour was premature, and the child was easily expelled. The woman died two weeks after. The second woman died undelivered.

Dr. Henry Davies has related to me a case of labour at the full period, with cancer, in which the uterus was ruptured, and death speedily followed delivery.

In 1770, Dr. Denman saw a case of malignant fungoid tumor of the os uteri with pregnancy at the end of the ninth month. The operation of embryotomy was performed, but the patient died before the child could be extracted.

He states that smaller tumors of the same character are not unfrequently met with in practice, and that little effect is produced by the labour pains for a long period, but that all at once the rigid os uteri yields and dilates speedily and unexpectedly, or perhaps in some instances is lacerated. In some cases, also, he states that the excrescences are of so tender a structure, that they are crushed by the passage of the head over them, and entirely destroyed.

If abortion does not take place where pregnancy exists with cancer of the os uteri in an advanced stage, the membranes of the ovum should be perforated, and if the disease is less extensive, at the seventh and a half month.

CASE CXLI.—A female, æt. 30, in the fifth month of her first pregnancy, began to suffer from sickness, fever, and constant pain and distension of the abdomen. On examining the belly it was easily perceived that the gravid uterus was pressed to the left side by a hard, painful, lobulated tumor on the right. This continued rapidly to en-

large, and to become more exquisitely painful, though leeches were applied in great numbers over the tumor, and calomel, antimony, opium, and cathartics, were administered internally. The painful distension of the abdomen soon became so great, that it was necessary to obtain relief by inducing premature labour. This was easily done. For a short time after delivery, the symptoms were less severe, but the fever, sickness, and painful distension, soon returned, and proved fatal.

A fibrous tumor, in a state of inflammation and suppuration, was found attached by a large root to the right side of the body of the uterus. The peritoneum which covered it adhered to the parietes of the abdomen, omentum, intestines, and liver. Numerous small fibrous tumors, the blood-vessels of which have been injected, were found imbedded in other parts of the parietes of the uterus. These were in a healthy state. This case occurred in the summer of 1840.

CASE CXLII.—On December 6, 1840, Dr. Blakeley Brown requested me to see a case of pregnancy, complicated with an ovarian tumor, which had appeared five years before conception took place, and had slowly enlarged. The patient was in the sixth month of her first pregnancy, and the abdomen was enormously distended, and a fluctuation perceived on percussion. The difficulty of breathing was so urgent that it was impossible for her to remain an instant in the horizontal position. We considered it necessary to induce premature labour, but the os uteri was so high up, and directed so much backwards, that great difficulty was experienced in passing the stiletted catheter into the uterus, to perforate the membranes. The anterior lip of the os uteri could only be reached with the finger. An instrument with a sharp point, and a smaller curve than that employed, could not have been introduced in this case, and the liquor amnii evacuated. On the 7th, labour pains commenced. Venesection and opiates were employed to promote the dilatation of the os uteri. The nates presented; and on the 9th, a dead foetus was expelled. The relief from the delivery was great, though the abdomen continued large, and the fluctuation distinct for several weeks.

August 10, 1841.—The ovarian tumor

has been considerably reduced in size since the repeated application of leeches, and the long-continued use of the liquor potassæ. The general health is nearly in the same condition as before pregnancy.

CASE CXLIII.—On March 12, 1828, I was called to an unmarried lady, whose abdomen had been slowly enlarging for several months. No suspicion having been entertained by her medical attendant that she was pregnant, mercury, diuretics, and the strongest cathartics, had been given to remove the supposed dropsy, and tapping proposed. When I examined the abdomen, the fluctuation was as distinct as in any case of ascites, and the right lower extremity was œdematous. Having received a hint from her nurse that pregnancy was possible, I examined and found the os uteri considerably dilated, and the membranes protruding. The presenting part of the foetus could not be felt. Labour pains having come on, and continued at long intervals during the night, I ruptured the membranes the following day, and sixteen pints of liquor amnii escaped. The head of the foetus being greatly distended with fluid, it would not pass, till perforated. Profuse hæmorrhage followed the expulsion of the child and placenta, and she died three days after, from inflammation of the muscular coat of the uterus.

CASE CXLIV.—On September 21, 1839, at 9 P.M. Mr. Young, of Piccadilly, requested me to see a patient in the seventh month of pregnancy with dropsy of the amnion. The abdomen was so enormously distended, that she could not for an instant assume the horizontal position. Fluctuation was distinctly perceived as in cases of ascites. The cervix uteri was obliterated, and the movements of the child in the liquor amnii felt. The abdomen had begun suddenly to enlarge three weeks before, and urgent dyspnœa soon followed. From the lividity of the countenance, the distressing sense of suffocation, and the coldness of the extremities, it could not be doubted that she would speedily sink, if not relieved. I passed up the stiletted catheter into the uterus without difficulty, and punctured the membranes at three points. The liquor amnii immediately began to flow profusely, and,

before the morning, ten quarts had escaped, and two premature foetuses had been expelled without difficulty. She recovered favourably.

CASE CXLV.—At 3 P.M. on January 2, 1840, I was called to see a lady, four or five months pregnant, affected with dropsy. The abdomen was much swollen, and the face, trunk, and extremities, œdematous. The difficulty of breathing was so urgent, that she was supported sitting upon the edge of the bed, with the feet resting upon a chair. It was obvious, if the symptoms were not relieved, that she could not live many hours. They had commenced in the second month of pregnancy, and had increased rapidly during the previous week. Diuretics, blisters, and drastic cathartics, had been employed without the slightest benefit.

On perforating the membranes with the stiletted catheter, an immense quantity of fluid rushed from the uterus, and continued to flow till the floor of the apartment was deluged. Although the size of the abdomen was reduced, the difficulty of respiration continued, with lividity of the lips, and rapid pulse. Six hours after the discharge of the liquor amnii, the os uteri was dilated to the size of a crown, but there were no labour pains. The distressing symptoms continued till the afternoon of the 3d, when the foetus and placenta were expelled without hæmorrhage. The power of swallowing was soon after lost, and she died in a few hours.

Four pounds of serum were found in the sac of the peritoneum, and three ounces in the pericardium. The heart was sound. The lungs were gorged with serum, and portions were unusually dense, and sank in water. The liver was healthy. The kidneys were harder than natural, the cortical part cutting like hard pork. The corpus luteum presented the usual appearance, both layers of the Graafian vesicle being inclosed within the yellow matter, and this was in immediate contact with the stroma of the ovary which contained it. The amnion was carefully examined, and was without blood-vessels.

CASE CXLVI.—At midnight, Sept. 14, 1832, Mr. Harvey, of Great Queen Street, requested me to see a patient with him in labour, who had the most distressing dyspnœa. She was held

up at an open window, and was gasping for breath. The face was livid, the extremities cold and œdematous. The os uteri was fully dilated, and the head had partially entered the brim of the pelvis. The pains had ceased. Mr. Harvey informed me that she had some valvular disease of the heart, and that dropsical symptoms appeared soon after she became pregnant. When labour came on, there was much difficulty of breathing experienced when she attempted to lie down, and as the first stage of labour proceeded, the dyspnœa increased, and became so severe that she seemed in danger of dying from immediate asphyxia. It was evident that she could not have long survived without being delivered, and that she had no power to expel the child. If the head had descended lower into the pelvis, it would have then been impossible to deliver her with the forceps, while held up by her friends before an open window. I opened the head, and extracted it with the craniotomy forceps.

The alarming difficulty of breathing gradually subsided, and she was alive a year after, and in her usual state of health.

CASE CXLVII.—On December 6, 1827, I was called to a young woman, near the full period of pregnancy, who had suffered for several years from an organic disease of the heart. The face was livid, the extremities cold, the pulse rapid and feeble, and the dyspnœa urgent. For three months she had suffered severely from palpitation of the heart, and frequent attacks of violent dyspnœa, threatening suffocation. The symptoms were relieved by V.S. to $\frac{3}{4}$ xii. putting the feet and legs in warm water, and giving an antispasmodic draught.

On the morning of the 7th she felt much better, but at 8 P.M. the difficulty of breathing returned, and she suddenly fell down and expired.

The pericardium adhered throughout closely to the heart, and the pleura of the lungs to that of the ribs extensively on both sides. The air cells on both sides were gorged with bloody mucus, and portions of the lungs hepatized. The uterus and its contents were healthy.

CASE CXLVIII.—Twelve years ago, a patient of the Middlesex Hospital, with organic disease of the heart, and who was seven months pregnant, sunk

down dead, suddenly. I was called to her half an hour after, when the action of the heart had entirely ceased. For several months previously she had suffered much from violent fits of dyspnœa and palpitation of the heart.

CASE CXLIX.—On December 11, 1838, with Mr. Jorden and Mr. Potter, I examined the body of a woman who had died suddenly the previous day, from organic disease of the heart. She was in the ninth month of pregnancy, and the fatal result was unexpected.

CASE CL.—About two years ago, a woman, six months pregnant, was admitted into St. George's Hospital, under the care of Dr. Chambers, with expectoration of blood, dyspnœa, and signs of valvular disease of the heart. The symptoms became gradually more urgent, and she died soon after the expulsion of the contents of the uterus. The tricuspid valve was diseased, and the lungs apoplectic.

CASE CLII.—A young married lady, in the fifth month of her first pregnancy, who had previously been in good health, began to suffer from violent irregular action of the heart, aorta, and carotid arteries. Several eminent physicians were consulted; who were led to believe, from the violence of the symptoms, that aneurism of the arch of the aorta existed. As pregnancy advanced, the patient became worse, and an unsuccessful attempt was made by an experienced accoucheur to induce premature labour. All who saw the case admitted that this was necessary, and the only means which could preserve her life; blood-letting, digitalis, and all other remedies, having failed to afford relief. She continued to suffer so much during the last three months of pregnancy, that it was feared some unfortunate accident would occur during delivery. Considerable anasarca of the face, legs, and arms, took place several weeks before the full period, with partial relief of the internal affection. The labour took place in July 1833, and was perfectly natural. The palpitation of the heart gradually disappeared, and she suckled her child.

CASE CLIII.—Mr. Beaman, of Henrietta Street, Covent Garden, requested me, several years ago, to see a patient, in Lambeth, who was in the fourth month of pregnancy, and had suffered for several weeks from incessant vomiting, with pain of epigastrium, and fever.

When every kind of treatment had failed, I punctured the membranes of the ovum, and discharged the liquor amnii. The vomiting ceased immediately after, and the fever subsided, though the foetus was not expelled for several weeks.

CASE CLIII.—In October 1836, Mr. Webster, of Connaught Terrace, called me to see a patient who was two months pregnant, and who had been attacked with faintness, violent sickness, and headache, soon after conception, and which had been gradually becoming more distressing. There was great emaciation and fever. The tongue was red, and the epigastrium tense and painful on pressure. The symptoms having assumed a very alarming character, and all remedies being useless, we resolved to puncture the ovum, and for this purpose the instrument was introduced into the uterus, but no liquor amnii followed. The vomiting, however, began immediately to subside, and she went to the full period, and was safely delivered of a living child.

CASE CLIV.—On May 17, 1838, I saw a lady, with Drs. Ramsbotham and Ashwell, in the early period of pregnancy, who had violent vomiting, great tenderness of the epigastrium and right hypochondrium, yellowness of the eyes, thirst, and quick pulse. The emaciation was so great, that, had it proceeded much further, she would soon have become completely exhausted. Had the symptoms not subsided under the use of calomel, the repeated application of leeches to the region of the liver, and very low diet, it would have been necessary to induce abortion.

CASE CLV.—A young married lady was attacked with constant sickness and vomiting at the commencement of the third month of her first pregnancy. It continued, in spite of all remedies, for ten weeks, when she was reduced to a state of the greatest emaciation and debility. When apparently dying, I recommended the induction of premature labour, but her husband and relatives would not consent to the operation. For a considerable period nothing was retained upon the stomach except a little brandy and water, and no hope was entertained of her recovery. Without any evident cause, the symptoms, however, gradually subsided, and she was safely delivered at the end of the

seventh month of a dead child, with a diseased placenta.

In this case the membranes should have been perforated long before the proposal was made.

CASE CLVI.—A lady, æt. 29, being six weeks pregnant, suffered severely from sea sickness, on the passage from Dublin to Liverpool, at the end of June 1839. The irritability of the stomach gradually became more distressing after her arrival in London, at the beginning of July, and nothing was retained, except a little brandy and water, for nearly twenty days. Prussic acid, effervescing draughts, calomel and opium, leeches, laudanum, and blisters to the pit of the stomach and region of the uterus, and all the other ordinary remedies, were totally useless. The emaciation and fever had become so great on July 22d, that it was evident she would soon die if not relieved. Dr. Merriman then saw her, along with Mr. Jorden and myself, and advised the *mistura cretæ* to be given, and creosote, and abortion to be induced if the symptoms were not relieved. To prove the necessity of great caution in this proceeding, Dr. M. related to us a case of vomiting during pregnancy, which had occurred some years before to a celebrated accoucheur, which had ended fatally after the performance of this operation, and for which he had incurred much odium. On the 24th, the symptoms being still more alarming, I evacuated the liquor amnii. Calomel, opium, and prussic acid, with the carbonate of soda, and a blister to the epigastrium, were ordered, but they did no good. On the 27th, the ovum was expelled, and a considerable quantity of coagulated blood, and she soon after began to sink, and died in a few hours. Drs. Chambers and Bright were also consulted in this distressing case. The coats of the stomach and bowels, and all the other viscera, were in a healthy state, and no morbid appearances could be detected in the membranes of the ovum.

About a drachm of clear fluid escaped from the *corpu luteum* when it was opened. Both layers of the Graafian vesicle were within the yellow matter, as represented in plate 7, vol. xxii. *Med. Chir. Transactions*. The nerves of the uterus, probably the seat of the disease, were not examined.

ON DIABETES.

To the Editor of the Medical Gazette.

SIR,

IF the enclosed outline of an essay on Diabetes (which, though first commenced some years ago, want of leisure, as well as some hesitation as to the propriety of my attempting a subject so recently and so luminously treated by Dr. Prout, at present prevents me from publishing in a more extended shape) should suit your pages, it is at your disposal.—I am, sir,

Your obedient servant,

E. STANLEY, M.D.

Milford, Hants, Aug. 18, 1841.

Among the diseases whose pathology is avowedly obscure, one of the foremost is diabetes. The following are the opinions respecting its nature entertained by those who have particularly directed their attention to its investigation:—

1. "The disease is dependent upon a morbid action of the stomach, or some of the chylofacient viscera, which necessarily, therefore, constitute its seat;" or, 2, "upon a dyscracy or intemperament of the blood, produced by a morbid action of the assimilating powers;" or, 3, "upon a retrograde action of the lacteals [lymphatics], and is consequently seated in the lacteal [lymphatic] vessels;" or, 4, "upon a morbid condition of the kidneys, and seated in these organs*."

The following are the objections which have been offered against these several views:—

1. As to the opinion that the chylo-poëtic organs are primarily affected, M. Good says, "the blood not containing a trace of sugar before it reaches the kidneys, seems a fatal objection to it." Now though this objection is apparently invalidated by more recent experiments, which have satisfactorily proved that "sugar is not found in the blood in a state of health, but it has been many times distinctly recognized in the blood of diabetic patients, where it probably always exists in a greater or less degree" (Prout on Stomach and Urinary Diseases, 1840. Introd., p. xciii.; not to quote other authorities), still we ought not to lose sight of the experiments of

Wollaston, Marcet, Henry, Bostock, Kane, Berzelius, Thenard, Vauquelin, Segalas, Nicolas and Gueudeville, &c.; from which it is evident that (in many cases at least) so little, if any, sugar is found in diabetic blood that we cannot, as a rule, suppose the far more considerable quantity met with in the urine to have been exclusively elaborated in the stomach, and thence, without having been duly assimilated by conversion into other principles, to have passed into the sanguiferous system. It may also be asked, how can any disorder of those organs account for the immoderately increased quantity of urine, which often far exceeds the ingesta, both liquid and solid, and which may, I think, with so much more plausibility be ascribed to a supply of the elements of sugar and water from elsewhere*, arising from a diminution either of the insensible perspiration, by which about half of the food (in weight) introduced into the stomach is said to be daily discharged again from the system; or of the exhalations from the lungs; or else from increased absorption from the atmosphere.

2. That there exists a "dyscracy" or imperfect animalization of the blood, seems evident; but from reasons just stated, it appears attributable to disordered function of other organs rather than those of assimilation primarily and exclusively.

3. With respect to the retrograde action of the lymphatics. Mr. Cruickshank (on the Lacteals and Lymphatics, p. 69) asks the pertinent question, why such an action should be only partial, and why the fæces should not be fraught with a similar fluid; and Dr. Wollaston thinks it so strongly opposed by the known structure of that system of vessels, that he believes few persons will admit its probability (Phil. Trans. 1811, p. 105.) Dr. Baillie is of a similar opinion (Trans. Med. Chir. Soc. vol. ii.)

4. Dr. Latham (Facts and Opin. concerning Diab. 1811), who considers the excessive appetite to be "a natural sen-

* Mr. M'Gregor (vide MED. GAZ. for May 1837) fed two diabetic patients with roast beef and water exclusively for three successive days, when he found that fermentation was produced in the contents of the stomach, and thence concludes that sugar had still been formed in it. Is it not less probable that the sugar was, in these cases, obtained from ingesta of that description, than from the gastric fluids, secreted from blood already overcharged with the saccharine elements introduced by some other means?

* Vide Mason Good's Study of Med., vol. v. 2d ed.

sation calling into its full exercise that organ through which the constant waste of the body must be directly supplied, and without which the patient must soon inevitably perish," thinks that the elements of sugar may exist in the blood, although not discoverable in substance, being "so weakly and loosely oxygenated as to be again readily evolved by the secretory action of the kidneys, not from any fault in the kidneys themselves, but from the regular and natural exercise of their functions, in separating from the imperfect blood such matters as are not properly combined with it." (p. 97.) Dr. Copland (Dictionary of Pract. Med.) says, "Diabetes may be frequently considered a remote effect in the chain of morbid causation; functional, or even structural, changes of the assimilating viscera, particularly the lungs and digestive organs, existing for many months, or even years, before the increase or saccharine state of the urine has attracted attention." At all events, the morbid condition of the kidneys assumed by some, "cannot be inflammatory, because when there are unequivocal marks of inflammation, which is seldom the case, they have been accompanied by a diminished, or an entirely suppressed, instead of a more profuse, secretion of urine." He further pertinently asks, "if the kidneys were primarily affected, how can the early disorder of other viscera be accounted for?" How is it, too, that exclusive of those cases where the disease can be traced to "concussions or injuries of the back, hard riding, &c.," or to excessive indulgence in various beverages, there are so many, in which the morbid cause manifestly did not primarily act on the kidneys? Dissection has also shown that the kidneys may to all appearance be in a healthy state (as in Dr. M'Intosh's two cases; vide Princ. of Path. and Pract. of Ph. ii., 278.) Dr. Copland says, "all the urinary organs have frequently been found natural."

I have, however, no doubt that the kidneys, from acting vicariously at first, may, after their function has been thus for a shorter or longer period excited to extraordinary efforts, become also morbidly affected, and continue their exertions, originally, like the discharges in cholera, intended by the organic agent to relieve, but after a while only

tending still further to depress the vital powers, till fatal exhaustion ensues, with or without alteration of structure. Again, it seems obvious that in a few cases, when diabetes appears after falls, strains, &c., there is primary disease of the kidneys, whether functional or organic.

Having for some time past paid considerable attention to the subject of diabetes, I feel inclined to prefer the following explanation of its phenomena:—

5. "The disease, except when caused by some local injury, is dependent upon an accumulation of carbon and oxygen, and a deficiency of nitrogen in the system, whether owing to the quality of the ingesta or atmosphere, or to a morbid condition of one or several of the organs destined to eliminate carbon, alone or in combination with oxygen, *i. e.* the lungs, the liver, and, in a majority of cases, the skin; those principles being directed to the kidneys*, which in consequence set up an increased and vicarious action; a considerable deviation of the nervous function from the normal state being, in some cases the cause, in others the necessary effect, of those morbid phenomena."

I shall now briefly state the facts and authorities which have led me to adopt this view.

"The relation," says Dr. Prout, "which exists between urea and sugar seems to explain in a satisfactory manner the phenomena of diabetes, which may be considered as a depraved secretion of sugar. The weight of the atom of sugar is just half that of the weight of the atom of urea; the absolute quantity of hydrogen in a given weight of both is equal; while the absolute quantities of carbon and oxygen in a given weight of sugar are precisely twice those of urea." The constituents of these two bodies are thus stated by that distinguished physiologist:—

(From this analysis those of other chemists do not materially differ.)

This surplus of carbon and oxygen, and deficiency of nitrogen, may be thus accounted for: I. Carbon may accumulate in the blood (as is shown to be the case by its condition and

* Dr. Prout (*loc. cit.* Introduction, p. xciv.) says, I have myself very frequently met with this [carbonic] acid in the urine.

Elements.	UREA.			SUGAR.		
	No.	Per atom.	Per cent.	No.	Per atom.	Per cent.
Hydrogen	2	2.5	6.66	1	1.25	6.66
Carbon	1	7.5	19.99	1	7.50	39.98
Oxygen	1	10.0	26.66	1	10.00	53.35
Azote	1	17.2	46.69			
	5	37.5	100.00	3	18.75	

colour when abstracted), 1. from insufficient elimination of carbonic acid gas by the lungs. Whilst in vain looking for organic changes, sufficient to account for such serious functional derangements, in the kidneys and stomach, which often present morbid appearances merely in consequence, it would seem, of undue exertion, we have overlooked those which so frequently occur in the skin and lungs. Dr. Copland (*loc. cit.*) says, "I have scarcely met with a case entirely uncomplicated with pulmonary disease." Dr. Bardsley (*Med. Reports and Exper.*) expresses himself to the same effect. In one of Dr. Warren's cases (*Trans. Coll. Phys.* 1813, vol. iv.) "the lungs were diseased; the cells containing matter." In Dr. Marcet's case (*Lond. Med. and Ph. Journ.*, vol. ii.) "a large ulcer was discovered in either lung, containing a considerable quantity of pus." In the two cases recorded by Dr. J. M'Intosh (*loc. cit.*) "the lungs were tuberculated." In a case of Dr. Elliotson's (*vide Lancet*, Feb. 18, 1837) there was "partial ulceration of the lungs." The imperfect performance of this function, then, would give rise to a large accumulation of carbon as well as oxygen in the system; for though I should think, with Prout and Turner, that Messrs. Allen and Pepys have overrated the quantity of carbon got rid of by that channel (3xj. in twenty-four hours), still it is no doubt considerable. Dr. Prout says, "Fyfe found that after swallowing intoxicating liquors, the quantity of carbonic gas formed in respiration is diminished. The same thing happens under a course of mercury, nitric acid, or vegetable diet." (To this list, anxiety of mind and other depressing passions, and, according to Prout, strong tea, might be added.) Hence additional causes of carbonic acid being retained in the system.

2. From disease of the liver, and consequent disturbance of its function of decarbonizing the blood transmitted to it. Mason Good says, "Dr. Mead having remarked that the disease is frequently to be traced amongst those who have lived intemperately, and particularly who have indulged in an excess of spirits and other fermented liquors, ascribed it to the liver, and the idea was very generally received in his day." Dr. Copland also states the liver to be more frequently diseased than the digestive organs. Dr. Prout (*loc. cit.*, p. 49) says, "A frequent complication in the earlier stages of diabetes is hepatic disorder or disease." Again (p. 54), "I have seen chronic inflammation, with congestive enlargement and jaundice, and too frequently organic disease of the liver, accompany this disease."

3. From disordered function of the skin; upon which, in fact, we find, in a majority of cases, the morbid cause (cold, perhaps certain electrical or other conditions of the atmosphere, &c.) has acted in the first place. According to Turner, the cutaneous secretion consists chiefly of water; but it contains some muriate of soda and free acetic (Prout, lactic) acid. Ure says, "Besides water and carbon, or carbonic acid gas, the skin emits an odorous (probably oily) substance. Its secretion in general reddens litmus paper."

According to Thenard, it contains a great deal of water, free acetic acid, muriate of soda, some phosphate of lime and oxyde of iron, and an inappreciable quantity of animal matter, approaching much nearer to gelatine than any other substance. Dr. Prout (*Bridgew. Treat.*, p. 524) is of opinion that "a large portion of the carbonic acid of venous blood is probably formed in the skin and analogous textures. The skin of many animals gives off

carbonic acid and absorbs oxygen; therefore its office is similar to that of the lungs: a function of the skin perfectly intelligible, on the supposition that near the surface of the body the albuminous portion of the blood is always converted into gelatine [which contains 3-4 per cent. less of carbon than albumen; *ibid.* p. 529] and that such conversion is one great source of carbonic acid in venous blood." . . . "The skin consists of little else besides gelatine." . . . "Gelatine is readily convertible into a sort of sugar, by a similar process as starch" (*ibid.* p. 473.)

A morbid state of the skin has generally been noticed in diabetic patients. Not to mention eruptions, and still slighter abnormal appearances, other more important changes are frequently observed. Dr. Prout (*loc. cit.*, p. 35) says, "I should say that diabetes usually *follows* cutaneous affections; and accompanies (perhaps *precedes*) the affections of the cellular tissue." And again (p. 36), "Diabetes very frequently (as far as my personal observation goes, *always*) accompanies carbuncles and malignant boils or abscesses allied to carbuncles. This is a fact mentioned by several of the older writers," &c. &c.

Dr. Latham, in a letter to Dr. Mason Good, says, "I have a patient at this moment, whose diabetes was first observed after a long confinement from carbuncle." (*Study of Med.*, v., 491.) He also mentions a case where the bite of a rat was followed by boils and abscesses, and general ill health; *then* by diabetes. (*Facts and Opin.*, p. 134.)

Such morbid state of the skin, or cold, moisture, or other morbid cause acting on it*, necessarily impeding its function in general, and the conversion of the albuminous portion of the blood into gelatine in particular, an accumulation of carbonic acid may justly be supposed to arise in the system, which escapes by the kidneys, where, by a process as yet hidden from us, it is converted into sugar.

It is highly probable, as I believe Dr. Prout has first suggested, that the accumulation or retention of albumen in the blood, resulting either from a particular kind of diet, or from impaired function of the skin, owing to

the nature of the climate or locality (want of sunshine, &c.), is the chief cause of scrofula, particularly in individuals born in tropical countries, whose skin is originally in a state of almost constant activity and excitement, and who, on arriving in this country, become so liable to that disease. The resemblance of scrofulous matter to curds would seem to countenance this opinion. Such an accumulation may, in the lungs, occasion the secretion of tuberculous matter. (The scrofulous diathesis is said by Dr. Copland to predispose to diabetes.)

Cases occur, in which perspiration, far from being suppressed, is often profuse. In these, on closer investigation, some other decarbonizing organ would probably be found to be diseased, and the skin to act vicariously.

II. Carbon and oxygen may also accumulate, and nitrogen become deficient in the blood, in consequence of a diet chiefly or exclusively vegetable. Starch, which is the prevailing principle in the diet of the poor, Dr. Prout (*on Alim. Subst.* vide *Phil. Trans.* 1827, P. ii.) looks upon as "sugar partly organized. Its composition varies according to moisture and temperature." According to Ure's analysis, wheat-starch, as found in commerce, consists of C. 38.55, O. 55.32, H. 6.13, in 100 parts; Sugar: C. 43.38, O. 50.33, H. 6.29. Berzelius: Starch, C. 43.5, O. 49.5, H. 7.0; Sugar (mean of three analyses), C. 43.265, O. 49.856, H. 6.879. Gay Lussac and Thenard: Starch, C. 43.55, O. 49.68, H. 6.77; Sugar, C. 42.47, O. 50.63, H. 6.90. Saussure: Starch, C. 36.29, O. 56.87, H. 6.84; Sugar of grapes, C. 36.71, O. 56.51, H. 6.78. ("100 parts of starch yield 110 of sugar." S. concluded that this sugar is merely a compound of water and starch.) Hence it is evident that the difference in the composition of these two bodies is very trivial, and the conversion of the one into the other may be merely the result of isomerism.

Turner (*El. of Chem.*, 5th Ed., p. 853) says, "Saccharine matter is developed when gelatinous starch is kept in a moist state for a long time, either with or without the access of air. If starch is boiled for a considerable time in water acidulated with one-twelfth of its weight of sulphuric acid*: it is

* Is the exemption of animals from diabetes owing to their skin being protected by the hair from injurious atmospheric, and especially electric, influences?

* It has been stated, I forget by whom, that

wholly converted into saccharine matter similar to that of grapes: this change takes place more rapidly if the temperature is a few degrees above 212." In how far the very common and (supposed) venial adulteration of bread with the super-sulphate of alumina may, in a similar manner, contribute to the formation of sugar in the digestive or other organs, it would not be easy to determine. Certain it is that one of Dr. Prout's cases (see a former edition) is that of a baker; that Dr. Elliotson has also met with several other cases among bakers; and that in populous town *cheap* bread is generally thus adulterated, and eaten by many of the poorer inhabitants, among whom the disease is most frequently met with.

Among the exciting causes of diabetes have also been classed "acidulous or fermented, particularly malt liquors, cider, &c." (Copland) "the use of acids and acidulous fluids," (Autenrieth). Now acetic acid contains C. 50.224, O. 44.147, H. 5.629; or, in other terms, C. 50.224, H. (or its elementary constituents) 46.911, with O. 2.863 in excess, (G. Lussac); C. 46.85, O. 46.82, H. 6.33, (Berz.); C. 47.05, H. 52.95, (Prout on Alimentary Substances); elements which, with very little variation in their proportions, also enter into the composition of sugar.

III. Carbon may accumulate in the system in consequence of its constant absorption from the atmosphere, which, in our populous manufacturing districts, is so abundantly charged with carbonaceous matter, especially when combined with moisture, so as to check the insensible perspiration, which, as a dry, warm atmosphere seems greedy of absorbing and abstracting from the body, so an opposite state of the air is apt to suppress and "drive in;" both elements (C. and H.) thus accumulating in the system, and assisting in the formation of sugar, as well as in increasing the quantity of urine. Dr. Carbutt (Clinical Lectures, 1834), has recorded

four cases of diabetes *insipidus*, and ten of diabetes *mellitus*, occurring at Manchester between the 19th of January, 1833, and the 10th of March, 1834—a number of cases, of a rare disease, so considerable, that there must exist a peculiar combination of local, atmospheric, or other causes, to account for its frequency. That, among others, the constant inhalation of carbonic acid gas, with which an atmosphere that supports the fires, as well as the respiration, of a crowded population, and that of their domestic animals, must become loaded, (its ready dispersion being impeded by its high specific gravity)—not to mention the electrical changes such an atmosphere probably undergoes—may not contribute towards producing the disease, few, I think, will deny.

Effects of remedies.—The foregoing opinions would seem to derive support from the beneficial effects of bleeding, (combined with animal, and abstinence from vegetable food), by which the carbonized blood is withdrawn, and a differently constituted azotized supply of it introduced instead; of opium, sulphur, hot and vapour baths, emetics, friction, warm clothing, exercise, and removal to a warm climate, from their tendency to restore the healthy function of the skin; of lime water, from its affinity for carbonic acid gas; of tonics, (iron, &c.) from their salutary action on the nervous system, &c. &c.

I now proceed to give a short abstract of the opinions and cases of various writers on this subject.

Cullen, (vide MS. Lectures, edited by Dr. Thomson, 1827), who thought that "no topical affection of the kidneys had a share in producing this disease, and that a fault in the assimilation of the fluids is rather to be blamed," observed "unusual dryness of the skin in every case; in one case it was not only dry and cold, but manifestly shrunk and shrivelled. By warm bathing it was restored to its usual moisture and softness, and by friction its heat returned; and as this took place the urine was diminished in proportion. Another patient I made go to a warmer climate. He had come from the dry rock of Gibraltar, in consequence of which he was seized with diabetes. I advised him to return: he followed my advice, was cured of the disease, and has continued perfectly

on some litmus paper having, by way of experiment, been exposed in the streets of London, acid reaction became manifest, caused, it was supposed, by sulphuric acid having been formed in the atmosphere charged with smoke containing the fumes of the sulphur so generally existing in coals. If this shall be confirmed by further experiments, I would not deny the possibility of the sulphuric acid thus absorbed from the atmosphere of populous towns, contributing, in conjunction with other causes, to the formation of sugar in the system.

well ever since; making his return to a colder country gradually."

Dr. Rollo, (*On Diabetes Mellitus*, 1797), though he believed the stomach to be the seat of the disease, reports a case (No. 2,) in which "the skin was dry and slightly hot: there was a tickling cough, and pain in the chest, always relieved by emetics. Skin improving; urine and general health likewise." He quotes a case from Richter, in which the disease was removed by an emetic. In another case, antimonials and warm baths proved palliatives. On some scorbutic symptoms appearing, the patient got *wort* for drink, and the diabetes disappeared. In a third case, ipecacuanha easily produced vomiting; and as often as this occurred, the disease disappeared for twenty-four hours. Also, a case of Dr. Gerard's, in which habitual perspiration had ceased, and cuticle had become dry, harsh, rough, and "dead" — then diabetes. After warm baths at 100° (for ten minutes each time) dead cuticle peeled off, and health improved.

In a case of Dr. Watt's (*Cases of Diabetes*, p. 222) the patient was "hide-bound." Soles of feet and palms of hands cold, and always dry. Disease came on after eating brambles, when in a violent sweat; had been in the habit of sweating profusely before disease appeared. Nitric acid, with little benefit; lime water, one quart per diem, with some benefit. Bled: skin began to soften. Pulv. Ipec. c. gr. v., every five hours. "The state of the skin deserves particular attention. It is in this quarter that the first impression of the disease is generally made, and we have no security of the system being fully restored till the skin becomes natural in its appearance and resumes its function."

Dr. James Clarke, of Nottingham, (*vide Edin. Med. and Surg. Journal*, 1810), attributes the disease to deranged function of the skin.

A patient of Dr. Latham's (*loc. cit.* p. 130), was cured by (easy) work among his fellow-labourers of the neighbourhood after the usual remedies had failed. Perspired copiously in consequence, without being weakened. "Lived on a large onion and fat bacon, and no great portion of bread."

In a case of *diabetes insipidus*, occurring in a female, æt. 50, related by Dr. Bostock, (*vide Med. Chir. Trans.* vol.

iii.) there was for some weeks before unusual scurfiness of the head: the whole surface of the body felt dry, particularly the palms of the hands; never perspired, though skin hot. No eruption except on hairy scalp. Urea rather more than four times the weight of the saline substances; daily about seven ounces and a half of urea; no sugar. Employed those remedies which were supposed to strengthen the digestive organs, and to reproduce a healthy state of the skin (preparations of iron and warm baths): cured. Has remained well for above twelve months. "It is difficult to decide whether this disease is the commencement of diabetes mellitus, or only tends to terminate in it."

From Dr. P. Warren's cases (*vide loc. cit.*) it appears that when a decided effect was produced by the opium on the skin, the urine was no longer sweet, or considerably improved in quality. On remission of opium and animal diet, urine sweet again.

Dr. Marsh (*Dublin Hospital Reports*, 1822, vol. iii. p. 430), gives the following reasons for believing the disease to be seated in the skin:—"1st. In many of the cases whose histories are recorded the earliest disturbance in the general health could distinctly be traced to some cause acting on the skin, and producing derangement of its functions. 2d. *Every* case of the diabetes mellitus is accompanied with a peculiarly morbid condition of the skin. In truth, I know not any disease in which this symptom is so uniform and so remarkable. 3d. None of the remedies employed produced the slightest beneficial effect until the skin began to relax, and a sweat to appear on the surface" In the case of — Riddall, a shoemaker, æt. 20, diabetes came on after a voyage during a violent storm. Had been four days at sea, up to his knees in water, and chilled with cold: unusual dryness of skin, and total absence of perspiration: four vapourbaths, without benefit; fifth, for twenty minutes; syncope, then feverish, &c.; soon covered with sweat; laborious exercise while wearing thick flannel; perspired freely; no medicines; better. "There are not any functions in the body more connected and interwoven, the one with the other, than those of the skin and kidneys. By both these organs substances either deleterious or useless are carried off from the animal economy. When from any

cause the secretion of one is increased, that of the other is proportionately diminished. They seem each capable of performing an action vicarious to the other; whence it would appear highly probable that very similar substances are expelled from the system by both these outlets. Diaphoretic medicines, either vegetable or mineral, may by a slight alteration in the mode of exhibition be rendered diuretic. The remark has been made that diabetes is most prevalent in England, ("Diabetes Anglicus," Sauvages),—not owing to peculiarity in diet, but atmospheric vicissitudes." . . . "There is scarcely any pulmonary exhalation." . . . "In a late case there was pulmonary disease: skin scaly and dry. Vapour baths. But the cure of a disease so obstinate and fatal ought not to be entrusted to any one single remedy; therefore bleeding, followed by baths; leeches to epigastrium occasionally; daily opening medicine; carbonated lime water; beef or veal tea, in moderation; vapour or tepid bath perseveringly every day, or twice a day. (Frequent immersion of the feet in warm water will sometimes succeed after the failure of more powerful remedies). Pulv. Ip. comp. gr. viij.—x., every third hour; residence in a warm climate. When there is amendment, and the patient regains strength, the cold bath proves an excellent cutaneous stimulus. On the patient's own resolution and perseverance much of the cure will depend. He should be particularly warned of the danger of laying aside too hastily a plan of treatment which tends to remove the symptoms of the disease, and to guard against a relapse, (by active daily exercise, &c.)"

Dr. Carbutt's cases (vide Clin. Lect. 1834), are calculated to support the opinion of the skin being primarily affected, except Case IV., where there was much sweating at night; but there were also symptoms of liver disease, (pain in right side and shoulder on inspiration; high coloured urine, and clayey fæces). Dr. Carbutt believes general, occasionally local, bleeding and vapour baths to be the most rational plan of treatment.

In a case of Dr. Lefevre's (MED. GAZ. for Nov. 29, 1834), "the skin was dry and rough. Could not excite perspiration by any of the ordinary means employed for that purpose. Pulv. Ipec.

c. gr. x. h. s. Warm bath every evening; no perspiration; then vapour bath at 45° R. (133° F.) Perspired most profusely; *urine diminished*. Diabetes hardly known in Russia. Attributes it to frequent use of vapour bath. Can find no instance of this disease recorded in the list of maladies entered in the books of the civil or military hospitals for the last twenty years. Sir J. Wylie had never seen a case among two millions of soldiers whom he had inspected during his long and useful career." (Dr. Trotter, in a letter to Dr. Rollo, inserted by the latter, loc. cit. says, "I have never known a case of diabetes among seamen, nor have any of my naval acquaintances." This exemption would seem connected with their comparative immunity from calculous disorders. Dr. Paris (Pharmacol., vol. i. p. 226, sixth edit.) asks, "Can the quantity of muriate of soda taken with their food, from its stimulating influence upon the cutaneous functions, be considered as affording a plausible explanation of this fact?" Others attribute it to their generally perspiring profusely at night).

Dr. Thomas Watson, (MED. GAZ. for Dec. 20, 1834), in reporting the case of a man, æt. 40, says: "Skin dry and harsh. Disease first came on after cold weather; during the preceding winter had been several times wet through, and had neglected to change his clothes. Bleeding; iodine and arsenic; blisters (3). Relapse from accidental wetting. Better after animal diet, and pills containing Quin. Sulph. gr. ij., Zinci Sulph. gr. i., Op. gr. ʒ. thrice a day. Relapsed once more after catching cold. Specific gravity of urine 1.036. Thirteen pints of urine; nine ditto of drink; diet not entirely animal. Pulv. Ipec. c. gr. v. thrice a day; occasional aperients; hot air bath twice a week; perspires freely; after a while urine less than drink. Found more bread than his usual small allowance to increase his urine. When left hospital, sweated easily and naturally. After eight months symptoms of phthisis, after getting his feet wet.—Kidneys presented no perceptible alteration of structure. Cavity in right lung. Rest quite solid."

Dr. Prout, in his recent most scientific, and at the same time eminently practical work on the subject, considers the use of warm clothing, frictions, warm or vapour bath, antimonials, the Dover's

powder, sulphur, camphor, &c. "an important class of remedies."

Having attended only one or two cases of the disease since I became persuaded of the above advocated being its true pathology, I have not had an opportunity of sufficiently testing it myself by pursuing a plan of treatment founded upon it. In the last case I saw, which had already reached an advanced stage, a habitual and rather profuse perspiration in the axillæ had formerly existed, which had been checked without any apparent cause, except, perhaps, a change of abode from a sheltered to a more exposed situation. Then red, afterwards white deposits in the urine; lastly, diabetes supervened. The Pulv. Ip. c. was tried, without acting on the skin, or any other beneficial effect. The warm or vapour bath, and animal diet, circumstances precluded a trial of; nor could I even prevail on the listless desponding patient to wear flannel and warm stockings. She died exhausted in less than three months. No inspection took place.

It was also my intention to have tried the nitrous oxyde gas, with a view to an artificial supply of nitrogen, as well as on account of its stimulating effect on the nervous system, but having had no opportunity of doing so, I would recommend both it and emetics, which cause so powerful a determination to the surface, to the attention of others, as likely to prove of service.

FUNGUS HÆMATODES.

To the Editor of the Medical Gazette.

SIR,

IF the following case, which I have recently attended with Mr. Bateman, of Islington, possesses sufficient interest for publication in your journal, I shall feel obliged by your inserting it.

I am, sir,

Your obedient servant,

T. ABRAHAM, M.R.C.S.L.

49, Old Broad Street,
Aug. 21, 1841.

A young lady, aged 20, in the autumn of last year, hurt her knee by a fall, but did not feel much of it at the time, and continued to walk without much inconvenience for about six weeks afterwards, when the joint became very

painful on being moved or pressed upon. It gradually enlarged all round, but was not discoloured. Darting and lancinating pains were at length felt in the joint and lower half of the femur, which much harassed and distressed her day and night. In this state I found her on the 15th of January last, when requested to attend the case with Mr. Bateman, who had been previously assisted by Sir Benjamin Brodie.

It is not my intention to occupy your space in detailing the treatment; suffice it to say, that the pains were mitigated in a few days, but every attempt to cure or suspend the progress of the disease proved useless. The swelling on each side and in front of the joint daily increased, presenting a soft and elastic feel, with an obscure sense of fluctuation. Mr. Bateman and I now considered that, as the disease was progressing, but confined to the limb, and the patient's health rapidly giving way, amputation was the only means of checking it: in this opinion we were afterwards corroborated by Sir B. Brodie.

On Feb. the 27th amputation was performed about two-thirds up the femur; this being rendered necessary by the extensive disease of that bone. On laying open the joint after amputation, a large hæmatoid tumor, or substance resembling coagulated blood intersected by cellular strata, was found in it, and the lower half of the femur carious, in which most probably the tumor originated. In about two months the stump was healed, and the patient gradually improved in health, so as to be able for a few weeks to take carriage exercise, and visit her friends. In the beginning of July, however, she was very ill, and it was discovered that effusion into the left side of the chest had taken place. On the 8th instant she expired.

A post-mortem examination having been permitted by her friends has set at rest any doubt that may previously have been entertained as to the nature of the complaint. My friend Mr. Blyth (Mr. Bateman being from home) and I found about two quarts of serum in the left side of the chest, extensive pleuritic adhesions, the whole of the left lung converted into an encephaloid mass, weighing about two pounds, of a reddish white hue, more consistent than brain, and greasy to the touch. The heart (forced into the right side of the

chest) was small and pale; the parietes thin, the valves sound: the right lung was much compressed, and thickly studded with calcareous deposits. No disease was found in the abdomen.

The above statement is forwarded for publication as additional evidence (if any be wanted) of fungus hæmatodes, or medullary sarcoma, being dependent on a morbid condition of the blood, and of its re-production and rapid growth in another part after it had been removed from its primary seat.

IODIDE OF POTASSIUM.

To the Editor of the Medical Gazette.

SIR,

THE perusal of the article "Iodide of Potassium," in Pereira's *Materia Medica*, brings to my recollection a case which occurred in my practice about three years ago. I ordered a gentleman three grains of iodide of potassium in a draught of peppermint water, three times a day. When he had taken the medicine three times he felt poorly; and in the course of an hour after the fourth dose he was attacked with a violent shivering fit, followed by intense headache, heat of skin, constant thirst, quick and very full pulse, and vomiting and purging at the same time. These symptoms were succeeded by great prostration of strength. Notwithstanding the exhibition of demulcents and opiates, the purging lasted for several days.

The effects of the medicine in this case were so violent that I have little doubt, if he had taken another dose, his life would have been forfeited. This is the only instance, which I have seen, of the iodide of potassium producing unpleasant effects in doses under ten grains.—I remain, sir,

Your obedient servant,

JAMES C. L. CARSON, M.D.

Coleraine, Aug. 19, 1841.

THE FLEXIBLE STETHOSCOPE.

To the Editor of the Medical Gazette.

SIR,

I HAVE been well pleased to see the able communications on the Flexible Stethoscope from Drs. Burne and Bird, in the pages of your journal. I cannot but

lament that a correspondence so interesting and valuable to the profession should degenerate into personal attack and mutual recrimination. However, this good effect has been answered: each controversialist has dismantled the other of any lurking pretension he might have to the introduction of the instrument in question, and has attributed it, correctly, to my estimable friend Dr. Stroud.

In the summer of 1838, I employed a flexible stethoscope, consisting of two feet of the ordinary elastic flexible tubing, and a small funnel-shaped extremity of wood, in some experiments I made to satisfy myself as to the cause of the various sounds occurring during the heart's action. This instrument conducted the sounds more distinctly, and was much cheaper, than the flexible ear trumpet: being devoid of an ear piece, it remained in the ear without being held there; thus leaving one hand at liberty to feel the pulse. Since that time I have constantly employed the flexible in preference to the solid stethoscope. I am informed by Mr. Thompson, instrument-maker in this town, that he has sold about a hundred instruments similar to that described above to medical practitioners residing in this town and neighbourhood.

I beg to corroborate Dr. Bird and Dr. Burne's statements as to the advantages possessed by the flexible stethoscope. In addition I feel desirous of calling the attention of the profession to the perfect ease with which pressure may be modified in using this instrument.

In simple pericarditis, in the early stage, on applying the extremity of the stethoscope lightly, the natural sounds of the heart are heard; on increasing the pressure they become replaced by the friction sound. Up to a certain point the greater the degree of pressure the louder is the friction sound. In the more advanced stages of pericarditis, when the pericardial surfaces are much roughened immediately over the apex of the heart and the lower part of the sternum, the friction sound is heard feebly on light pressure; but at a short distance from these points the natural sounds alone are distinguished; when pressure is exerted and increased, the friction sounds come into play. When pericarditis and endocarditis coexist,

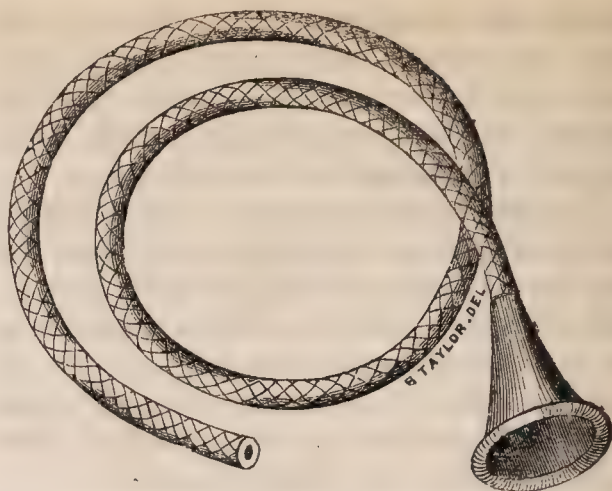
if the inflammation of the lining membrane of the aorta be sufficient to modify the natural sounds, the bellows murmur is heard, on light pressure, over the centre of the sternum : on increasing the pressure the friction sound replaces it.

In adhesion of the two pericardial surfaces, with general enlargement of the heart, accompanied, as is often the case, by regurgitation from the left ventricle during its systole, through the auriculo-ventricular opening into the corresponding auricle, owing to imperfect closure of the mitral valve, the noise from the shock of the walls of the heart against the parietes of the chest may be heard over the apex; followed by the dull rumbling sound of muscular contraction, ending in an abrupt loud sound, caused, I conceive, by the sudden withdrawal of the hardened walls of the heart from the thoracic parietes. Near the seventh or eighth dorsal vertebra on the left side a bellows murmur is heard during the first sound, caused by the auriculo-ventricular regurgitation: increase of pressure does not modify this sound. On applying the stethoscope slightly over the region intermediate to this situation and the apex of the heart, the bellows murmur is heard; on making strong pressure it is replaced by the various sounds heard over the apex. In other cases of diseased heart, varying the pressure modifies the abnormal sounds.

In the various murmurs produced by pressure over the veins in the neck, over the uterus, liver, and abdominal tumors, the ease with which the degree of pressure may be modified by the flexible stethoscope greatly facilitates the production of the sounds.

In those cases of diseased lungs, where the vocal resonance communicated from the thoracic parietes is altered, the flexible stethoscope affords great facility in making a rapid comparative survey of the chest. The vocal resonance is much more extensively heard on light than on firm pressure.

With your permission I hope to follow this short statement of the advantages to be derived from modifying the pressure of the stethoscope in the diagnosis of the diseases of the heart and lungs, by further communications containing a more detailed account, with illustrative



cases, of the points which have been mentioned in the foregoing remarks.

I am, sir,

Your obedient servant,

FRANCIS SIBSON,
Resident Surgeon, General
Hospital, near Nottingham.

Aug. 13, 1841.

MEDICAL REFORM.

To the Editor of the Medical Gazette.

SIR,

IN my recent answer to Dr. R. Hull's paper against Medical Reform, I merely proceeded upon the rational argument for its prosecution, but having now before me a pamphlet on that subject by Dr. Kidd, Regius Professor of Medicine in the University of Oxford, I am tempted to resume my pen, in order to shew Dr. Hull that not only the general practitioners are advocates of medical reform, but also that the Regius Professor of the *most* conservative University of Oxford, a conservative himself, is on the same side of the question. I am moreover tempted to do this because I do not remember to have seen any remarks on this very remarkable pamphlet in your journal. I shall endeavour to give a brief analysis of its contents.

Dr. Kidd's opening sentence is rather curious:—"Though," says he, "the cause of reform in general has probably been more injured by its professed friends than by its opponents, yet it is, nevertheless, true, and many national as well as corporate statutes acknowledge and act upon the principle, that 'genuine reform is genuine conservatism.'" The principle here enunciated is in itself undoubtedly true—genuine reform is undoubtedly genuine conservatism; yet some limitation even of this principle must be made in order to

render it generally applicable. True reform must, indeed, be adapted to the state of society in which it is to take place: it must be tardy in its progress, must be carefully weighed as to its ultimate effects, and not the result of the passions of a multitude, to which the statesman is forced to yield. Careful investigation of grievances must precede their prudent removal: this requires time: but how much more time does it require to frame new laws adapted to the new circumstances of society!

Dr. Kidd then says, that he "hopes that his loyalty to some of those institutions of which he has the honour of being a member, and which are partially implicated in the present question, will remain unimpeached for the future, as he trusts it ever has been heretofore." This requires scarcely a remark: the Regius Professor of Medicine, the holder of one or more Professorships of Anatomy, and the Librarian of the Radcliffe Library, is not, cannot be, one who would wish for sudden change.

He then proceeds to state, that from his connexion with the College of Physicians and the University of Oxford, he has received many marks of attention which would not perhaps have otherwise fallen to his lot; and he is still more grateful to the University for having within the last few years reformed the statutes relating to medical examinations so effectually, as to remove the objections which were previously, with justice, made to them." They did, indeed, require reform, since almost any one with the slightest possible modicum of medical knowledge could, without the slightest difficulty, pass those examinations: in short, they were a mere farce.

The next subject to which Dr. Kidd draws attention, in order to shew "the true character of that strong desire for medical reform, which is at present very extensively diffused among the members of the medical profession in this kingdom," is the history of the College of Physicians. It is unnecessary for my present purpose to state how the College was at first constituted; how candidates were, before its formation, examined by the bishops, assisted by doctors in physic, and others expert in the faculty of surgery; how that at first the College consisted of foreigners, or members of foreign universities;

how that all were at first "Socii," or "Collegiæ," and all on an equality; how that the privilege of making by-laws was granted to the College, and from that sprang the distinction between Fellows and Licentiates, and the gradual progress of exclusion, until the removal of a licentiate to the fellowship became a great rarity; how the licentiates feeling aggrieved, repeatedly applied to the Court of King's Bench, but without gaining redress; and, lastly, how the alteration in the statutes of the University of Oxford were at last reformed, on the recommendation of the late Lord Stowell.

Having disposed of the history of the College of Physicians, which is well known to almost every medical man, in consequence of the repeated examinations the subject has undergone, both by the friends and foes of the College, Dr. Kidd states, that there are no less than nineteen different sources of medical degrees and sciences in the United Kingdom, of which seventeen are active, and that many even of these are incapable of granting legal protection to their graduates; and cites the London Apothecaries' Company, which affords very imperfect protection; the English Universities, no protection; the College of Physicians, whose actions at law against unlicensed practitioners are a dead letter, and most easily evaded; and lastly, the College of Surgeons, which has no means of protecting its members, or even of punishing those who without its diploma call themselves "surgeons."

"The question of medical reform," says Dr. Kidd, "like all other questions which involve the contemplation of changes in existing customs and institutions, requires very calm consideration, as well with regard to its original expediency as to its requisite extent.

"If the united voice of nearly all the individuals who constitute the medical profession may be admitted as a just indication of the necessity for reform, nothing more need be said of that necessity; *for that united voice is already raised in favour of the measure.* But there still remains to be discovered the best means of effecting such salutary changes as may with the least inconvenience remedy the several evils which require correction." What do you say to this, Dr. Hull? Surely you do

not intend to call us all radicals. If so, you include, as we shall presently see, the Regius Professor of conservative Oxford.

Again, "it is equally as noble in communities, as individuals, to acknowledge and amend an error, as it is ignoble to persist in it; and it must be admitted by every *unprejudiced* inquirer that, had all the existing institutions of the medical profession adhered to the spirit of their original foundations, many of the present evils would have been prevented."

I am now obliged to make some rather long extracts from Dr. Kidd's pamphlet, in order to elucidate his plan of medical reform, which I may say is of a most moderate character—at the same time strikes at the root of the present abuses, and yields all that can rationally be required, but which cannot coincide with the arrogant pretensions of Dr. Hull.

"It can hardly be denied that a change is desirable in that part of the existing system above described, which refers to the irregularity of the standard of qualification expected by the different boards of examination for license to practise. But since such change would require the establishment of one national board of examination, which alone should have the power of granting license to practise, it is in the highest degree probable that to such a change every existing board would be at first disposed to object as an annihilation of its own power. And yet, if in the proposed new national board, each existing board were to be represented by one or more of its members, among others chosen from the profession at large, and if a certain number of the whole were in succession delegated to act as examiners in each session, the objection might probably be removed.

"With respect to the future condition of existing institutions, there appears nothing in the least derogatory to their dignity; nor, which is of much more consequence, to their professional utility and efficiency, in considering them henceforth as independent scientific societies; which, retaining all their present members and internal laws, might henceforward admit new members, as customarily admitted to the Royal, Linnean, and other scientific societies. Each of the existing institutions might still have its own library

and museum, and its own lectures; each might have its own meetings, and publish its own Transactions; and all vying with each other in the spirit of a liberal emulation, might continue to benefit both individuals and the public, quite as effectually as under the present system."

"To the demand that of every candidate for a medical license, such an examination should be required as might show a due qualification to practise either as a 'physician,' or a 'surgeon,' or as a 'general practitioner,' there does not seem any reasonable objection; *nay it is quite expedient that every candidate should be so qualified.* For, although it is true, as is observed in a very judicious and candid article on Medical Reform, in the 'Quarterly Review' of December 1840, that very few physicians practise surgery in this country, yet, as might have been justly added, had it occurred to the writer of that article, *it is also true that every physician ought to be acquainted with the principles of surgical practice.* And it is equally true, as indeed is implied in the same article, that, even in London, all surgeons must necessarily act as physicians in very many cases of their specific practice; while, out of London, except in the largest provincial towns, no surgeon could gain a competence were he to confine himself to a practice purely surgical. Hence, therefore, it follows that every candidate should be qualified for all three branches of medical practice; since this being granted in the case of the 'physician' and 'surgeon,' it must necessarily be granted in the case of the 'general practitioner,' inasmuch as he combines in his practice the functions of the other two."

This is the cream of Dr. Kidd's little book; it at once corroborates the views put forth in my last letter to your journal, and, as is evident, it overturns Dr. Hull's favourite aphorism, that the general practitioners are above the source of the clamour raised for medical reform, since not only a physician, but even a Professor of the oldest university in the kingdom, writes in favour of these views. I must make one further extract, which will require some comment. "I know not whether I am correct in supposing that there are some among the advocates of Medical Reform who are desirous of making the

proposed national board the source, not only of medical licenses, but of medical titles also; nor do I know how such a regulation would be regarded by the members of the profession at large. The obvious objection to the measure is its apparent inutility: for, provided the board can give the license to practise in any or in all the three branches, into which the profession is actually, and always must be divided, it would appear that the essential point is gained, as far as individuals are concerned: and, on the other hand, no corporate bodies or communities need be jealous that the power of granting titles or degrees should be extended to one additional source of such titles, further than from the motive of a consequent diminution of their pecuniary revenues—a motive, which, at the present day, cannot be supposed to influence the members of a liberal institution in their collective capacity.”

Here I close my extracts from Dr. Kidd's exceedingly well-written and gentlemanly pamphlet; one which shows great liberality of feeling on his part towards that branch of the profession which Dr. Hull treats with contumely and contempt unbecoming his situation. But I may remark that these extracts show that, supposing Dr. Kidd's plan worked out, the distinction between physician, surgeon, and general practitioner, must soon cease, since every one who possessed the license would meet another who possessed a degree from some other source on a perfect equality, as the possession of the license would mark his perfect capability to practise as a physician. But even supposing things to go on as they were, and the distinctions to subsist, as Dr. Kidd affirms they would, the physician who practised as a physician alone could not refuse to meet another who equally possessed a degree, and assumed the title, but practised as a general practitioner, on the plea that it was not gentlemanly to practise thus, and not drop the degree; because the other might retort upon him that he likewise possessed the license which enabled him to practise as a physician when he chose to do so; but that, from accidental circumstances of locality, he was compelled to practise surgery and dispense his medicines.

In conclusion, I ardently anticipate and desire that form of medical reform

which Dr. Kidd has so ably advocated, and whose authority so evidently bears out the statements and arguments I lately made and used, so as completely to overturn Dr. Hull's ill-judged assertions.—I am, sir,

Your obedient servant,

PHILIP B. AYRES, M.B. Lond.
M.R.C.S.L.

ON THE PROPAGATION OF THE VARIOLÆ VACCINÆ BY CRUSTS FROM THE COW.

To the Editor of the Medical Gazette.

SIR,

THE propagation of the vaccine disease from the crusts found on the animal has been attempted in India, and I believe in other countries. I am not aware that the practice has been successfully followed in England. This circumstance induces me to trouble you with the following details. It has of late been very difficult in this country to detect the disease on the cow in a state capable of affording lymph in its pure and active condition: it is therefore a point of some interest and importance to be assured that the crusts may be found to contain the virus in a degree of purity and activity sufficient to impregnate the human constitution.

On the 7th of this month my friend Mr. Coles informed me that he had heard of the existence of the variolæ vaccinæ at a farm a short distance from this place. The following morning we visited the cows. One only had been affected by the complaint. The teats were covered with the remains of the disease, but not a single vesicle existed from which lymph could be taken. Under these circumstances I recommended Mr. Coles to collect a few crusts. This was done, and on the 10th of this month he triturated two of them with cold pure water, having previously separated the margin and outer layers of each crust, and with the fluid thus obtained he inoculated six children, with three punctures on each arm. Out of these thirty-six punctures only one took effect. The vesicle formed in this instance was rather small, but very perfect in its form and character; so much so that no hesitation was felt about the propriety of attempting to propagate the disease from it. Mr.

Coles accordingly used it on *two* children on the 17th, and *one* on the 19th, all of which succeeded. From one of these patients, vaccinated on the 17th, he vaccinated four on the 24th; and from that vaccinated on the 19th, he charged about forty points on the 26th. All the vesicles were very fine and perfect, with the exception of those on one of the children vaccinated on the 17th. The deviation in this instance clearly arose from the coexistence of an affection of the skin, which manifested itself the day after the lymph was inserted, and so completely altered the character of the vesicle as to render it unfit for use. I am the more induced to dwell for a moment on this latter event as I have reason to know that the evils arising from inattention to deviations of this kind are not sufficiently regarded by gentlemen employed by the Guardians in vaccinating the poor. I myself, at least, have had communications from two Unions on this very point. It cannot therefore be too strongly stated that persons vaccinated under such circumstances ought not to be considered as secure from subsequent attacks of small-pox.

I am, sir,

Your obedient servant,

JOHN BARON, M.D. F.R.S.

Cheltenham, August 30th, 1841.

MEDICAL GAZETTE.

Friday, September 3, 1841.

"Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO:

THE STUDY OF INSANITY.

ALTHOUGH it be quite true that, as we said last week, all that will be ever accomplished for the advancement of the real and beneficial knowledge of insanity, will proceed from the authorized medical practitioners who study particularly this branch of our common science, yet there can be no doubt that progress in this department is lamentably delayed by the difficulties

which lie in the way of any one who would truly qualify himself for undertaking the charge of insane patients. For this purpose, more than for the treatment of any other class of special diseases, a special education is necessary; and yet, in this country, it is scarcely to be had in an efficient manner at any expense of labour or of money.

For the study of diseases of the eye, although much of ophthalmic surgery is included in a knowledge of general principles, our several Eye Infirmaries afford a most ample field—a field to which, perhaps, none in Europe is superior; and for the diseases of the Ear, or any other organ, if there be no good special institutions, yet it is never difficult, in any general dispensary, to obtain a number of patients amply sufficient for profitable study. But with cases of Insanity it is far otherwise; they can be attended and studied only in their appropriate asylums; and these are either altogether closed against students; or are opened in so restricted a manner that no real study can be carried on in them: so that insanity, which requires more than any other medical subject a special study, and a knowledge of which is communicated in the least possible degree by the ordinary instruction in medicine, is just that for which the least facility for special study is afforded. The consequence is, that the majority of those who are called to the charge of the insane, gain their knowledge of the peculiarities of the disease of madness, either by study on the continent, or (which is more common) by an unprefaced study of their own patients, and a severe tax upon the well being of the first that come under their care.

This is the more to be lamented, because the care of the insane is almost exclusively entrusted to those who

profess to treat only them. It is obviously impossible for the whole body of students to be instructed to any profitable extent in the nature and treatment of insanity; they cannot all see patients, and the few lectures that can be devoted to the subject are practically almost useless. The result is, as Mr. Tuke, in the work to which we last week referred, says, "that it is rare to meet with a general practitioner" (that is, we presume, any one who is not exclusively a mental physician) "who can undertake with any right confidence the treatment of the insane." This being true, it follows that those who do take charge of them ought from the first, to be thoroughly competent for the undertaking. A monopoly being granted to them, they ought to be well fitted to hold it; but they cannot be so unless they are provided with opportunities for study.

There is but one mode of remedying this evil: all public lunatic asylums should be open during a certain portion of the day to medical students, or to those who have already taken some diploma, on the payment of a certain moderate sum; and no one should be deemed eligible to medical offices in such asylums who had not been engaged to some extent in the special study of insanity. By this simple measure the public would be insured a sufficient supply of practitioners competent to treat this malady; for the attendance at the asylums would not be limited to those who intend to treat insanity exclusively, any more than the attendants upon eye infirmaries are only those who propose to practise nothing but ophthalmic surgery; and there would besides, be a certainty of obtaining a competent person to take charge of any public asylum, and to instruct those who might attend it as pupils.

The benefit would not stop here.

There can be no doubt that the best stimulus to induce a medical officer of a public establishment to do his duty to the patients, and to endeavour, by every exertion, to increase his own power of doing good, is the constant watching of some intelligent medical students. They may often regard his unavoidable mistakes as faults of ignorance, and his caution as indolence; but this matters little: the more hypercritical they are, the more will he find it necessary to be on his guard, and to be able to justify every one of his proceedings, and so the more would he benefit both the patients and his pupils.

The presence of students would also exercise imperceptibly a good influence upon the conduct of inferior attendants. We have no fear of the evils of the secret system of managing asylums returning in these times; yet it cannot be denied that errors both of judgment and in temper are constantly committed by keepers, which the most vigilant superintendence of visitors, directors, and medical officers, can never wholly prevent. These, however, would cease in the presence of students, who, being nearer to the keepers, and more constantly among them, would far better know their plans of proceeding: just as, in general hospitals, they know much more of the doings of nurses and others than do either the governors or medical officers.

The only seeming objection to the opening of asylums to medical students is unimportant and unfounded; but were it a sound one, it would be of less moment than the advantages of exciting a greater search into the true pathology of insanity, and of communicating widely the knowledge so obtained. It is supposed that pupils could not be admitted without disadvantage to many of the curable patients. The same objection is always made to their ad-

mission into general hospitals, by those who are not aware that it is, instead of a disadvantage, in every respect a great boon to the patients. And we do not see why it should not prove equally so in the case of asylums. If it be true that the admission of a large number of students would be inconvenient, there is no danger that any great number would attend. Not more than one student in twenty of all that are in London attends the eye infirmaries. A far smaller proportion, probably not more than one in fifty, would habitually visit the asylums ; and even this number might, if it were found necessary, be diminished, by limiting the right of attendance to those who have finished their general medical education.

IS MEDICINE ENTITLED TO RANK AS A BRANCH OF SCIENCE ?

OUR scientific readers will start at this being made a question: yet it is a question which seems to have been not only made, but practically answered in the negative, by the recent proceedings of the British Association for the Advancement of Science. At their late meeting at Plymouth, a medical section was formed, with Dr. Roget for its president; who, from his having held that office at two former meetings, must have been well acquainted with the best mode of promoting and transacting the business of the section. Yet, whilst other sections met and were in full operation, we find that the medical department was a blank during the first three days of the meeting. "Section E. Thursday, *nil*. Friday, *nil*. Saturday, *nil* !" Thus report the newspapers. On the following Monday and Tuesday, the section did meet, when some communications were read and discussed; and thus ended the diphemeral existence of the medical section of 1841.

And what prospect do we find for the

ensuing year? Let us see what "sinews" have been supplied for future exertions. We copy from the *Times* the sums granted to the several sections.

To Section A. (Mathematics & general Physics.)	£1433	18	6
To Section B. (Chemistry and Mineralogy.) . . .	215	0	0
To Section C. (Geology and Geography.) . . .	605	0	0
To Section D. (Zoology and Botany.)	279	0	0
To Section E. (Medical Science.)	0	0	0
To Section F. (Statistics.)	150	0	0
To Section G. (Mechanical Science)	350	0	0
<hr/>			
Total,	3032	18	6

So the British Association has made no provision for the medical section of 1842. And not only this, but it has refused to make any. A correspondent informs us that the Committee of the Section at Plymouth unanimously recommended that the sum of £25 should be placed at the disposal of a committee, to defray the expenses of experiments on the action of medicines. Yet this grant, so small, and for so important an object, was refused; whilst thousands were voted for various purposes of other sections, many of them laudable enough, but none of more certain and extensive utility than that contemplated in the refused grant.

When the British Association was first formed, medicine had its representatives among its founders; and it was urged by these that many departments of medical science might derive much benefit from the plan of co-operative and concentrative investigation which was to be the distinguishing feature of this Association. How far these anticipations have been realised may best be answered by referring to the reports of the medical section published in the Transactions of the Association. The value of most of these reports cannot be questioned, and may

be adduced as a sufficient proof of the claim of medicine to partake in the proceedings of the Association.

It is true that at some of the sectional meetings, communications have been read and observations made during the discussions, that were creditable neither to their authors nor to those who applauded them: but the same remark is abundantly applicable to the proceedings of other sections; and only proves a want of due vigilance on the part of the committees, whose duty it is to take care that the communications, as well as the discussions, shall be worthy of a scientific body.

It becomes, then, a matter for serious consideration whether the medical members of the British Association ought to acquiesce in this virtual exclusion from their share in the resources and proceedings of the body which they contribute to support. It is a fact that a large proportion of the members of the Association are medical men. These, by their influence and exertions, as well as by their subscriptions, have materially aided in establishing and maintaining its prosperity; and it is for them now to consider whether they will submit to be the humble providers for the mathematicians, the meteorologists, the geologists, &c., of other sections, or whether they will assert the claims of their own section to a due share in the funds of the Association. Be it remembered that the appropriation of these funds to the purposes of science is the most distinguishing characteristic of this Association, and if the medical section do not get the benefit of this, its connection with the Association becomes merely nominal.

We do not advise our professional brethren to withdraw from the Association: this would be to abandon the ground which they have a right to maintain. As with other departments of science, so with regard to medicine,

the plan of the British Association is capable of aiding in its advancement by promoting co-operation for given objects, and by defraying the expense of experiments,—means offered by no other society or institution.

But we do exhort the medical members of the Association to claim what is their due of these advantages, and thus to maintain the rights of their section; and if this cannot be effected by the influence of those medical men who are now members of the Council, let others, who are members of the general committee, move that a due proportion of the contributions of those who subscribe for the sake of attending the medical section, be devoted to the purposes of that section.

SUBSTANCE OF A CLINICAL LECTURE

ON

CHELOID TUMORS,

Given at St. George's Hospital, May 25th,

BY MR. CÆSAR HAWKINS.

I HAVE recently brought under your notice the subject of cancer of cicatrices, and I have been asked by several gentlemen whether two other cases of disease of the skin, now in the hospital, are also of a cancerous nature. In fact, however, they are examples of a rare disease, of a perfectly different character from cancer, innocent in its nature, but very curious in many points, and, moreover, generally incurable. It was described, I believe, first by Alibert, under the name of *cancroides*, in some of his works; but the best description of it is by Rayer, in his admirable work on the Skin, who has adopted the Greek term, *cheloid tumor*, from $\chi\eta\lambda\eta$; both names having the same meaning, and arising from little prolongations of the tumor, like the crab's claws.

The term *cancroid tumor* seems very objectionable, however, as it would mislead you with regard to the nature of the disease, since we already have the name of cancer, signifying the existence of a similar fancied resemblance, although, as I have said, the two diseases are perfectly dissimilar in their nature. The difficulty of all verbal descriptions is shewn by this very term, as it has

led to a singular collection of cases under this name, by Dr. Warren, in his book on Tumors, which I see in your hands occasionally,—this gentleman, having grouped together, under the name of cancroïd tumor, not only the disease that Alibert meant by it, but also the common spider nævus, and cases of fungus hæmatodes with extensive disease of the glands!

In fact, the irregular prolongations in question are by no means universal,—perhaps even not so common as the oval or quadrangular figure in the two cases in the hospital at present. I have seen it in a marked degree in one case, a little child, five years old, who had suffered from small-pox, and some weeks afterwards two or three of the cicatrices became affected with some new action, with much heat and pain; in consequence of which a reddish growth formed in each place in the skin, rather more than a quarter of an inch high, the centre being perhaps three-quarters of an inch broad, with three or four prolongations from it of about half an inch more, each gradually ending in the healthy skin. This was an example of the disease in a more active state, with much more vascularity of the new growth than in the two cases before us, which are of the more common chronic character.

The term cheloid tumor means exactly the same thing as the cancroïd growth of Alibert; nevertheless it does not seem so objectionable as the other, as the difference of sound from the term cancer previously in use is less likely to lead to confusion by the different morbid growths being confounded together.

One of the cases now in the hospital is that of Mary Moody, æt. 25, who was admitted under my care last week, May 19th. This young woman has several flat elevations of the skin of the left breast, which altogether cover nearly all the upper half of the gland: they are painful, especially when handled, and the whole surface of the mamma is tender when touched. The tumors are somewhat oval in shape, and from half an inch to an inch and a half in diameter; and there are five or six of them. She says they first made their appearance two years ago as “little knobs” in the skin, which after a time ulcerated, and since that time they have occasionally ulcerated slightly, the last time being three or four months ago. They are of a whiter colour than the rest of the skin, and firmer in texture, and are elevated about a line only above the general surface. The pain in these tumors is such as to keep her awake at night. Her countenance is pale, but she appears to be otherwise in good health. Catamenia regular, but profuse in quantity, and the pain is generally lessened at the

time of their appearance. She is very subject to hysterical fits, and her whole manner is nervous.—Ordered a sedative lotion of Goulard and laudanum, shower-bath daily, and a mixture of decoction of aloes and spirit of ammonia and assafoetida three times daily.

The other case now under our notice is a patient of Mr. Cutler’s, of whom I have received the following notes:—Thomas Cradock, æt. 21, was admitted on the 12th of the present month, with a large tumor, of a circular or quadrangular shape, situated about the centre of the sternum: it is somewhat larger than a crown-piece, and apparently is only formed in the skin of the part. It is elevated about a quarter of an inch from the surface, and it has an abrupt margin; it is of a somewhat reddish colour, some parts, however, being whiter than the rest, and the surface is a little corrugated, and irregularly depressed, though nearly flat over the whole. There is another smaller tumor near it, of similar nature, forming an oval prominence, half an inch in length, with the skin wrinkled over it. He complains of much shooting pain in these parts. He states that the larger of these tumors has existed for about ten years, and that he received a kick from a horse on the part a short time before it formed. He has had it twice removed, the first time by ligature and afterwards by caustic, but it never was so large as it is at present. You saw them removed last week (on the 20th) by Mr. Cutler, by an incision including an oval piece of skin with both tumors, and the edges were then brought together by suture. The appearance, on a section of the part, is before you.

Some gentlemen present will perhaps remember another case, that of a little girl, who was operated on by Mr. Babington, about two years ago, I think. She was about 12 years of age, and had been burnt on the abdomen when young. A year before her admission, two points of skin, about four inches apart, began to grow in the cicatrix, and formed ultimately two tumors, about an inch and a quarter long and half an inch wide: they were smooth, and nearly of the natural colour, or a little redder, and elevated about three lines; and they felt very hot and tender to the patient. Mr. Babington removed them by incision. Whether they returned again I know not; but it is not always quite fair to presume that a disease has not reappeared when the patient has not presented himself again to notice.

Rayer says, the cheloid tumor does not ulcerate; but Alibert’s observations were different; and you see in my patient that there has been superficial ulceration, though it is now healed; and, in another case, I have

known the ulceration to remain for a great length of time. This was in a woman, 30 years of age, who was sent up to me from a distance in the country, with very extensive ulceration over the skin of one breast, and the chest above it, in a considerable number of what I believe to have been these flat cutaneous tumors. They had begun eighteen months before I saw her, after a blow, and an issue had been made for them, the ulceration having spread over them afterwards. I saw this woman last autumn in the country, nearly three years after she was in the hospital, and although much better, there was still some ulceration, and several tumors now cicatrized. This woman had the same hysterical constitution and acute sensibility of skin that my present patient possesses. She suffered such intense pain that she begged to have the breast amputated, which I did not choose to do; indeed, the disease had spread to a considerable distance beyond it. The irritation of the ulcers had at that time caused some enlargement of the axillary glands; but, although supposed by herself to be cancer, the disease had no one point of resemblance, except the intense pain. The ulcers themselves were much more like the raw surface of ulcerated condylomata.

Such, then, is the history of five cases of this disease, which I have seen, and I think there were some others previously which I did not then distinctly recognise: some of you have also seen three instances, and all of you two cases of this rare disease; of which Alibert only appears to have seen eight cases, when he wrote his description of it, to which Rayer has added five more, which he had witnessed; and, to make the subject clear to you, I will next give you a short general account of the cheloid tumor, including some points of their observation, as well as my own.

The *appearance* of the tumor is that of a simple elevation of the skin, of a pale red colour, sometimes more vivid than in either of the two cases you have seen; and the vessels on the surface are somewhat larger than in the adjoining skin, and by their alteration, from heat or excitement of any kind, the appearance of the tumor varies from time to time. You may see some white lines running across the elevation, giving it a shrivelled aspect; and if it has diminished in size, the whole growth looks not unlike a tough cicatrix, as in this plate of Rayer's, in which the tumor arose in a scar. The tumor is fixed and firm, and resisting to the touch, especially if it is on the increase; the surface is flat, or a little depressed in the centre, if stationary, and more elevated and round if it is increasing; and if there are more than one, you may often perceive one elevated and smooth, while another is

wrinkled; or opposite ends of the same tumor may present these different appearances.

The *shape* of the cheloid tumor may be round or oval, or quadrangular, or very irregular; and the prolongations from the body or angles of the tumor, from which it derives its name, two or three perhaps in number, are very irregular, being straight or curved, and gradually ending in a point, or ceasing more abruptly with a smooth round extremity. These claws, or branches, sometimes lie under a considerable portion of the thickness of the cutis, having the appearance of a guinea-worm or other solid body under the skin, giving you the appearance you saw in the smaller and more recent of these two which Mr. Cutler excised; and the epidermis in this state of tumor may be in little scales on the surface.

Their *number* varies, being single, or double, or perhaps there are several in the same person; if there are one or two they are sometimes quite distinct, though near each other, as in Mr. Cutler's case; or, if numerous, they may be partly confluent, as in my own patient; and they may be several inches apart, as in Mr. Babington's little girl.

The *symptoms* they produce vary from a sense of heat or itching to intense pain, which is described as of a burning kind; and this may be so great as quite to affect the health, especially if there is the additional hysterical pain of the skin, as in both my patients, in whom the breast was the seat of the disorder; in the present one you find frequent fits of convulsions, and excessive tenderness both of the tumors and around them. This pain is made worse by increase of the circulation, as by exercise, or friction, or by agitation of mind, or by menstruation commonly; but, in our present female patient, it is lessened at those periods. In the young man the pain is not near so great; and sometimes the tumor will be quite indolent, so as to occasion hardly any pain whatever.

The *sex* of the patients is, according to Alibert, generally female, and hence he calls the disease a disorder of the absorbent system; (how the fact would prove this, however, if true, I know not;) Rayer, on the contrary, saw the cheloid tumor in four men, and only one woman; for myself, I have seen it in two women and one female child, in one man, and in a child, whose sex I do not remember. Probably both sexes, therefore, are nearly equally liable to it; and Rayer says it is in persons of lymphatic temperament that it takes place.

With regard to the *age* of the patient, Rayer says that he has not observed it previous to the second dentition; and that most of the patients whose cases had been published were adults or aged persons. I

have seen it at the age of 12, and in one child at 5; consequently before the second dentition was over, at all events—supposing it to have commenced. The young man is 21, the female 25, and another 30 years of age. Perhaps, therefore, it might form at any age.

Next, as to their *situation*. Alibert says they occur almost always on the sternum; six out of his eight cases having been thus formed; Rayer also had three cases in the same situation; the other two being on the buttock and the face, and Alibert's on the face and the neck. Out of the five cases that have fallen under my notice, one was on the face and forehead, another on the abdomen, two on the breast, and one only on the sternum: one of those beginning on the breast subsequently spread a little beyond it on the chest.

Next as to the *causes* of the disease. In the young man the tumor is ascribed to the kick of a horse; in the woman, to a blow; and in my other female patient, from the same cause. It arises also in the cicatrix of small-pox, or of a burn, as the other two cases shew you; or probably from any exciting cause in which the skin is altered; and sometimes they would appear to arise without any obvious cause.

Their *progress*, when once formed, varies much; sometimes they slowly and uninterruptedly increase, and alter their shape by irregularity of growth, or by sending out their characteristic branches; sometimes they grow to a certain size, and then remain stationary; occasionally, as in one case I have seen, they may shrivel and lessen, or nearly disappear, with only the appearance of a scar left in the part. They do not generally affect the health at all, as you saw in the young man. In my present patient the health is much affected with hysterical symptoms; but I do not know that this is directly owing to the tumors. In one of my other cases, the woman's health was obviously much deteriorated by the pain and irritation of the tumors when in an ulcerated state; and when I saw her again, she had recovered from these effects, the tumors being comparatively indolent nearly three years afterwards. At all events there was no appearance, even in this case, of cancerous deterioration of the system, though some had supposed the tumors to be of this nature.

Little need be said of *diagnosis*, for the tumor is quite peculiar in appearance. Occasionally, indeed, in an early stage, it may be somewhat like a cancerous tubercle in the skin; but it is redder and more irregular, and less elevated, and its progress soon shews the difference of the cases, the new growth never becoming warty, as cancer of

the skin does, nor fungous if it be ulcerated; and the situation of the tumor usually, which is rare for cancer, and the youth of many, would help the distinction, notwithstanding Alibert's perplexing similarity of name. At first sight the tumor might look like a nævus, but its firmness and flatness, and want of vascularity, will directly shew the want of resemblance.

In the next place, as to the *texture* and *appearance* of the tumor on *dissection*. No mention is made by Alibert and Rayer of this important part of the history of the cheloid tumor. Before I had seen Rayer's work I had made notes of the appearance of the tumors which Mr. Babington removed, which appeared to me to be formed by the whole thickness of the cutis, thickened and elevated, and with its tissue unravelled, as it were; so that fibrous bands ran in a reticulated manner through the tumor, with the usual cellular appearance of the skin between the meshes, but without any new deposit of any kind in the structure. In these preparations, removed by Mr. Cutler, you may see exactly the same appearance, but somewhat firmer and harder; but in the smaller of the two, in which it may be supposed to be in a very early stage, you may perceive a small oval substance, covered by the outer layer of the cutis, with the cuticle in a natural condition, while the deeper portion of the cutis is also unchanged; the tumor itself, scarcely more than the size of a large pin's head in diameter, is therefore in the central part of the cutis, and consists of the intermediate layers of the cutis, scarcely distinguishable from the rest after maceration, except by a little greater hardness.

Microscopical examination shews that this portion possesses the same appearance as the ordinary texture of the skin in its normal condition. I requested Mr. Toynbee, who is more used to the microscope than I am, to examine sections of each of these tumors, on the day they were taken out, without telling him what they were; and his note to me says:—"I have examined with care the specimens that you brought me, but I fear that at present the microscope will not throw much light upon the nature of these morbid structures. 1st. The two smaller slices (taken from the larger tumor) appear to be composed of delicate cellulo-fibrous tissue, among which very few cells can be traced. 2d. Sections of the growth in the integument (the small recent tumor) present also a very fine lax tissue, in which are interspersed nucleated cells." I fear this does not enlighten you much. In fact, hitherto the microscope does not seem to have shewn much more than the naked eye can perceive; and sometimes it shews even less distinction between natural

and morbid tissues; referring them all indiscriminately to nearly the same elementary parts. Such, at least, is the impression with which one rises from a perusal of Müller's recent researches on the intimate structure of tumors.

The *nature* of these tumors appears to me, therefore, to be little more than *simple hypertrophy* of the skin, in which the texture is unravelled, as it were, in the way that you may see in many cases of anasarca; only, instead of mere serum, you have a more organised deposit in the interfilamentous parts. If the hard bands and growths of burns are taken as analogous to the cheloid tumor, but in a more active state, you have some confirmation of this opinion, since these subsatnces are not mere contracted cicatrices, as you might imagine from some descriptions, but actual morbid growths, which occur on the surface without any contraction, and increase to a great thickness, and which are yet only a vascular hypertrophied condition of cutis. The cheloid tumor is perhaps most like a chronic and hardened condyloma; only the latter affect the surface of the cutis, and are generally deprived of cuticle, and moist, because they occur only where surfaces are kept moist and in contact; as in a girl in the hospital some time ago, who not only had them in the usual situations, on the labia and thighs, but had great masses also in the axillæ and umbilicus, where the secretions were retained.

One might almost take warts, condylomata, and cheloid tumors, as different degrees of similar enlargement, according as the papillæ chiefly, or the outer surface, or the whole thickness of the cutis, were the seat of the hypertrophy; allowing, also, for different degrees of moisture and rapidity of growth; or you might almost add the Barbadoes leg, and tumors of the scrotum and labia of the same disease, in which the subcutaneous tissue is also diseased; while the growths of burns give you analogous hypertrophy of the new tissue of cicatrices in a state of mere inflammation.

It would thus appear, as in all simple growths of natural tissue, that the cheloid tumor is innocent in its character, and is only a chronic tumor of the cutis. Alibert supposes it may be intermediate between dartre and cancer, possessing the scaliness of the one, and the pain of the other. The two diseases (if so unmeaning a term as dartre be used) have, however, nothing whatever in common, nor the cheloid tumor with either; unless, perhaps, some cases of mild superficial cancer of the skin have been confounded with the cheloid tumor, for one plate looks very like ulcerated cancer of the skin.

Such, then, being an account of this rare and singular formation, let us next consider the treatment applicable to it. And if it be, as I suppose it is, innocent in its nature, there is no harm in trying to relieve or cure the complaint; it is not, as in cancer, that you thus trifle away the time, till the opportunity for operation has gone by.

a. An operation for its removal by the knife frequently fails. I do not know the results in Mr. Babington's case; so we will hope that it is cured, as the girl has not returned. The tumor in Mr. Cutler's case was of a large size, and was, therefore, very proper for removal, especially as the ligature and caustic had previously been tried ineffectually. But Alibert and Rayer both speak of the tumor generally returning after it has been excised; perhaps, therefore, it may again come back, as it has twice before. In one case published by Dr. Warren he has given a plate of this curious circumstance—that after the removal of one tumor of this description, seven fresh ones made their appearance; namely, one long one in the central line of the incision, and six others of a smaller size, representing the apertures made by these ligatures that had been used in the previous operation*.

It is not very surprising that a disease, which is yet not malignant, should thus return, since we find warts in the same manner very difficult to eradicate, and their own secretion seems capable of propagating the formation; there is a disposition in the texture of the skin, or in the constitution, for them to grow frequently, and in great numbers, and at last, perhaps, they cease altogether in some unaccountable manner. This tendency to reproduction in the cheloid tumor shews us, however, that when we operate we should do so freely, with a good deal of surrounding skin. Dr. Warren says that, in one case, in which great attention was thus paid to the operation, the tumor was finally eradicated, although it had repeatedly returned before, after less skin had been removed.

b. Probably we have the same choice between the knife and the caustic that we have in cancer, but you perceive that it has been used ineffectually in Mr. Cutler's case, as well as the ligature, which last seems to be a bad way of operating in any case. Circumstances may render either the knife or caustic preferable to the other in particular cases, but if caustic is employed, it must, of course, from the structure of the diseased part, be so applied as to destroy the whole thickness of the cutis, and act a little beyond the tumor itself.

* We understand that it is already beginning to grow again (Aug. 20), and that there are the same appearances in each of the punctures of the needles as in Dr. Warren's case.

c. But the removal of the whole diseased structure, either by operation or by caustic, is not always practicable; in my present patient, for instance, and in a previous one, where the skin of the breast was affected, the diseased structure was much too extensive for removal, unless in some worse disease than this is, in which, as in cancer, severer plans may be justifiable. It has been observed in some rare cases that the new growth has ceased to increase, and has shrivelled up, and almost dispersed of itself. It is possible that we may sometimes imitate this natural cure, by some applications of astringent qualities, which may dry up the surface, as it were, without inducing ulceration or sloughing. In the child in whom it took place after small pox, I believe that much good was done by a strong solution of lunar caustic, and, perhaps, nitric acid, sulphate of copper, and iodine, with iodide of potassium, may each act in strong solution in a similar manner, being applied with a brush, and immediately dried again, leaving the surface corrugated; or, perhaps, lunar caustic in substance, applied so as to make a dry thin eschar, may answer the same purpose. In the acute thickening of burns, I have often known the new growths checked, and the pain immediately relieved, by the continued application of oil to the surface, by means of a piece of lint; and perhaps this may have some influence also on the apparently analogous growth of the cheloid tumor. I learnt this first from a patient in whom several points of painful growth and contraction were taking place on the neck and chest, and to whom some friend had recommended neatsfoot oil. I thought any other oil would produce the same influence, which I have found in reality very beneficial, and so I understand some of my colleagues have also done since I tried it, when the cicatrices were painful and increasing.

d. In the painful or in the ulcerated state of the tumor, I presume the ordinary applications useful in ulcers will promote cicatrization, and act as sedatives. I have ordered, in this patient of mine, a strong solution of goulard and laudanum, which, in the few days she has used it, has given some relief. A lotion of prussic acid will probably do the same. The various ointments of iodide of lead, or mercury, may be tried, or lotions of mineral acids, or black wash, or simple lime water, or the different preparations of zinc, and so on. In our patient, ulceration has several times returned after the surface had quite healed; and such was the result in the other ulcerated case*.

* Since this lecture was given, oil and laudanum, and strong solution of iodine, have been tried, each apparently at first with some diminution of the tumors, but the latter, in her hys-

e. It does not appear that internal remedies are likely to be of much service for this local growth. Rayer does not appear to have made many trials to cure the disease, either by local or general remedies, but Alibert made numerous attempts to do so, he tells us, but found them all fail. In this patient I have ordered assafoetida, and the shower bath, and purgatives, with a view to her excessively nervous temperament, and hysterical fits, and the latter symptom is already better. In the other woman, I have learnt that her usual medical attendant, partly by my advice, has tried various means, both before the short time she was under my care, and subsequently—sarsaparilla, and other tonics, and alteratives, and purgatives, oxymuriate, and other mercurials, and iodine in some forms, but at the end of above four years, the disease remains, though the general health has much improved, and the ulceration and local suffering have proportionately lessened. It is probable that there is no specific, either local or general, for the complaint, and that the ordinary rules of attention to the health may do some little good, but cannot effect a cure.

f. It will remain, then, for us to endeavour to soothe the state of the patient, and relieve the pain, and perhaps in some measure check the disease, and improve the general health by some of these means I have spoken of; and that we should seldom have recourse to operation, as being not often necessary for a non-malignant disease, and also because experience shews that it has generally failed; and, finally, that if we do have recourse to the knife, or to the use of caustic, they must be employed so as to destroy the whole disease in a free manner.

LIVERPOOL SUMMER ASSIZES.

Friday, August 27, 1841.

*Before Mr. Justice Wightman and a
Special Jury.*

THE MASTER, WARDENS, AND SOCIETY OF
APOTHECARIES, *versus* GREENOUGH.

THIS was an action brought to recover penalties alleged to have been incurred by the defendant by having practised as an apothecary at St. Helen's, in Lancashire, without having obtained the certificate of qualification required by the 55 Geo. III. c. 194. The action was tried at the Liverpool summer assizes of 1839, when a verdict was found for the defendant.

The Court of Queen's Bench having set

terical state, has given much pain, and has been intermitted, and solution of sulphate of copper ordered.

this verdict aside, and directed a new trial, the cause now came on for trial accordingly.

Mr. Cresswell, Mr. J. L. Adolphus, and Mr. F. Robinson, appeared for the plaintiffs.

The defendant did not appear.

Mr. Cresswell addressed the jury as follows :—

May it please your lordship : Gentlemen of the jury, this action is instituted by the Apothecaries' Company in discharge of a very important public duty with which they are entrusted. There is not any of you who can entertain the least doubt of it being absolutely necessary that there should be some superintending power over those who practise the medical profession.

It is by no means a piece of policy that is confined to that profession. In various other instances where the rights of mankind, not their health, are concerned, the legislature has thought fit to establish certain regulations in order to take care that none but duly qualified persons shall be entrusted with the care and management of such rights. During an assize it is brought immediately to one's recollection that attorneys who practise in this court are bound to undergo a certain period of instruction, and are then admitted to practise under certain regulations established by statute.

And, gentlemen, it is still more important that the health of the people of this country should be guarded in this manner, and especially is it necessary for the benefit of the poorer classes, that they whose necessities may tempt them to go to an inferior class, in order to get medicine cheaper, should not be exposed to the dangers necessarily incident to taking medicine from unskilful and uneducated persons.

Long ago, a charter was granted to the Apothecaries' Company, but it was found that their means of doing good were very much limited, and a public Act of Parliament was passed in the year 1815, for the purpose of preventing uneducated and unauthorized persons from practising as apothecaries in this country; and it would be found, if you were to inquire throughout the country, that the examination, to which aspirants to that profession have been subjected by the Apothecaries' Company, has done a great deal to raise the standard of medical knowledge in this country. And, gentlemen, it is with the view of discharging this important duty, and very far from having a desire either to press hardly on this defendant, or to put money into the funds of the Society, that this action is instituted.

Gentlemen, the statute I speak of was passed in the year 1815, and at that time a clause was introduced compelling all persons who meant to practise as apothecaries to submit to a certain examination. The learned counsel then read the 14th section of

the Act, which provides that it shall not be lawful for any person or persons (except persons already in practice as such) to practise as an apothecary in any part of England or Wales, unless he or they shall have been examined by the Court of Examiners, or the major part of them, and have received a certificate of his or their being duly qualified to practise as such.

Gentlemen, here is a reservation in favour of those who were in practice at the time the statute passed, in order that any vested rights should not be invaded by a statutory authority. This present defendant could hardly have been in practice at that time, because, as I shall shew you, he would be a boy of eight or nine years old ; so that no question can arise of that sort.

The 20th section imposes a penalty on all persons practising without a certificate. The learned counsel read the 20th section, and then proceeded—

Gentlemen, I shall prove to you that this defendant practised as an apothecary at St. Helen's for a considerable time, and in spite of remonstrance.

It appears that his sister—or rather that he,—had set up a druggist's shop in his sister's name. His sister's name was over the door. He was the person who attended in it; who administered medicines, who sold medicines to persons who came, and who attended sick persons in the neighbourhood, prescribing and administering medicines.

Now, gentlemen, there are two means by which parties attempt to evade this Act of Parliament. First, they call themselves "surgeons," and they think that by calling themselves "surgeons" they are beyond the reach of all the liabilities attaching to them as apothecaries. If a man has to attend any surgical case, for instance a broken limb, and administers medicines merely as auxiliary to his operation as a surgeon, that is practising as a surgeon. But if a man, calling himself a surgeon, is invited to attend a case of a medical description, for instance for a cough, it is not because he calls himself a "surgeon" that he does not practise as an apothecary. Sometimes they try to get rid of the Act by saying they are chemists and druggists; but it is no part of the duty of a chemist and druggist to prescribe medicines; and the newspapers must have informed you of the calamitous accidents resulting from persons in chemists' shops venturing to administer medicines. I will read a short paragraph from a judgment given some years ago on the subject by a learned judge. He says, "The business of a surgeon is, properly speaking, the treatment of external ailments and injuries of the limbs. With a view to the recovery of a patient in a case of that description he may perhaps prescribe and dispense medicine. But the act has drawn

the distinction between the various departments of the art with great precision. A chemist may prepare and vend, but not prescribe or administer medicine. Each is protected in his own branch, and neither must interfere with the province of the other. We think the plaintiff has interfered with the province of the apothecary, and that therefore this rule must be discharged."

That was a case where a surgeon sought to recover for his attendance and medicine in a case of typhus fever; and the Court held that, not being a qualified apothecary, he could not recover. I could prove many cases in which the defendant has acted as an apothecary, but I shall not go through more than one or two. The defendant is not here to defend the action, and it is only on account of the public importance of the matter that I am going through the statement. I shall shew that this person, the defendant, was residing at St. Helen's; that various persons called him in to attend on them; that he did attend, saying sometimes they were sick of a fever, and sometimes of a cold. I think his favourite disease was "pleurisy fever," "intermittent fever." On one occasion he stated that he had three hundred bills to make up; and I think you will be quite certain that, if the Apothecaries' Company meant to act on the powers given them by this Act of Parliament, they should take notice of a case like the present.

Towards the close of this statute there is this section: "Provided always and be it further enacted, that nothing in this Act shall extend, or be construed to extend, to prejudice or in any way to affect the trade or business of a chemist and druggist in the buying, preparing, compounding, dispensing, and vending drugs, medicines, and medicinal compounds, wholesale and retail; but all persons using or exercising the said trade or business, or who shall or may hereafter exercise the same, shall and may use, exercise, and carry on the same trade or business in such manner, and as fully and amply to all intents and purposes, as the same trade or business was used, exercised, or carried on, by chemists and druggists before the passing of this Act."

Now this cause was brought to trial in this place once before; and the learned judge who tried it, on reading this section, thought it was a question whether, before this Act passed, a chemist and druggist had been in the habit of prescribing medicines; because, he said, the Act allows him to do now what he could do before, and if before that Act he could prescribe, so he may prescribe since; leading to a very strange conclusion, and certainly repealing the Act if that were so, and making it perfectly nugatory. For why should a man perfectly uneducated—brought into a shop just as a little boy perhaps, to

serve pills across the counter first—why should he be allowed to practise after as an apothecary, when a person regularly educated as an apothecary, if not examined, could not do so? Therefore it seemed to those who were engaged in the cause, that that learned judge had not put a correct construction on this section, but that the meaning of it was that a chemist and druggist could go on only "in buying, preparing, compounding, dispensing, and vending drugs, medicines, and medicinal compounds." The question was raised before the superior Court, where they decided that this does not give to any chemist the power of acting as an apothecary, and prescribing medicines. That decision being given, this cause was brought down to trial; and I trust that as there is a solemn judgment of a superior Court, that no chemist and druggist has a right to act as an apothecary in dispensing medicines otherwise than by the prescriptions of authorized persons, it will tend to remove any misconception that may have arisen since that trial took place. I shall prove the case very shortly, and the defendant, after hearing that judgment in his own case, will no longer attempt to practise I should think.

I should state that, though we might go for many penalties, I shall only ask for one, that there may be a judgment in the case establishing that which the Company have all along sought to establish for the public benefit, and in discharge of their public duty.

Mary Owen examined—I am the wife of William Owen, of Peasey Cross, near St. Helen's. I lived there in 1839. John Gerrard lodged with me; he was the guard of a railway carriage; he was taken ill in January, 1839, and died in the February following. I know the defendant; he lived in St. Helen's at that time. John Gerard sent me to fetch the defendant, and he came; he bled John Gerard, and told me to put him into a warm bath: he ordered him some medicine; I went and fetched some of it myself from the defendant's shop. The defendant continued to attend John Gerard till his death. On the Sunday after Gerard was taken ill, I asked the defendant what was the matter with him; he said it was the intermittent fever; he said it was betwixt the pleurisy and the typhus.

Henry Marsh examined.—I am a shop-keeper at St. Helen's. My wife Elizabeth Marsh was taken ill in the summer of 1839; the defendant was called in to attend her as a doctor; he prescribed medicines for her, and supplied them; he said her complaint was inflammatory fever. He told me he had upwards of 300 patients—that he had 300 bills out. The defendant is about thirty years of age.

Mr. Cresswell said he thought it unnecessary to offer further evidence.

Mr. Justice Wightman summed up as follows: — Gentlemen of the jury: this action is brought by the Master, Wardens, and Society of Apothecaries, against Henry John Greenough, to recover a penalty from him for having practised as an apothecary without being duly qualified so to do.

In the year 1815, an Act of Parliament was passed for regulating the practice of apothecaries throughout England and Wales, and by that Act it was declared that it should not be lawful for any person (except persons already in practice) to practise as an apothecary in any part of England and Wales, unless he should have been examined by the Court of Examiners of the Apothecaries' Company, and have received a certificate from them of his qualification to practise.

Therefore, unless the defendant has either a certificate from the Court of Examiners, or has been in practice before the 12th of July, or on the 12th of July, 1815, it is penal for him to practise as an apothecary.

According to the evidence of the last witness, Greenough appears to be hardly thirty years old at this time; and therefore, in the year 1815, we can hardly suppose that he could by possibility have been in practice as an apothecary. And unless, therefore, he has the certificate of the Court of Examiners testing his qualification to practise, it is illegal in him so to do.

But there is an exception in the Act of Parliament with respect to the practising of chemists and druggists; for it is said, "That nothing in this Act contained shall extend or be construed to extend, to prejudice, or in any way to affect the trade or business of a chemist and druggist, in the buying, preparing, compounding, dispensing, and vending drugs, medicines, and medicinale compounds."

Now it is said that a number of persons have availed themselves of that exception to practise, professing to act as chemists and druggists, but who have in fact practised as apothecaries. The distinction, however, appears to be one recognised by the Act of Parliament itself, that an apothecary does not merely prepare, compound, dispense, and vend drugs and medicines, but applies and administers them; because in the fifth section of the Act, as applied to apothecaries, there are certain penalties for apothecaries refusing to do certain things, among which are mentioned, as part of the ordinary duties of apothecaries, not only compounding, mixing, preparing, and selling medicines, but applying and administering them. If a person not only sells and mixes medicines, but applies and administers them in the ordinary course of practice in attending on patients, no doubt he comes within the definition of an apothecary. That would be what the Act itself seems to distinguish from

the ordinary selling of drugs (which a chemist or druggist may do) in the office and duty of an apothecary, which is not only selling drugs simply, but applying and administering drugs, which he himself vends and prepares.

Now, gentlemen, in this case you have the evidence of two witnesses who speak to the defendant's having attended two patients in the ordinary mode of an apothecary. The parties were ill. They were not surgical cases, both were cases of fever, and were described by him to be "betwixt pleurisy and typhus," and "an inflammatory fever."

Gentlemen, I hardly know what question there is for you. If you are satisfied that the defendant did act as an apothecary on any occasion, it will be your duty to find a verdict for the plaintiff. If you think he did not, but that he merely acted as a chemist and druggist, (which, as it seems to me, we are not warranted by this evidence in believing, for he administers the remedies which he himself has prescribed and prepared), you will find for the defendant.

The jury returned a verdict for the plaintiffs for one penalty of £20.

EXTRACTION OF A GOLD-PIN PASSED FAR INTO THE URETHRA.

BY DR. BOINET.

I WAS called some time since, to visit a young man, who, for the purpose of excitement, had introduced a gold-pin into the urethra, into which it suddenly slipped from his grasp and disappeared. It was more than two inches long, and the head which had been pushed in first was as large as a hemp-seed. In his attempt to push it out again he had made it go further towards the bladder, and when I came to him the head of the pin was in at the membranous part of the urethra. I could easily put it in the perineal region, and applying my thumb on the head to prevent its going on towards the bladder, I tried to push it out of the canal, pressing in the direction opposite to that by which it had entered, and at the same time pulling the penis, to prevent the point from catching in the folds of the mucous membrane. But, in spite of all my precautions, my attempts appeared to make it go towards the bladder, especially whenever I tried to disengage the point from the mucous membrane into which it kept running. I had scarcely any instruments with me, nor indeed would any ordinary ones have been of any use; therefore my endeavours to draw it out were unavailing.

I was then determined to run the point of the pin through the wall of the urethra, and then to turn the pin end for end, and push

the head towards the external orifice. This I accomplished in the following manner :— With the left thumb I firmly fixed the head of the pin, and then bending the penis double at the part where the point of the pin lay, I made the latter pass through the wall of the urethra, and drew out all but the head, which now lay where the head had just previously been. This done, I carried the shaft of the pin backwards, and so made the head more forwards, and then pushing on the shaft from behind forwards, I pushed the pin head first towards the external meatus, through which I now easily drew it out with a pair of dressing forceps. In a word, to perforate the urethra from within outwards, to turn the pin, to push it on, and to extract it—such were the manœuvres of this operation.

The consequences of this perforation were of the simplest kind: the patient scarcely ever felt a pricking in making water. Three days after he was perfectly well. Subsequently, however, in consequence of a severe and maltreated gonorrhœa, an abscess formed around the urethra, and was followed by a fistulous opening; but it was remarkable that this opening was situated at a considerable distance from the part at which the urethra had been punctured. — *Gazette Médicale*, Mai 1, 1841.

IMPORTANT DECISION.

WE beg to direct attention to the decision given in the case of the Society of Apothecaries *versus* Greenough. It will be found at page 924 of our present number.

MATERIA MEDICA.

WE are requested by Mr. Battley to state that on the 30th of September he will exhibit in the great hall of the Royal College of Physicians, by permission of the President and Fellows, samples of drugs, and of certain original and active preparations, which he invites the members of the medical profession to inspect; and that he will continue his analysis of those drugs during the autumnal and winter months at the Laboratory attached to the Ophthalmic Hospital, Moorfields.

The subjects of analysis will be regularly announced in this journal.

UNIVERSITY COLLEGE HOSPITAL.

ON Saturday, the 7th inst., Mr. Thomas Morton, author of “Surgical Anatomy of the Perinæum, “Surgical Anatomy of Inguinal and Femoral Hernia,” &c., &c., and one of the Demonstrators in University College, was appointed Assistant Surgeon to the Hospital attached to that Institution.

APOTHECARIES’ HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, August 26, 1841.

R. H. Williams, Penryn, Cornwall.—M. Ryan, Dublin.

TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 21st Aug. 1841.

Small Pox	6
Measles	18
Scarlatina	12
Whooping Cough	30
Croup	6
Thrush	6
Diarrhoea	11
Dysentery	2
Cholera	6
Influenza	0
Typhus	21
Erysipelas	1
Syphilis	1
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	145
Diseases of the Lungs, and other Organs of Respiration	214
Diseases of the Heart and Blood-vessels	21
Diseases of the Stomach, Liver, and other Organs of Digestion	88
Diseases of the Kidneys, &c.....	5
Childbed	3
Ovarian Dropsy	0
Diseases of Uterus, &c.	7
Rheumatism	0
Diseases of Joints, &c.....	2
Ulcer	0
Fistula	0
Diseases of Skin, &c	0
Diseases of Uncertain Seat	96
Old Age or Natural Decay.....	58
Deaths by Violence, Privation, or Intemperance	15
Causes not specified	2
Deaths from all Causes	776

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N. Longitude 0° 3' 51" W. of Greenwich.

August.	THERMOMETER.		BAROMETER.	
Wednesday 25	from	46 to 63	30·04 to	30·02
Thursday . 26		57 73	30·11	30·20
Friday . . . 27		58 78	30·19	30·15
Saturday . 28		53 70	30·12	Stat.
Sunday . . . 29		51 73	30·11	30·02
Monday . . 30		53 72	29·96	29·82
Tuesday . 31		56 70	29·74	29·80

Prevailing wind, S.W.

On the 25th, cloudy; rain in the morning and afternoon. The 26th, morning overcast, otherwise clear. The 27th and three following days generally clear. The 31st, cloudy; rain fell during the afternoon.

Rain fallen, 19 of an inch.

CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

THE
LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, SEPTEMBER 10, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XLIX.

*Influenza. Symptoms and progress. Con-
jectures as to its cause. Treatment.*

CATARRH, which was the principal subject of the last lecture, occasionally prevails far and wide as an epidemic disease. I speak, indeed (February 4, 1837), during the immediate presence of one such visitation, although the extreme violence of the complaint that has been raging among us is now fast subsiding. You can hardly be without curiosity to know what has been learned respecting an influence which has thus, on a sudden, before your eyes, disturbed and sickened a whole community. I have here used, without thinking of it, the very word by which, in a foreign version, the disorder is denominated. It has received, however, various names; for it has been known and noticed from remote antiquity. Cullen calls it *catarrhus e contagio*: and under that head, in his Nosology, you will find a copious reference to recorded accounts of epidemic catarrh, as it has been observed to spread over great portions of the world. In France the disorder thus prevailing, is styled the *grippe*. The Italians, putting the cause for the effect, call it *influenza*, the *influence*: and this last term influenza has now become naturalized in our language. Since Cullen's time there have been four or five more of these epidemics. One in 1782, which extended over all Europe, visiting every country therein, affecting more than one half of its inhabitants, and proving fatal to very many of

them. You will find, in the third volume of the *Transactions of the College of Physicians* a good account of the disease as it then shewed itself in this country. In the spring of 1803 another instance of it occurred; and of this the history, as compiled from the testimony of 124 observers, is preserved in the 9th and 10th volumes of the *London Medical and Physical Journal*. In the month of April, in the year 1833, the influenza again made its appearance, and prevailed extensively, both here and elsewhere: and of the influenza of 1837 you have had, and you still have, the opportunity of being observers. A very good and instructive sketch of this epidemic malady, compiled by Dr. Hancock, is to be found in the *Cyclopædia of Practical Medicine*. To that article, to the publications I just now mentioned, and to the works enumerated by Cullen, I may refer you for much that is curious and interesting in the history of the disease; but which would not be so well adapted to our immediate purpose in this place—namely, of seizing upon the practical facts which have been ascertained respecting influenza.

Symptoms.—One characteristic feature of this species of catarrh, as distinguished from the ordinary sporadic disorder, is the sudden occurrence, in the outset, of more decided febrile disturbance. The first two patients whom I saw in the epidemic of 1833 had just the symptoms which frequently mark the commencement of an attack of continued fever; and I did not know, at my first visit, what was about to happen to them. The symptoms, taking them altogether, are somewhat as follows. The patient is chilly, and perhaps shivers: presently headache occurs, and a sense of tightness across the forehead, in the situation of the frontal sinuses; the eyes become tender and watery; and sneezing and a copious defluxion from the nose ensue, followed or accompanied by heat and uneasiness about the throat, hoarse-

ness, and a troublesome cough, and oppression of the breathing. In short, the symptoms are the symptoms of catarrh; including in that term all the varieties of disorder which are sometimes met with separately—gravedo, coryza, bronchitis: and with these symptoms, a sudden, early, and extraordinary subdual of the strength; and, most commonly, great depression of spirits. The debility which comes on at the very outset of the complaint is one of its most singular phenomena, taking place, in some cases, almost instantly, and in a much greater degree than would seem proportioned to the other symptoms of the malady which it thus ushers in. Indeed, this rapid and remarkable prostration of strength is more essentially a part of the disorder than the catarrhal affection, which sometimes (though rarely), is absent, or imperceptible. It is upon the mucous membranes, however, that the stress of the disease generally falls; especially upon the internal lining of the air passages. Those of the alimentary canal seldom escape entirely; but they suffer in a less degree. The tongue is white and creamy, the palate loses its sensibility, the appetite fails, nausea and vomiting are not uncommon, and sometimes there is diarrhoea. The pulse, in the uncomplicated disease, is soft, and generally weak. The patients complain also of pains in the limbs and back, and of much soreness, a bruised, fatigued, or tender feel, along the edges of the ribs and in various parts of the body.

In its simple form and ordinary course, the disease abates of its violence after two, three, or four days, and the patient is usually convalescent before the termination of the week: but cough and much debility are apt to survive the other symptoms, and while these continue, the complaint is very easily renewed.

Pre-existing disease, and peculiar constitutional habits and tendencies, modify considerably the character of the influenza, as it affects different persons. I do not attempt to represent its various phases; they are fit subjects of study for yourselves.

Causes.—I have remarked that Cullen makes this species of catarrh to proceed from contagion. But the visitation is a great deal too sudden and too widely spread to be capable of explanation in that way. I will not say that the disease may not be in some degree infectious; for there is reason to believe that other epidemic disorders, having many points of analogy with the influenza, are some how imparted from one individual to another, although they are mainly produced by some influence which resides in the atmosphere. There are points in the history of influenza which furnish a strong presumption that the exciting cause of the disorder is material, not a mere quality of the atmo-

sphere; and that it is at least *portable*. The instances are very numerous, too numerous to be attributed to mere chance, in which the complaint has first broken out in those particular houses of a town at which travellers have recently arrived from infected places. But this great and important question of contagion I hope to examine with you more rigorously on a future occasion. What I wish to point out now is the fact that the influenza pervades large tracts of country in a manner much too sudden and simultaneous to be consistent with the notion that its prevalence depends exclusively upon any contagious properties that it may possess. You are aware that it has recently seized upon all parts of this metropolis—and I believe I may say of nearly the whole kingdom—within the space of a very few days. It has been observed to occur also at the same time on land, and on board different ships which have had no communication with the shore, nor with each other. Thus it is stated in the Transactions of the College of Physicians, that on the 2d of May, 1782, Admiral Kempenfelt sailed from Spithead with a squadron of which the *Goliath* was one. The crew of that vessel was attacked with influenza on the 29th of May; and the rest were at different times affected, and so many of the men were rendered incapable of duty by this prevailing sickness that the whole squadron was obliged to return into port about the second week in June, not having had communication with any shore, but having cruised solely between Brest and the Lizard. This happened in one part of the fleet. In the beginning of the same month, another large squadron sailed, all in perfect health, under Lord Howe's command, for the Dutch coast. Towards the end of the month, just at the time therefore when the *Goliath* became full of the disease, it appeared in the Rippon, the Princess Amelia, and other ships of the last mentioned fleet, although there had been no intercourse with the land. Similar events were noticed in the epidemic of 1833. One or two curious instances of the sudden sickening of considerable bodies of men in different places at the same time, were related to me on good authority. On the third of April in that year—the very day on which I saw the first two cases that I did see of the influenza, all London being smitten with it on that and the following day—on that same day the *Stag* was coming up the channel, and arrived at 2 o'clock off Berry Head, on the Devonshire coast, all on board being at that time well. In half an hour afterwards, the breeze being easterly and blowing off the land, 40 men were down with the influenza, by six o'clock the number was increased to 60, and by two o'clock the next day to 160. On the self same evening a regiment on duty

at Portsmouth was in a perfectly healthy state, but by the next morning so many of the soldiers of that regiment were affected by the influenza, that the garrison duty could not be performed by it. I make no doubt that facts of a like nature have occurred during the present epidemic, and will be made known in due time. They illustrate several important points in respect to the disease: viz. the impossibility of accounting for its prevalence upon the principle of mere contagion—the suddenness of its invasion—and the early and extreme prostration of strength with which it is attended.

The occurrence of epidemic catarrh, as well as of most other epidemics, is unquestionably connected with some particular state or contamination of the atmosphere. What that state is, or what may be the kind of contamination, no one knows. The present epidemic followed hard upon the sudden thaw that succeeded the remarkable snow storm of the last week of the last year. A similar coincidence between the breaking out of the same disorder, and a sudden elevation of the temperature of the atmosphere, happened at St. Petersburg in the epidemic of 1782. “On a cold night (Maertens says), the thermometer rose 30° of Fahrenheit; the next morning 40,000 people were taken ill with the influenza.” Now if every epidemic had been preceded by similar changes in the weather, we might resolve the universal prevalence and sudden accession of the complaint, into the effect of the cold and damp state of the air, produced by the thaw. But it is not so; for, as Dr. Hancock observes, there has not been any uniform connexion between any one sensible quality of the atmosphere as to heat or cold, rain or drought, wind or calm—and the invasion of the epidemic. “Et tempore frigidiori et calidiori, et flante tam Austro quam Boreâ, et pluvioso et sereno cælo, peragravit hæc omnes Europæ regiones, et omnia loca indiscriminatum.” Irregularities and great vicissitudes of weather have however gone before the disease in very many instances: but sometimes one condition of the atmosphere, and sometimes another, has been its immediate predecessor: and the epidemic has frequently been observed to fall partially and capriciously, like the blight over a field or district. Short, in his chronological history of the weather, says that thick ill-smelling fogs preceded, some days, the epidemic catarrh of 1557. Jussieu states that the grippe of the spring of 1733 appeared in France immediately after offensive fogs, “more dense than the darkness of Egypt.” So also in 1775, Petit informs us that in France the disease was ushered in by thick noisome fogs. In the same year it visited the shire of Galloway in Scotland, where, we are told, “a continual dark fog and parti-

cular smoky smell prevailed in the atmosphere for five weeks, the sun being seldom seen.” Dr. Darwin has recorded that, in 1782, “the sun was for many weeks obscured by a dry fog, and appeared red as through a common mist:” and he supposes that “the material which thus rendered the air muddy probably caused the epidemic catarrh which prevailed in that year.” You will call to mind here the dark fog which brooded over this city in the midst of the raging of the distemper about ten days ago, and which was repeated, in a less degree, on Wednesday last (Feb. 1).

It has been observed also, that shortly before, or during, or soon after, the prevalence of these epidemic catarrhs—*epizootic* diseases have raged; various species of brutes, and of birds, have been extensively affected with sickness: while on some occasions prodigious swarms of insects have made their appearance. In short, a great variety of facts concur to render it probable that some peculiar condition of the air existed, which, though it might be favourable to the multiplication of some species of living creatures, such as the insects just referred to, operated as a poison upon the human body, and upon the bodies of many of the brute creation.

It is a very curious circumstance in the history of epidemic catarrhs, and worthy of your reflection, that they *travel*; migrate, as it were from one place to another: and moreover, that they hold, for the most part, to certain courses, in spite even of opposite winds, and of variations of temperature. It has been noticed that the influenza generally follows a westerly direction, or one from the south towards the north west. In this remarkable property it resembles, as you may perhaps be aware, the epidemic cholera.

Although the general descent of the malady is, as I have said, very sudden and diffused, scattered cases of it, like the first droppings of a thunder shower, have usually been remembered as having preceded it. The disorder is most violent at the commencement of the visitation; then its severity abates; and the epidemic is mostly over in about six weeks. Yet the morbid influence would seem to have a longer duration. In a given place nearly all the inhabitants who are susceptible of the distemper suffer it within that period, or become proof against its power. But strangers who, after that period, arrive from uninfected places, have not, apparently, the same immunity.

The locality does not appear to be thoroughly cleared of the poison for some time: or perhaps a more cautious statement of the fact would be, that the disorder generally shews itself again in succeeding years, but in a milder and less general form. This must

depend either upon some remaining dregs, or possibly some revival, of the injurious influence; or upon some abiding predisposition impressed upon the bodies of men by its former visit. You may hear, every year, of Mr. *So and so* having the influenza. In many instances, no doubt, common sporadic catarrh is dignified by that name; but it is certain also that many of the colds, and bronchial disorders, of the seasons which immediately follow a period of genuine influenza, are attended with much more languor, debility, muscular aching, and distress, than belong to an ordinary attack of catarrh.

All this is very curious; and very mysterious. All this, or much of it, is true also of all the diseases which are known to prevail occasionally as epidemics. The facts that have now been mentioned respecting the influenza, warrant, I think, the conclusion that it does *not* depend upon any mutations in the ordinary qualities of the atmosphere; upon any degrees or variations, I mean, of its temperature, its motions, or its moisture; upon what is expressed in the single word *weather*. Concerning a calamity so generally felt, and so obscure in its origin, conjecture, you may well believe, has not been idle. One hypothesis assigns the complaint to some change in the electrical condition of the air: to its becoming negatively electric; or to its being such as to cause an excessive accumulation of electricity in the animal economy. The facts adduced in support of these views are of this kind. Meat, sent up, by means of a kite, high into the atmosphere, during the prevalence of the disease, has returned putrid. Large heavy separate clouds, in a state of negative electricity, have been observed just before the setting in of an epidemic. Thunder storms, and tumults of the atmosphere, have occurred at the same periods. During the raging of one epidemic 300 women engaged in coal dredging at Newcastle, and wading all day in the sea, escaped the complaint. It has been thought that this exemption might be accounted for by supposing that the almost constant immersion of the body in a conducting medium prevented any undue collection of electricity.

Again, it has been fancied that the tolerably definite course of the epidemic, in its migrations, might be somehow connected with magnetic currents.

Another hypothesis, more fanciful perhaps, at first sight, than these, yet more easily accommodated to the known phenomena of the distemper, attributes it to the presence of innumerable minute substances, endowed with vegetable or with animal life, and developed in unusual abundance under specific states of the atmosphere, in

which they float, and by which they are carried hither and thither. Myriads of these animalcules, or of these vegetable germs, coming in contact with the mucous membranes, and especially with that of the air passages, irritate (it is imagined) these surfaces, and exercise a poisonous influence upon the system. Now, the sporules of certain fungi, which ruin the health, and destroy the vitality of larger plants on which they prey, are inconceivably small. I shall prove to you presently, that vegetable effluvia are capable of producing, in the human body, symptoms not very dissimilar from those of the influenza. Again, that the waters of this globe swarm with living creatures, which are invisible by our unaided eyes, the microscope has taught us. Others, too small even to be estimated by that wonder-shewing instrument, in all probability exist. We cannot doubt that the gaseous fluid which surrounds this planet equally teems with living atoms. We know that multitudes of insects, and of cryptogamous plants, infinite in number in respect to our finite powers of computation, *are* sometimes suddenly hatched or developed, in places which were previously free from them. It is easy to conceive that atmospheric infusoria (so to speak) may rapidly congregate or vivify, in masses sufficient to render deleterious the very air we breathe. If this be so, we can understand how such a cause of disease may first act here and there, and presently overspread large districts; how it may move, or be wafted from place to place, or be carried about by persons; how its course and operation may be circumscribed and definite; and how some germs or ova may remain after the visit, retaining their vitality, and ready in future seasons again to start into life and activity under favouring circumstances. Taking the insect hypothesis, and knowing as we do that some animal poisons (that of small-pox, for example) have the singular property of multiplying themselves in the human body, like yeast in beer, we may conceive that diseases, produced by animalcules, may thus infect the fluids of the body, and become contagious in the fullest sense of that term. Lastly, the uniform duration of these epidemics has been supposed to add probability to the hypothesis that they result from the operation of some organic principle, which has its definite periods of growth and of decay. All this is sheer hypothesis: but it is as good an hypothesis as I am able to offer you; and you must be content to conceive of it as possibly the true one, until a better shall be discovered.

Treatment.—The character of debility which is so conspicuously impressed upon this disease, bears closely upon the treatment required for its cure, or its safe conduct.

As in all other epidemics, the severity of the complaint is extremely variable in different persons. In some it proves a very trifling malady, which soon passes off, and requires little or no assistance from medicine. In others it is a very distressing affection, and lays the foundation for other and still more serious, though more chronic diseases: and in some, and more especially in the old and the unsound, it shews itself a very fatal disorder. The absolute mortality under the recent epidemic has been immense: the daily newspaper obituaries have been unusually long; and you may have remarked, that the ages of the persons whose deaths they announced were in almost all cases great. The funerals have been so frequent, that difficulty has been found in performing them without indecent hurry and confusion. One undertaker, of whom I was informed, had at one time 75 dead bodies to inter—*above ground*, as he expressed it; and mourning coaches, and black horses, could not be procured in sufficient numbers to meet the demand for them. The absolute mortality, therefore, I say, has been enormous; yet the relative mortality has been small. You will hear people comparing the ravages of the influenza with those of the cholera, and inferring that the latter is the less dangerous complaint of the two; but this is plainly a great misapprehension. Less dangerous to the community at large (in this country at least) it certainly has been; but infinitely more dangerous to the individuals attacked by it. More persons have died of the influenza in the present year than died of the cholera when it raged in 1832; but then a vastly greater number have been affected with the one disease than with the other. I suppose that nearly one half of those who were seized with the cholera perished: while but a very small fraction indeed of those who suffered influenza have sunk under it. The only fatal cases that I have seen have been in persons advanced in life, or in persons whose lungs were previously known to be unsound.

Now the treatment of the influenza is pretty well understood. The chief risk of mistake is that of being too active with the lancet. Certainly those affected by this disorder do not well bear active depletion. Of course no one would think of blood-letting except the symptoms were severe, and the distress great; but even in such cases, much caution is requisite in adopting that remedy. If you find that the inflammation has extended to the pleura, or to the substance of the lungs, it may be necessary to open a vein, or to apply cupping-glasses over the chest; but this is a very unpleasant necessity. Such is the result of all that I have seen, and heard from others, of the present epidemic; and such is the result of the re-

corded experience of nearly all previous epidemics. You will find abundant evidence of this collected into a summary view by Dr. Hancock. In 1510, Dr. Short says bleeding and purging did harm. In 1557, bleeding was said to be so fatal, that in a small town near Madrid, 2000 persons died after it in the month of September. In 1580, Sennertus, after stating that where blood-letting was omitted, the mortality was not greater than 1 in 1000, adds, "*Experientia enim hoc comprobavit, omnes fere mortuos esse, quibus vena aperiebatur.*" Dr. Ash observes that, in 1775, it was never necessary to bleed at Birmingham; and that, in a neighbouring town, 3 died who were bled, and all recovered who were not bled. And a great deal more evidence to the same purpose you may find in the article I have referred to.

I believe the best plan of management—as far as any general plan can be laid down—is to keep the patient in bed, and after clearing the bowels by a mild aperient, to give a couple of grains of James's powder every six hours, with a saline draught, and slops, till the first brunt of the disorder is over; and then, if the cough be troublesome, and the breathing laborious, and much rhonchus, or sibilus, or crepitation, is audible in the chest, to apply a blister, and to give expectorants and diuretics. What I prescribed a great many times was something of this kind: half a drachm of oxymel of squills, a drachm of the spiritus ætheris nitricus, and sometimes another drachm of paregoric, in almond emulsion. With respect to full doses of opium, when the feverishness is abated and the headache gone, I should recommend the same practice which I described in the last lecture. If there be any lividity of the skin, or of the mucous membranes, it is dangerous to give a full dose of opium. On the other hand, if there be no visible indication in the complexion that venous blood is circulating in the arteries, opium given at bed-time will have sometimes a magical effect in relieving distress, and (by giving rest and refreshing sleep) in recruiting the strength also. In cases in which the powers of the system are prostrate, and the face and lips are livid, and the patient is tugging to expectorate the mucus that is filling up his air passages, you should have recourse to ammonia, to nourishing broths, and it may be to wine and water: and when all danger from the disease is over, but the patient remains feeble, languid, and out of spirits, then is the fit time to administer tonic medicines; and although snake-root and cascarilla are well spoken of by many practitioners, I know no tonics so good as the sulphate of quina, or of iron, for such patients.

As to external applications, mustard poul-

tices, blisters, and the like—and to the inhalation of the steam of hot water,—these may each and all be very useful; but I have nothing to add concerning the time and manner of their employment to what I said upon the same subject in the last lecture.

There is one point in the treatment which I must not omit to notice, although I cannot tell you much about it from my own experience. Dr. Thomas Davies, an accurate observer, and one well qualified to form a judgment in the matter, states that he found a *mercurial* treatment answer well in severe cases, in the epidemic of 1833. He perceived that active depletion was not well borne, and discovering that in the bad cases there was always crepitation in the lower lobes of the lungs, he thought mercury was one of the most proper remedies to subdue the inflammation, and to occasion absorption of the fluid effused into the air cells. He had severe cases to deal with. He says that it happened to be his duty to admit the patients into the London Hospital during the week the epidemic was at its height, and that 32 beds which were placed at his disposal were all soon filled with individuals labouring under the severe forms of influenza; so severe, that he believed the greater number of them would have perished if they had been allowed to wander about the streets, or even to have remained at their own homes, with the insufficient attention they could there have obtained. Only one or two of these patients were bled, but they were all put under the influence of mercury. This treatment commenced on Thursday, and all who, by Saturday night, were affected in the usual way by the remedy, safely and ultimately recovered, with the exception of two; and one of these had hypertrophy of the heart, and diseased aortic valves. His object was not to salivate, but merely to make the gums tender. It was of course necessary that the action of the mercury should be prompt; and he found that the most quick and efficacious means of obtaining it was by rubbing in the linimentum hydrargyri.

LECTURES

ON THE

FUNCTIONS OF THE NERVOUS SYSTEM.

BY W. B. CARPENTER, M.D.

LECTURE X.—(continued.)

*Functions of the Cerebellum (continued)—
Functions of the Cerebrum.*

AMONG the arguments adduced by Gall and his followers, in proof of the connexion

between the cerebellum and the sexual instinct, is one which would deserve great attention, if the facts stated could be relied on. It has been asserted, over and over again, that the cerebellum, in animals which have been castrated when young, is much smaller than in those which have retained their virility,—being, in fact, *atrophied* from want of power to act. Now it is unfortunate that vague assertion, founded on estimates formed by the eye from the cranium alone, is all on which this position rests; and it will be presently shown how very liable to error such an estimate must be. The following is the result of a series of observations on this subject, suggested by M. Leuret, and carried into effect by M. Lasaigne:—The *weight* of the cerebellum, both absolutely, and as compared with that of the cerebrum, was adopted as the standard of comparison. This was ascertained in ten stallions, of the ages of from nine to seventeen years; in twelve mares, aged from seven to sixteen years; and in twenty-one geldings, aged from seven to seventeen years. The average weight of the cerebrum in the stallions was 433 grammes; the greatest being 485 gr., and the least (which was in a horse of ten years old) being 350. The average weight of the cerebellum was 61 gr.; the greatest being 65 gr., and the least 56 gr. The average proportion borne by the weight of the cerebellum to that of the cerebrum, was, therefore, 1 to 7.07; the highest (resulting from a very small cerebrum) being 1 to 6.25; and the lowest (resulting from an unusually large cerebrum) being 1 to 7.46. Throughout it might be observed, that the variation in the size of the cerebellum was much less than in that of the cerebrum. In the twelve mares, the average weight of the cerebrum was 402 gr.; the highest being 432 gr., and the lowest 336 gr. That of the cerebellum was 61 gr.; the highest being 66 gr. (which was in the individual with the smallest cerebrum), and the lowest 58 gr. The average proportion of the weight of the cerebellum to that of the cerebrum was 1 to 6.59; the highest being 1 to 5.09, and the lowest 1 to 7. The proportion was, therefore, considerably higher in the perfect female than in the perfect male. In the twenty-one geldings, the average weight of the cerebrum was 419 gr.; the highest being 566 gr., and the lowest 346 gr. The average of the cerebellum was 70 gr.; the highest being 76 gr., and the lowest 64 gr. The average proportion was, therefore, 1 to 5.97; the highest being 1 to 5.16, and the lowest 1 to 7.44. It is curious that this last was in the individual which had the largest cerebellum of the whole; but the proportional weight of the cerebrum was still greater. Bringing together the results of these observations, they are found to be quite opposed

to the statement of Gall. The weight of the cerebrum, reckoning the cerebellum as 1, is thus expressed in each of the foregoing descriptions of animals :—

	Average.	Highest.	Lowest.
Stallions . . .	7.07	7.46	6.25
Mares . . .	6.59	7.00	5.09
Geldings . . .	5.97	7.44	5.16

The average proportional size of the cerebellum in geldings, therefore, is so far from being *less* than that which it bears in entire horses and mares, that it is positively greater ; and this depends not only on diminution in the relative size of the cerebrum, but on its own absolutely larger dimension, as the following comparison of weight will show :—

	Average.	Highest.	Lowest.
Stallions . . .	61	65	56
Mares . . .	61	66	58
Geldings . . .	70	76	64

The difference is so remarkable, and appears, from examination of the individual results, to be so constant, that it cannot be attributed to any accidental circumstance arising out of the small number of animals experimented on. The average weight of the cerebellum in the ten stallions and twelve mares is seen to be the same ; and the extremes differ but little in the two ; whilst the average in the gelding is more than one-seventh higher, and the *lowest* is considerably above the average of the preceding, while the highest far exceeds the highest among the entire horses. It is curious that Gall would have been much nearer the truth, if he had said that the dimensions of the *cerebrum* are usually reduced by castration ; for it appears from the following table that this is really the case :—

	Average.	Greatest.	Least.
Stallions . . .	433	485	350
Mares . . .	402	432	336
Geldings . . .	419	566	346

The weight of the cerebrum of the largest gelding is far above the highest of the stallions ; but it seems to be an extraordinary case, as in no other was the weight above 490 gr. If this one be excluded, the *average* will be reduced still further, being then about 412 ; this will be seen, on looking over the whole table, to give a very fair idea of the usual weight in these animals, which is therefore *less* by about one-eighth than the average of the stallions. The increased size of the cerebellum in geldings may perhaps be accounted for, by remembering that this class of horses is solely employed for its muscular power, and that the constant exercise of the organ is not unlikely to develope its size ; whilst stallions, being kept especially for the purpose of propagation, are much less applied to occupations which call forth their motor faculties.

Functions of the Cerebrum.

In regard to certain general positions, there is little difference of opinion amongst physiologists in reference to this much-controverted subject ; and it will be desirable to inquire what may be considered as firmly established, before we proceed with details of a more questionable nature. We shall, as before, apply to comparative anatomy, to experiment, and to pathology, for our chief data. Any general inferences founded *only* upon observation of the phenomena presented by man must be looked upon with suspicion ; since every advance in comparative physiology leads us to perceive, how close is the functional relation between organs that are really of analogous nature in different classes of animals. The first general proposition which I shall lay before you is, that the cerebrum is the sole instrument of *intelligence*,—by which term I imply the voluntary adaptation of means to ends, in a manner implying a perception of the nature of both. As I have already pointed out, the actions performed by the lower animals are often such as to leave us in doubt, whether they are the result of a mere instinctive impulse, or of an intelligent adaptation of means to ends ; and we are guided in our determination chiefly by the uniformity of these actions in the several individuals of the same species. If we analyze any of our own instinctive actions, we shall perceive the same independence of design on our own parts, as that which we attribute to the lower animals. No one would assert that the tendency to sexual intercourse is the result of a knowledge of its consequences, and of a voluntary adaptation of means to ends ; or that, if we can imagine a man newly coming into the world in the full possession of all his powers, he would wait to eat when he was hungry, until experience had taught him that the swallowing of food would relieve the uneasy feeling. It has been already shewn that, in the infant, the act of sucking may be performed even without a cerebrum ; and for this and other similar actions, therefore, it is doubtful whether consciousness is a requisite condition. Adult animals, whose cerebral hemispheres have been removed, will eat food that is put into their mouths, although they will not go to seek it ; and this is the case with many human idiots. When the functions of the brain are disturbed, or in partial abeyance, as in fever, we often see a remarkable return to the instinctive propensities in regard to food ; and the physician frequently derives important guidance as to the patient's diet and regimen, particularly as to the administration of wine, from the inclination or disinclination which he manifests. The *intelligence* of an animal may be further estimated by his degree of educability,—that is, by the degree in which his

natural habits may be changed by the influence of man, and the complication of the mental processes which he appears to perform under his new circumstances. We all know that insects,—the most active of all Invertebrate animals, are but little susceptible of such influence. I have never heard of a case in which an insect of any kind could be taught to recognize any one who had been in the habit of feeding it, or to show any other unequivocal mark of intelligence. Bees and other insects, which exercise much art in the construction of their habitations, and which execute a variety of most curious contrivances beautifully adapted to variations in their circumstances, appear to be entirely guided in their operations by instinct; since all bees will do the same in the same circumstances: we do not find one community clever, and another stupid: and for a bee to be destitute of its peculiar tendency to build at certain angles, would be as remarkable as a human being without a tendency to eat. The only manifestation of educability which I have ever noticed, during a pretty long familiarity with the habits of bees, is the acquirement of a power of distinguishing the entrance of their hive from that of others around. When a swarm is first placed in a new box, and the bees have gone forth in search of food, they often seem puzzled on their return, as to which is their own house; more especially if there be several hives, with similar entrances, in one bee-house; and it has been proposed to paint these entrances of different colours, in order to enable the bee to distinguish them more readily. In a short time, however, even without such aid, the bees are seen to dart from a considerable height in the air directly down to their proper entrances; shewing that they have *learned* to distinguish these by a memorial power. This I have observed most remarkably, in a case in which a hive is placed in the drawing-room of a house, the entrance to it being beneath one of the windows; the adjoining houses have windows precisely similar, except in the absence of this small passage; and I have often noticed that, when a new stock has been placed in this hive, the bees are some days in learning the exact position of their house, considerably annoying the neighbours by flying in at their windows. In insects, as I have already stated, we can discover little or nothing that is analogous to the cerebrum of Vertebrata; the cephalic ganglia manifestly corresponding chiefly to the ganglionic enlargements at the upper end of the medulla oblongata, which are connected with the organs of special sensation.

On comparing birds with insects, we at once see a very remarkable difference in the character of their actions. Their instinctive tendencies are of much the same kind; and

the usual arts which they exhibit in the construction of their habitations, in procuring their food, and in escaping from danger, must be regarded as instinctive, on account of the uniformity with which they are practised by different individuals of the same species, and the perfection with which they are exercised on the very first occasion. But in the adaptation of their operations to peculiar circumstances, they display a variety and fertility of resource far surpassing that which is manifested by insects; and it is not doubted, by those who have attentively observed their habits, that in such adaptations they are often guided by real intelligence. This must be the case, for example, when they make trial of several means, and select that one which best answers its purpose; or when they make an obvious improvement from year to year in the comforts of their dwelling; or when they are influenced in the choice of a situation, by peculiar circumstances, which, in a state of nature, can scarcely be supposed to affect them. The complete domesticability of many birds is in itself a proof of their possessing a certain degree of intelligence; but this alone does not prove the possession of more than a very low amount of it; since many of the most domesticable animals are of the humblest intellectual capacity, and seem to become attached to man, principally as the source on which they depend for the supply of their animal wants. This is the case with most herbivorous quadrupeds, and with rabbits, guinea-pigs, &c.; as well as with the gallinaceous birds. The attachment of the dog or the elephant is evidently of a much higher kind, and involves a much larger number of considerations; and their actions are evidently the result, in many instances, of a complex train of reasoning, differing in no essential respect from that which man would perform in similar circumstances. The epithet, "half-reasoning," commonly applied to these animals, does not, I think, express the whole truth; for I am satisfied that their mental processes are of the same kind with those of man, and differ more in the degree of control which the animal possesses over them, than they do in their own character. We have no evidence that any of the lower animals have a voluntary power of guiding, restraining, or accelerating their mental operations, at all similar to that which man possesses; these seem to be of very much the same character as that which we perform in our dreams, different trains of thought commencing as they are suggested, and proceeding according to the usual laws, until some other disturb them. Although it is customary to regard the dog and the elephant as the most intelligent among the lower animals, I am not certain that we do so with justice; it is very possible that we are misled by that

peculiar attachment to man which in them must be termed an instinct, and which enters as a motive into a large proportion of their actions; and that, if we were more acquainted with the psychical character of the higher *Quadrumana*, we should find in them a greater degree of mental capability than we now attribute to them. One thing is certain,—that the higher the degree of intelligence we find characteristic of a particular race, the greater is the degree of variation which we meet with in the characters of individuals; thus every one knows that there are stupid dogs and clever dogs, ill-tempered dogs and good-tempered dogs, as there are stupid men and clever men, ill-tempered men or good-tempered men. But no one could distinguish between a stupid bee and a clever bee, or between a good-tempered wasp and an ill-tempered wasp; simply because all *their* actions are prompted by an unvarying instinct.

Before enquiring into the comparative size of the brain, in different animals, it is desirable to obtain a general notion of its structure. Three principal sets of fibres may be distinguished in the white or medullary substance, of which the great mass of it is composed. These are the ascending fibres, which proceed from the sensory tract, and diverge from the thalami optici to the periphery; the descending fibres, which converge from the periphery towards the corpora striata, and then pass downwards into the motor tract; and the commissural fibres, which establish the connection between the various parts of the periphery and of the substance of the brain. It is on the very large proportion which the commissural fibres bear to the rest, that the bulk of the brain of man and of the higher animals chiefly depends; and it is easy to conceive that this condition has an important relation with mental operations, whatever be our view of the functions of different parts of the brain. The different relative distribution of the grey and white matter in the cerebrum, from that which is elsewhere presented to us in ganglionic masses, naturally suggests the enquiry, how far this corresponds with what has been stated of their probable functions. It may be remarked, in the first place, that we have no evidence whatever, that the endowments of the fibres are in any degree changed, by passing from the nervous trunks into the brain; and that it is only where they terminate in the grey substance, that such an alteration takes place in their structure, as to warrant an assumption, in the absence of other evidence, that their function also is altered. The amount of ordinary vascular action in the grey substance, as compared with that which takes place in the white, is an important circumstance in favour of the view, that it is the part in which all

changes originate, and that the fibrous portion, like the trunk of the nerve, serves only to conduct or transmit the effects of those changes. This position derives additional support from the effects of disease. It has been frequently remarked that, if we compare those cases of cerebral disease in which there is delirium, with those in which it does not occur, we shall find that it is most common in cases in which there is an inflammatory affection of the surface, or of the membranes,—extending from them into it; whilst, in deep-seated inflammation, the most important symptoms are those which are derived from sympathetic affections of the muscular system. It has been even proposed to establish a diagnosis between inflammation of the membranes (especially of the arachnoid) and inflammation of the substance of the brain, upon this general fact; but it is to be remembered that (to use the words of Lallemand) “it is impossible that the arachnoid should be inflamed, without the surface of the brain in contact with it being also affected; but its tissue not being altered, there merely results from this vicinity exaltation in its functions.” All the cases, therefore, which have been referred to in support of this diagnosis, really tend to establish the proposition, that the peripheral portion of the cerebrum is the part really affected. It is absurd to suppose that inflammation of the membranes, without any abnormal condition of the brain itself, can seriously affect the mental operations. It has been further remarked that arachnitis of the convexity of the brain is more attended with delirium and other symptoms of excitement, than similar inflammation affecting the base, in which coma supervenes earlier, with little or no previous disturbance of the intellect: this, too, corresponds with the doctrine I have been laying before you; since the influence of any effusion about the origins of the nerves and the medulla oblongata is well known to be prejudicial to their functions as conductors, even entirely suspending them, whilst, from the inferior vascularity of these parts, they are not liable to manifest symptoms of excitement, from the contiguity of an inflamed membrane. In fact, inflammation of the white substance of the brain is itself rather attended with a state of torpor, or of partial suspension of its usual operations, than of excitement; irregular convulsive actions are not unfrequently seen, as a result of it; but these are often manifested, when the power of the will over the muscles is destroyed. It may not be difficult to account for this difference of symptoms, by reflecting, that a large proportion of the medullary substance of the brain consists of a sort of extremely delicate cellular tissue, by which the fibres are connected together, and through which the

blood-vessels are distributed; and that it is probably in this that the principal changes take place, of which the early stages of the inflammatory process consist: these changes, being accompanied by turgescence of the vessels, and by effusion into the tissue surrounding them, must occasion a degree of pressure on the enclosed fibres, which shall destroy their conducting power, and shall thus cut off the body from connection with the centre of the intellectual operations; whilst they may at the same time give rise to many irregular and involuntary movements of the muscles to which the fibres thus affected are distributed.

This view is further supported by the researches of Foville on the alterations of the brain which are connected with insanity. His observations are deserving of great confidence, both for the sake of his own high character and attainments, and on account of the careful manner in which they were made. To avoid trusting to his memory for comparison, Foville has been in the habit of examining the brain of a person who died without any disease in this organ, at the same time with that of one who died insane. In acute cases of insanity, he frequently found the cineritious portion intensely red, but without adhesion to the membranes; whilst in chronic cases, he found the cortical substance indurated and adherent to the membranes. In nearly all cases of insanity accompanied with general paralysis, he has found the white portion of the brain injected and indurated; and he conceives that the fibres had become adherent to each other. It has been supposed by Calmeil that the paralysis of the insane is connected with disease of the cineritious substance; but Foville states that he and his colleagues have made many hundreds of observations on cases, in which there were well-marked alterations of the cortical substance of the brain, without any other manifestations during life than disorder of the intellect. This view is supported by Bouillaud, and by several other eminent pathologists; as is also the other part of the proposition, that morbid alterations in the medullary portion are connected with disorder in the transmission of motor impulses to the muscles.

It is important to bear in mind this induction,—for such I think we may regard it,—when forming our opinions upon the functions of the cerebrum in general, or of its several parts, from the various data supplied to us by comparative anatomy, and by experimental and pathological enquiry. For in regard to the first of these sources it is to be remarked, that the *size* of the brain does not, considered alone, afford a means of judgment as to its power. The quantity of grey matter on its surface should rather be our guide; and this we may judge of, not

only by the depth of the layer, but by the complexity of the convolutions by which the surface is extended. In no class, save in Mammalia, do we find the surface marked with convolutions; and in general we do not meet with that fissure between the hemispheres, which greatly increases the extent of surface. In forming comparisons as to the connection between the size of the brain, and the intelligence, in different animals, we must not be at all guided by its simple proportional dimensions; since it is very evident that it is rather the proportion of the bulk of the brain to that of the whole body, upon which we should found our comparison. But even this is not altogether a safe guide; and many physiologists have endeavoured to compare the size of the brain with the aggregate bulk of the nerves proceeding from it. This is, I think, a much fairer measure; but it cannot be taken without great difficulty. I am disposed to think that, for all practical purposes, the comparison of the bulk of the brain with that of the spinal cord will answer very well. The following table, the materials of which are drawn from Serres' Comparative Anatomy of the Brain, exhibits the three diameters of the brain of a number of different animals, and that of the spinal cord at the second cervical vertebra; I should myself rather propose the whole weight of the brain, compared with the weight of a certain proportion of the spinal cord in the cervical region, as a more simple criterion. The three last columns present, in round numbers, the three diameters of the brain, reckoning that of the spinal cord as 1, for the sake of easy comparison. (See Table top of next page.)

As might be expected, the brain of man bears by far the highest proportion; but this proportion is not so superior in the transverse and vertical diameters, as in the antero-posterior; in fact, in the proportion of the vertical diameter, the brain of man is equalled by that of the dolphin, and nearly so in that of the transverse diameter. In the complexity of the convolutions, however, and in the thickness of the grey matter, the cerebrum of man far surpasses that of this cetaceous animal. In these respects the higher Quadrumana present the nearest approach to it; but they are much inferior in size. In descending the scale of Mammalia, there may be observed a gradual simplification in the general structure of the brain, depending upon a great diminution in the amount of commissural fibres; until in the Marsupialia the brain presents nearly the same condition which it offers in birds,—even the corpus callosum, or great transverse commissure, being deficient. These animals manifest a much lower degree of intelligence than many birds evidently possess; and it is interesting to remark, that their cerebral

	Diameter of Spinal Cord.	DIMENSIONS OF CEREBRUM.			Proportional Dimensions.		
		Ant.-post.	Transv.	Above downward.			
Man	1100	17000	7500	9000	1—16 $\frac{1}{3}$	1—6 $\frac{5}{8}$	1—8 $\frac{1}{5}$
Dolphin	1000	9500	5850	8200	1—9 $\frac{1}{2}$	1—5 $\frac{4}{5}$	1—8 $\frac{1}{5}$
Mandrill	950	8100	3200	4900	1—8 $\frac{1}{2}$	1—3 $\frac{1}{3}$	1—5
Tiger	1600	9400	4250	6400	1—5 $\frac{7}{8}$	1—2 $\frac{5}{8}$	1—4
Dromedary	1900	10500	5050	5800	1—5 $\frac{1}{2}$	1—2 $\frac{5}{8}$	1—3
Kangaroo	1200	5300	2350	3800	1—4 $\frac{2}{5}$	1—2	1—3 $\frac{1}{8}$
Vulture	800	3200	2200	1550	1—4	1—2 $\frac{3}{4}$	1—2
Falcon	500	1900	1450	1200	1—3 $\frac{4}{5}$	1—3	1—2 $\frac{2}{5}$
Swallow	175	1000	600	550	1—5 $\frac{5}{7}$	1—3 $\frac{1}{2}$	1—3 $\frac{1}{7}$
Pie	450	2000	1400	1200	1—4 $\frac{2}{5}$	1—3	1—2 $\frac{2}{3}$
Turkey	500	1750	1250	1200	1—3 $\frac{1}{2}$	1—2 $\frac{1}{2}$	1—2 $\frac{2}{5}$
Parroquet	400	2900	1400	1700	1—7 $\frac{1}{4}$	1—3 $\frac{1}{2}$	1—4 $\frac{1}{4}$
Tortoise	300	1600	500		1—5 $\frac{1}{3}$	1—1 $\frac{3}{5}$	
Crocodile	300	800	500		1—2 $\frac{3}{5}$	1—1 $\frac{3}{5}$	
Viper	200	600	300		1—2	1—1 $\frac{1}{2}$	
Frog	300	500	400		1—1 $\frac{2}{5}$	1—1 $\frac{1}{3}$	
Shark	700	2300	1100		1—3 $\frac{1}{3}$	1—1 $\frac{4}{7}$	
Cod	575	725	800		1—1 $\frac{1}{4}$	1—1 $\frac{2}{5}$	
Lamprey	275	400	300		1—1 $\frac{1}{2}$	1—1 $\frac{1}{4}$	
Angler	400	400	300		1—1	1— $\frac{3}{4}$	

hemispheres are proportionably smaller than those which we find in many birds; the diminution in their relative size not being counterbalanced (as it is in some other instances) by increased complexity of structure. In the class of Birds, we observe that the Vulture and the Falcon, whose predacious instincts give them a considerable amount of general energy, are much inferior in the size of their brains to the Insessorial birds, which are more intelligent; and that of all there is none in which the brain is so proportionally large as it is in the Parrot tribe, the educability of which is familiar to every one; whilst the easily-domesticable, but unintelligent Turkey, has a brain of scarcely half the proportional size. The very small size of the cerebrum in Reptiles and Fishes completely harmonises with the same view; these animals presenting for the most part but feeble indications of intelligence. Among Reptiles, the Tortoise has a cerebrum comparable in length to that of Birds; but its breadth and depth are far less. The largest cerebrum among Fishes are found in the Shark tribe, the superior intelligence of which is well known to those who have had the opportunity of observing their habits; and it is interesting to remark, that their surface occasionally presents an appearance of rudimentary convolutions.

CASE OF CÆSAREAN SECTION.

BY

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[For the London Medical Gazette.]

MARY ANN JONES, the subject of the following operation, was a poor woman 39 years of age, of the lymphatic temperament, swarthy complexion, and of a peculiar bodily conformation. Her stature when married was five feet and one or two inches. The forehead was low and receding, the alveolar arches large, and the lower part of the face prominent, so as to make the facial angle more than ordinarily acute. The limbs were long in proportion to the size of the person, the spinal column short, the chest shallow, and the abdomen always prominent. She was married in 1825, and delivered of her first child in December of the same year. Her second delivery took place in 1828; third in April 1831; and her fourth in April 1833. Each of these labours was natural, and comparatively easy, and her recovery speedy. Three of these children are now living, and

in good health; the fourth died early from marasmus. During this last pregnancy she became affected with severe rheumatic pains in the limbs and back, and especially around the pelvis; the labour, however, was easy and quick, like the preceding ones, and she had a speedy recovery to a tolerable state of health, except that the pains did not entirely leave her. During the whole of her next pregnancy, which had arrived at its full period in November 1834 (one year and a half after the fourth), she suffered severely from the pains above mentioned, and her stature became considerably diminished. She was then under the care of Mr. Clough, as a patient of the Manchester Lying-in Hospital. Mr. Clough, who was called in for the first time only a few days before symptoms of labour came on, found the deformity of the pelvis so considerable—the antero-posterior, at its widest part, as far as could then be ascertained, being under two inches—that it became a question whether he would not be obliged to have recourse to the Cæsarean section as the safest, if not the only available means of delivery. At a consultation, however, of the medical officers of the hospital, one only coincided with him in opinion, the majority recommending delivery by embryulcia, which was accordingly performed, and accomplished with considerable difficulty. Her recovery was favourable, and in six weeks she was again able to go about her house.

During the next four years she was pregnant four times, and had premature labour induced at the end of the fourth month each time,—twice by Mr. Clough, and twice by me; the patient having been placed under my care in consequence of Mr. C. being obliged to leave town on account of ill health.

Many attempts were made during her late (10th) pregnancy to bring on abortion, but without success. The ergot of rye, which before had produced the most desirable results, completely failed, although repeatedly and fully tried*. Perforation of the membranes was also several times attempted, both by myself and a professional friend, an old and experienced practitioner, but with no better success. By this

latter means a coloured discharge, and other signs of approaching labour, were twice produced, and continued several days, but gradually subsided; and utero-gestation having now arrived near the end of the eighth month, was allowed to go on without further interference. It would have been dangerous to employ a sharp-pointed instrument for perforation, as it was with the greatest difficulty that the hand could be introduced, so as barely to touch the os uteri with one finger, and consequently could not be used efficiently as a guide for an instrument of this kind.

The patient's health had suffered considerably during the last two or three months of her pregnancy. The pains in the back and limbs, and around the pelvis, extending along the sciatic and crural nerves, were very severe, and for the relief of which she was prescribed an anodyne medicine, to be taken during the day, with an increased dose of it every night at bedtime. Her appetite for food was very defective, and the stomach exceedingly irritable.

It was thought by those who were in the habit of seeing her frequently, that her stature had become diminished during this pregnancy. This circumstance could not, however, be confidently asserted, as a measurement had not been previously made. She was, about six or eight weeks before the operation, exactly four feet and three inches high, thus shewing a diminution of nine or ten inches at least from her ordinary stature.

I was summoned to attend her on the evening of the 14th of August, 1840, gestation being then at its full period. The pains were slight and lingering, and by the aid of an anodyne draught she passed the night comfortably. On the following morning the pains were still infrequent and trifling, but of a character indicating that labour was near. Four of my professional friends were consequently requested to meet me in consultation, at 12 o'clock. Two of them only could attend at that hour, and as the pains were scarcely more urgent than when she was visited in the morning, we agreed to meet at 7 o'clock in the evening. The patient had been in active labour two or three hours when we arrived. The pains were strong and fre-

* It is due to Mr. Clough to state, that his employment of the ergot of rye in this instance was before its specific power in such cases was generally known.

quent, and evidently occasioned great suffering. There had been no discharge except of mucus, so that the membranes in all probability remained entire. The frequent examinations which I had previously made of the pelvis had enabled me to ascertain with tolerable certainty to what extent the deformity had proceeded, and I now stated my opinion to be, that the greatest available space at the upper aperture of the pelvis, in its antero-posterior diameter, was not more than one inch and a quarter: in this opinion several who had also seen the patient with me before fully coincided. It was with great difficulty, however, that an efficient examination could be made, as the contraction in the cavity of the pelvis, and at its outlet, precluded the introduction of the hand to a sufficient extent. The tuberosities of the ischia were found to approach anteriorly to within a very short distance of each other, and the outlet was much encroached upon by the apex of the sacrum, which had advanced considerably towards the arch of the pubis. After each had again made a careful examination, we retired into another room for consultation. The conclusion was, that the dimensions of the pelvis were too small, not only for instrumental interference by the natural passages, but also for the fragments of the foetal cranium to pass, had perforation and separation of the bones been practicable. The Cæsarean section was therefore decided upon, and recommended to be performed without delay. I now returned to the room where the patient was lying, and informed her that it would be impossible to effect delivery in the usual way, and that the only means that could afford her or her child a chance of being saved was the Cæsarean section, the nature of which I briefly explained to her. She consented without a moment's hesitation to submit to any operation that should be deemed necessary, and hoped it might be done as soon as possible.

Catheterism, and other necessary preliminaries, having been attended to, the patient was placed upon her back, her shoulders a little raised on pillows, and the knees flexed over the edge of the bed, when I placed myself, for the sake of convenience, in the kneeling posture. Two assistants were stationed near her shoulders to keep her still, and

one on each side of her near to me, each holding a candle; another was prepared to supply whatever instruments, sponges, &c. might be needed. The external incision was made in the line of the uterine axis, commencing about four inches above, and extending nearly the same distance below, the umbilicus, and running a little obliquely from within outwards: its upper extremity being half an inch, and its lower an inch and a half to the left side of the linea alba. A short incision was first made through the whole thickness of the abdominal parietes, which were extremely attenuated, and continued by means of a director and probe-pointed bistoury. The cut into the uterus corresponded in direction with the external one, and was about seven inches long. A small incision was first made with the scalpel, and continued with the probe-pointed bistoury, the fingers being used to direct it instead of the grooved probe. The placenta being attached at this part of the womb was immediately torn through with the fingers, and the child and it extracted without the slightest difficulty. The uterus, the parietes of which, in its distended state, were not thicker than shoe-leather, immediately contracted to the size of a child's head, and its walls became about three-quarters of an inch in thickness. Some of the small intestines now protruded at the upper part of the wound, but were immediately replaced, and held in by Mr. Fawdington. The parts were then sponged, the external wound brought together, and secured by six interrupted sutures, adhesive plasters in long straps, and a broad circular bandage. The lower fifth of the wound was left uncovered by adhesive plasters, in order to allow any discharge to pass freely from the interior: a piece of spread lint and a fold of the bandage being all that was thought necessary at this part. The loss of blood amounted to about eight or ten ounces; it issued principally from the placenta while being torn through. There was very little discharge, indeed scarcely an oozing of blood from the divided edges, either of the abdominal parietes or the uterus.

The woman bore the operation almost without a murmur, and said she had not suffered more than she should have done from two or three ordinary labour pains. The child, who was alive and healthy, was extracted in less than two

minutes after the first incision was made, and the whole operation completed, and the patient in bed, in twelve or fifteen minutes from the commencement. The pulse was not materially affected, and the patient was lively and talkative directly after being put to bed. A quarter of an hour after the operation, which was at 9 o'clock P.M., three grains of solid opium were administered, and directed to be repeated every four, five, or six hours. She passed a comfortable night: slight uterine pains came on three or four hours after the operation, and she had some lochial discharge at the same time *per vias naturales*. There has been a little oozing from the lower part of the wound.

The remainder of the case shall be given, as nearly as possible, verbatim, but a little abbreviated, from my notebook.

Twelve hours after the operation.—The patient feels free from pain, and tolerably comfortable; she has taken freely of gruel several times during the night, and has had a little tea and bread and butter for breakfast; the urine has passed freely. Strict injunctions had been given that I should be sent for whenever a desire to void the urine became urgent. She, however, got out of bed, even without the knowledge of the nurse, while she was absent from the room. The opium has not produced more than a tranquillizing effect, although nine grains have been taken at three doses; no thirst; pulse 96.

The opium to be continued in doses of two grains every two hours.

End of the first day.—Has been pretty easy and free from pain during the greater part of the day; has taken her food with relish; lochial discharge continues in moderate quantity, *per vias naturales*; the pulse was 92 in the middle of the day—it is now 104; complains of soreness over the region of the stomach, with inclination to be sick.

Opium to be continued.

Second day, morning.—She has been very restless, and has vomited several times during the night, and continues to do so: the matter ejected from the stomach is a thick brown fluid, like yeast, and smells very sour; pulse 136; thirst urgent.

Ordered a saline mixture, to be taken in a state of effervescence every three or four hours.

Evening.—No repetition of the vomiting since morning; thirst abated; no rigors; pulse 112; the abdomen is tender, and a little tympanitic; there have been severe uterine pains, with discharge of clots of blood *per vias naturales*; the bowels have not yet been relieved.

An ounce of castor oil to be taken immediately; the opium to be continued every six hours.

The urine has passed freely, and she has slept several hours during the afternoon; there has been considerable oozing from the wound, so as to moisten the plasters and loosen their attachments; fresh plasters have consequently been applied.

3d day, morning.—Bowels still unmoved; belly distended, but not so tender; has slept during the night from 11 o'clock till 4, and became again composed after taking some gruel and biscuit; thirst not considerable; skin moist; pulse 120: there is constant hiccup and eructation, with inclination to be sick.

Ordered two grains of calomel, to be taken with next dose of opium, with a view to allay the irritability of the stomach; to have an enema administered immediately.

Evening.—The bowels are still unmoved; the enema came away unmixed with fæculent matter; abdomen considerably enlarged, especially in the region of the right and transverse portions of the colon; hiccup and retching continue, but there has been no return of the vomiting; lochial discharge continues, but in small quantity; the tongue is slightly furred, and has two aphthous patches near its tip; no thirst; pulse 120.

To take four grains of calomel and six of extract of colocynth immediately, and the enema to be repeated.

In consequence of the hiccup, and retching, distension of the abdomen, and constant restlessness, with attempts to turn from side to side, the plasters have again become completely loosened, and have been renewed. The edges of the upper half of the wound are in close contact, and appear to have adhered; the lower half is much separated in the intervals between the sutures, and one or two of the threads seem in danger of being torn out. Catheterism was performed, although she had no difficulty in voiding the urine, but complained of un-

easiness in the region of the bladder, arising probably from the costive state of the bowels.

4th day, morning.—The bowels have been freely acted upon three times since midnight; tension and uneasiness of the abdomen much relieved; hiccough abated; thirst considerable; tongue white; pulse 124.

Ordered the opium to be continued in doses of two grains every eight hours; to take barley-water instead of common gruel for beverage.

Evening.—Bowels have been evacuated copiously several times since morning; dejections liquid, but quite fæculent; the abdomen is no longer tense or painful on pressure; the urine has passed freely; tongue white; pulse 120. Her appetite for food has been very good ever since the operation, and she continues to take a proper quantity of gruel, sago, beef-tea, &c.

5th day, morning.—Has had a very restless night owing to the bowels having been too much relaxed; she became very cold in the middle of the night, and continued so for some time, although the weather was oppressively hot. Has taken a pint of gruel with a glass of sherry wine in it since midnight. Pulse 128.

To take her dose of opium immediately.

Evening.—Bowels have been moved twice since morning; belly quite easy; tongue clean and moist, and the aphthæ have nearly disappeared; thirst abated; pulse 120; has slept several hours. On raising the lint from a part of the wound uncovered by adhesive straps, a little healthy looking pus appeared on the edges—not oozing out from the abdominal cavity: this is the first appearance of suppurative action.

Ordered the opium to be taken in doses of one grain, combined with the same quantity of sulphate of quinine every 6 hours.

6th day, morning.—Has passed a sleepless but not uneasy night; she slept much yesterday; the tongue continues moist and clean; thirst rather more urgent; belly free from tension or pain; the bowels have acted five times during the last twelve hours; pulse 130.

Evening.—As the bowels continued to act too freely, the following mixture was prescribed in the middle of the day, which has arrested the purging, and she now feels much more comfortable:—

R Mist. Cretæ, ℥v.; Conf. Aromat. ʒiss.; Tinct Opii, mxxx.; Tinct. Catechu, ʒj. M. fiat mist. cujus sum. ʒj. q.q. 3tiis horis.

She continues to take her food remarkably well. To-day she took a basin of boiled milk with bread for breakfast; beef-tea, with a glass of sherry wine in it—she preferred them mixed together—at 11 o'clock: two small mutton chops and potatoes, with a little jelly and a glass of wine, at half-past two; and tea and gruel, with a little wine, at separate times during the evening.

7th day, morning.—Has slept well during the night; tongue clean; no thirst; pulse 118; belly soft, and free from pain; the bowels have acted four times since last evening.

To continue taking the astringent mixture, and pills of quinine and opium.

Evening.—Pulse 120; appetite good; the other symptoms the same as they were in the morning; the edges of the wound are red, and begin to throw out healthy granulations; the secretion from it is sanio-purulent. There has hitherto been the greatest difficulty to keep the edges of the wound in apposition at its lower half, although the plasters are good, and such as are in common use at the Royal Infirmary: they are applied in long straps, so as to reach round the body. But the heat of the skin, the constant oozing from the edges of the wound, and the perspiration, the continual movements of the abdominal parietes following the respiratory actions, together with other more powerful impulsions, as from coughing, straining, turning about from one side to the other, in spite of the strictest injunctions to keep quiet, all tend to loosen and soften the plasters. The abdomen being so pendulous, too, lying on the upper part of the thighs, and appearing as if all the viscera were impacted in its lower region, render it difficult to secure the parts properly.

To take six ounces of porter twice a day.

8th day, morning.—Has slept well during the night; tongue clean; pulse 120; bowels regular; considerable oozing from the wound.

Evening.—Pulse 120; other symptoms the same as in the morning; the appetite continues very good; she has taken two hearty meals of animal food, and a pint bottle of porter, besides gruel, with wine, at intervals during the

day; the plasters have become completely loosened, so as to allow the edges of the wound to fall apart. Instead of the common bandage I have this evening applied a broad belt, which buckles by three straps in front, such as labouring men wear.

Omit the astringent mixture.

9th day, morning.—Has had a comfortable night; bowels regular; tongue clean; pulse 126.

Evening.—All the symptoms the same as in the morning. She has taken her meals quite as well, and with as much relish, as she did yesterday. The edges of the wound have thrown out healthy granulations the whole length, and secrete a healthy pus; but in consequence of several of the sutures having been torn out, and the difficulty of keeping the edges at the lower half in apposition, no healing has yet taken place. There was not the slightest disposition in the parts to heal by the first action, as the edges at their upper third or half were kept well in contact until the period of reaction was fairly past, and no healing was effected. The upper third of the wound seemed to be healed, as mentioned in the notes at the end of the third day, but afterwards separated. I have introduced four new sutures, and secured the wound well together, by means of a different kind of plaster, recommended by Dr. Robinson, which is composed of equal parts of the *emplastrum labdani comp.*, and *emp. saponis*, spread upon strong canvass. At this dressing the wound was very large. A small knuckle of bowel, the breadth of a sixpence, covered with granulations, was insinuated betwixt the edges at the upper part; the granulating edges of the uterus were seen below. Firm adhesions are formed between the neighbouring viscera and the parietal peritoneum, near the surface of the external wound. These adhesions were noticed on the 3d day.

10th day, morning.—Has passed a restless night, from pain in the hips and back; pulse 120; tongue clean.

Evening.—Pain in the hips continues; pulse 120; tongue clean and moist; no thirst. Appetite as usual.

Ordered an *emplastrum belladonnæ* to be applied over the ischiatic foramen on each side, being the parts to which the pain is referred.

On raising the plasters from the lower part of the wound, it seems much contracted, and not more than three inches in length from its upper to its lower extremity. The labouring man's belt, properly adjusted, forms a most excellent bandage; and the new plaster maintains the edges of the wound in perfect contact, and is not affected either by the moisture or warmth of the parts.

11th day, morning.—Has passed a comfortable night; tongue clean; no thirst; pulse 126; abdomen easy; the bowels have acted three times.

Ordered a little arrow-root with brandy, to be taken occasionally.

Evening.—The appetite remains quite as good as before. To-day she took coffee, with bread and butter, and an egg, for breakfast; mutton chops and potatoes, with porter, for dinner, and gruel with a little brandy at intervals during the day. She has taken three half pints of porter since morning. The bowels have acted too freely to-day, but without pain; tongue clean and moist; no thirst; pulse 130. The pain of the hips has entirely left her.

Ordered an ounce of the astringent mixture, as prescribed on the sixth day, to be taken immediately.

12th day, morning.—Has passed a sleepless night, but remains without pain; abdomen quite easy; bowels have acted only once; tongue moist; pulse 120. She looks very haggard, and much emaciated.

Evening.—She has been restless all day, and feels exhausted for want of sleep. She complains of her bowels being griped and uneasy. The tongue is clean, but preternaturally red. These symptoms may possibly have arisen from the action of the quinine, which is ordered to be omitted.

To take one grain and a half of opium, and an ounce of the astringent mixture at bed-time.

There has been no perceptible discharge from the uterus for several days.

13th day, morning.—She has passed a very comfortable night, and feels quite refreshed; the bowels are quite easy; tongue clean and moist; pulse 120.

Evening.—She has taken her food heartily during the day, and is cheerful and in high spirits. The bowels are easy and regular; tongue clean; is

quite free from pain, and seems every way to be in a favourable state, except that the pulse remains at 120 beats in the minute.

14th day, evening.—She was disturbed during part of last night by the pain in the hips and along the right thigh, which has continued to trouble her occasionally during the day. Her bowels, however, are easy and regular; tongue clean; appetite good; pulse 116.

Ordered a grain and a half of opium to be taken at bed-time.

15th day, evening.—The pain entirely left her last night, and she slept well. The appetite remains good; tongue clean; pulse 110. As the bowels have been rather too much relaxed, she is ordered to take a dose of the astringent mixture. The wound looks remarkably healthy; the upper part of it has healed by granulation.

16th day, morning.—Has slept well during the night; the bowels are easy; tongue clean; pulse 110; appetite good.

Evening.—She has taken very little sustenance since morning, and feels languid and in low spirits. The tongue is clean, but dry; mouth parched; pulse 130, and feeble. She has no pain. On dressing the wound, the edges have become quite flabby since the last dressing, and the granulations look pale. It is difficult to account for this very sudden and unfavourable change.

Ordered an ounce and a half of port wine to be taken every four hours.

17th day, morning.—She has passed a comfortable night, and is free from pain; the bowels are regular; tongue not so dry; pulse 104, with a little more volume.

Evening.—She seems to be much improved since morning. The appetite is better.

18th day, evening.—The pain of her hips disturbed her rest a little last night, but she is quite free from it now, and is better in every respect; pulse 124.

To take a grain of opium at bed-time, and to continue the wine.

19th day.—She is still improving: pulse 110; the wound looks well, and the granulations are plentiful, and healthy in appearance.

To continue the wine.

719.—XXVIII.

She sat up about an hour this afternoon, and was in high spirits.

20th day.—Still improving. Appetite, good; tongue clean; bowels regular; indeed there remains scarcely an unfavourable symptom, except that the pulse is again at 120, without any assignable cause.

To take one grain of sulphate of quinine every four hours.

21st day.—She continues improving. Pulse 120; tongue a little aphthous on one side. The wound looks healthy, and the process of healing is going on favourably.

22d day, morning.—Has had a restless night from pain in the hips, which is referred to the ischiatic region. On making a minute examination no swelling or tenderness is discoverable about the parts complained of. It appears to commence about the sacrum and sacro-sciatic nerves, and extends along their course down the thigh. Tongue moist, but aphthous; pulse 115.

Ordered the mel boracis to be applied to the aphthæ.

Evening.—She has not taken her food so well to-day; the pain of the hips continues severe; tongue dry and red; pulse 120.

Ordered one grain of opium to be taken at bed-time, and barley-water or thin gruel to be continued for beverage.

23d day, morning.—Has slept well during the night; tongue no worse; pulse 120.

Evening.—She has had a return of the pain in the hips and back, and does not feel so well as before. The tongue is red and aphthous at its edges, and the throat smarts when wine or porter is taken. Pulse 120, irregular and intermittent.

Omit the quinine. To take the following pills at bed-time: \mathcal{R} . Ext. Hyosciami; Hyd. cum Cret. aa. gr. iv. M. ft. pil. ij.

On dressing the wound the granulations look healthy, but its lower portion has not yet adhered; the uterus is apparently quite healed.

24th day, morning.—Has had a comfortable night, although but very little sleep; tongue still red and aphthous; pulse 106, small and feeble.

Evening.—Has not been so well to-day as yesterday; tongue no better;

pulse 120; appetite impaired; bowels confined.

To take a dose of castor oil, and a grain of opium two hours afterwards.

25th day, morning.—Has had a better night; slept several hours; the bowels have acted twice since the oil was taken; tongue somewhat better; pulse 112, with a little more volume.

Evening.—Bowels have acted several times since morning; pain less; tongue still red, but the aphthæ appear better; pulse 70. The wound looks well, and the healing process appears to be going on favourably.

26th day, evening.—Had a very comfortable night; appetite pretty good; pulse 108 in the morning, now 120. The pain in the back and hips has returned with great severity; the tongue continues aphthous at its tip and edges, and severe smarting is complained of in the throat and gullet when any thing is swallowed.

Ordered a grain of opium and two of Hyd. c. Cret. to be taken every four hours. Continue the mel boracis to the tongue.

27th day, evening.—She was very restless all last night. Mouth and fauces worse; the pain and smarting along the œsophagus is very severe, so that she is not able to swallow any thing except the mildest fluids; pulse 110, irregular and feeble.

Applied the nitrate of silver to the aphthæ.

28th day, evening.—The mouth and throat are a little easier; pain in the right hip and thigh continues; pulse 110, feeble. She has taken a fair quantity of boiled milk, sago, and beef-tea during the day. The wound still looks well, and the suppuration is healthy.

Ordered as much gruel and brandy, mixed in equal proportions, as she can take. To continue the opium without the Hyd. c. Creta.

29th day, evening.—Slept better last night. She has been taking, since morning, fifteen grains of powdered cinchona in an ounce of port wine every four hours. Mouth no better; pulse 120, feeble and irregular. She has taken two pints of gruel and milk with brandy during the day. Bowels relaxed.

To take a draught of the astringent mixture.

30th day, morning.—Has passed a sleepless night; tongue and throat in a very bad state; the wine with cinchona produces intolerable smarting when taken; pulse 120, intermittent. She has taken a pint of gruel with brandy during the night, and the wine and cinchona at regular intervals.

Evening.—Is very low indeed; pulse 126, irregular and feeble; tongue the same as yesterday; ptyalism considerable. She had, this morning, a very copious evacuation together with numerous ascarides. The worms came away in a mass as large as a pullet's egg, enveloped in mucus—a complete nest. The wound was dressed at 12 o'clock; it looks flabby, and the granulations are pale and much reduced. Only one dose of the wine and cinchona has been taken to-day; it seemed to have afforded no benefit whatever, and produced so much smarting, that its continuance was not insisted upon. The pain of the hips and thigh has almost entirely left.

31st day, evening.—Had a restless night from pain in the hips and thigh; tongue dry and tremulous, with an inability to protrude it beyond the teeth; pulse 130, small and intermittent.

Ordered to take as much brandy and gruel as she is able.

32d day, morning.—She is evidently sinking; pulse fluttering and intermittent; tongue dry, red, and glazed. She is very still, and unwilling to speak or move.

Evening.—The tongue appears more moist; pulse 86, with a little more volume, regular; wound the same. Has passed four thick fæculent motions, and on the whole seems better.

33d day.—The patient died this morning at 7 o'clock; 32 days and 10 hours after the operation.

On inspecting the body 13 hours after death, the upper part of the wound, three inches in length, had healed by granulation. A small knuckle, the size of a section of a hazle nut, of the intestinum ileum, had insinuated itself between the edges at this part, but did not protrude beyond; its surface was covered with granulations. The rest of the wound was flabby; and the granulations, which appeared

so luxuriant and healthy a week before, had become partially absorbed. A small cul de sac existed between the lower extremity of the cicatrix and the fundus uteri; this was in progress of being filled up by granulation. The incision into the uterus was at its fundus, and at this time one inch in length; both sides were studded with healthy granulations, a circumstance somewhat remarkable, considering the unhealthy appearance of the external wound. I believe that the wound in the womb had been healed, as previously mentioned in the notes of the 23d day; but subsequently separated at the time when the reparative process ceased to go on externally, and that the adhesions were destroyed by absorption. The womb, which is in my possession, was scarcely larger than it is usually seen to be in its healthy unimpregnated state. The external wound, as well as that in the uterus, was excluded from the general peritoneal cavity by a most complete barricado, formed by adhesion of the parietal and visceral peritoneal surfaces, imperforate at every point, and situated at a distance of from half an inch to an inch from the edges of the external wound. These adhesions were perfect on the fourth day after the operation, and I think earlier. On carefully dissecting away the peritoneum from the fore part of the pelvis on its inner aspect, a gush of well-formed pus flowed out from the side of the right psoas muscle. The cavity containing this fluid was within the sheath of the psoas magnus, and was traced forwards to the body of the pubis, and thence downwards to the cavity of the hip-joint, which was filled with matter of the same kind. The ligamenta teres and transversum were almost entirely absorbed; and the head of the thigh-bone, and corresponding surface of the acetabulum, were denuded of cartilage, and quite carious. From the hip-joint the abscess had taken a course upwards, passing beneath the cotyloid ligament and periosteum, over the body of the pubis to its posterior aspect, the whole of which was also carious. The stomach, bowels, liver, and other abdominal viscera appeared healthy.

[To be concluded in our next.]

ANGINA PECTORIS.

To the Editor of the Medical Gazette.

SIR,

THE obscurity which hangs over the pathology of angina pectoris gives importance to evidence of even a negative character. This, together with the unusually rapid progress and termination of the following case, must plead my apology for intruding it on the columns of your journal.—I am, sir,

Your obedient servant,

JOHN FOSSE HARDING,
M.R.C.S. and F.R.M.C.S.

Spencer Street, Northampton Square,
August 31, 1841.

J. C., æt. 64, full habit and sanguine temperament, applied to me on the 16th inst., while labouring under an attack of intense dyspnœa, which disappeared under the use of aperients, &c. I should have attributed the affection to a functional disturbance of the digestive system, had not a recurrence of the attack on the 20th inst. led me to examine the symptoms with greater attention. I ascertained that the paroxysms were brought on and increased by any muscular effort. The first sensation complained of was an agonising sense of constriction in the upper part of the thorax, succeeded by a painful tingling of the left arm, extending to the extremities of the fingers. These symptoms, together with the dyspnœa, continued to increase progressively for about ten minutes, after which they disappeared as gradually. The lungs were ascertained on auscultation to be perfectly healthy. The impulse of the heart was increased, and the second sound accompanied by a bruit de souffle, which was particularly audible above the left clavicle. The coexistence of these symptoms, without any appreciable lesion, did not allow me to hesitate to diagnose—angina pectoris. Depletion, antispasmodics, full doses of acid. hydrocyan. &c. were persevered in for some days without any apparent relief. On the 25th inst. in consultation, Dr. Latham suggested an opiate at bedtime in addition to the means formerly employed. On the following morning the patient considered himself much worse, and stated that the paroxysms had been more frequent and more intense during the past night than previously. On the evening of the 26th he was again

visited: he had had eight or nine paroxysms during the day; countenance anxious; pulse vibrating. The draught was again administered, and the patient's friends warned of the increased probability of a speedy and sudden termination of his existence. The prognosis was verified by the patient's death on that night, at about two A.M. Having obtained permission to examine the thoracic and abdominal viscera, we proceeded in the evening of the 27th to the autopsy.

The pleuræ were found perfectly free from adhesions, and the pulmonary tissue healthy throughout. The pericardium contained about two ounces of serous fluid, perfectly transparent and free from lymph. On examining the external appearance of the heart, we found it slightly enlarged and loaded with fat. A few opaque patches were observed, as if produced by thickening of the serous membrane investing the heart. The coronary arteries of course received our special attention, and were found in every respect perfectly normal. Neither the osseous degeneration nor calcareous deposits could be ascertained to exist. On laying open the left ventricle, the muscular structure was found in such a state of ramollissement, that the finger could be passed through the parietes without difficulty at any part. The aortic valves were found in every respect healthy, with the exception of a slight hypertrophy of the corpora arantii. The mitral valves presented a cartilaginous thickening, but not to such an extent as to interfere materially with the due performance of their functions. The right cavities of the heart were next examined, but no morbid appearance could be discovered, with the exception of the ramollissement above alluded to, which was common to both sides. The aorta and abdominal viscera were perfectly free from disease. As an examination of the cranial cavity was objected to, we were unable to push our investigations any further.

RUPTURE OF THE RECTUS FEMORIS.

To the Editor of the Medical Gazette.

SIR,

I HAVE known ruptures of the tendo Achillis, plantaris and palmaris longus muscles, without fracture, dislocation,

or laceration, of the surrounding parts; but until I saw the following case, I never had met with a rupture of the tendon of the rectus femoris muscle unattended with fracture, dislocation, or laceration, of the adjacent parts; and if my memory serves me right, I do not think there is a case on record of the kind.

Mr. G. Shindler, a printer, a fine stout hale man, was induced to join his son in walking to Woolwich, a distance of five miles, to see the launch of her Majesty's ship, the Trafalgar. In returning, both of them ran some distance, when from a sudden trip of the foot, Mr. Shindler fell to the ground. He heard a loud snap just above the knee-pan, and nearly fainted from the excessive pain. With difficulty he was got up, and removed home, when he applied hot fomentations to the thigh, gave it rest, and thereby recovered from the severity of the injury. Six weeks afterwards he for the first time applied to me, stating that he had to walk to and from his shop, which caused great pain in the thigh, and he stated, in addition, that there was a very large swelling, which, on examination, I found to be the rectus femoris muscle, drawn one-third up the thigh, and with a corresponding depression above the patella. In these days it might have been supposed that no one would be at a loss to restore the injury, hearing and seeing how tendons are divided with so much *sang froid*, and with such reported good success. In this I beg leave to say, that I do not call in question that great improvement in modern surgery, the cure of talipes by incision and extension, forming as it does one of the most efficient means in the cure of that kind of deformity. It is generally believed that the tendinous structures, after division, have a tendency to approximate; and the experiments of Gunther, Von Ammon, and Bouvier, on dogs and horses, have supported such a statement, showing an intervening ligamentous substance, uniting the divided ends of a tendon. Now my patient's case is the reverse, and I am unable to effect any good by position or pressure. I informed Mr. S. that, as far as the injured muscle was concerned, the power was gone, but that it will be supplied from the two vasti muscles. Therefore the only measure I adopted was to remove the

tenderness by counter-irritation, and to endeavour to restore the lost power by extension of the leg. By the bye I have somewhere heard it said, when speaking of the cure of talipes, that it would be advisable to divide the tendon near to the fleshy or muscular part. Probably, in the above case, the rupture may be near to the muscular bands, but I am not able by examination to ascertain the exact part that is separated; I should, consequently, be slow to admit such a doctrine in the operation for talipes.—I remain,

Yours respectfully,

JOHN GRANTHAM.

Crayford, Kent, Sept. 6, 1841.

ANALYSES AND NOTICES OF BOOKS.

“L'Auteur se tûe à alonger ce que le lecteur se tûe à abrêger.”—D'ALEMBERT.

The Cure of Strabismus by Surgical Operation. By W. MACKENZIE, M.D. &c. Being an Appendix to the first, second, and third editions of his “Practical Treatise on the Diseases of the Eye.” 8vo. pp. 30. London, 1841.

AFTER so much has been written rashly on this subject, it is well to find some calmly reviewing their own and others experience, and publishing it in a plain and instructive form. Dr. Mackenzie has done this, and, although it contain little that has not been written before, his pamphlet will be very acceptable to all, whether they possess the treatise to which it is an appendix or not. What is now most wanted for the perfecting of the operation is, as our recent correspondent Mr. Barker suggests, an accurate account, by authors who can be strictly relied on, of the conditions of a number of cases twelve months or more after the division of the muscle. There can be no doubt, that in a majority of those first operated on, unnatural prominence and eversion of the eye now exist; so that in a walk along the streets one may now recognize almost as many who have been *cut* as there were two years ago obviously squinting. But we have at present no sufficient evidence whether this defect, which is only rather less awkward than an ordinary strabismus, be the necessary result of the operation in a particular class of cases, or (which seems more probable)

of too great a separation of the muscle, or of a neglect of the proper orthophthalmic measures after the operation, or of all these or any other causes. There can be little doubt that the early operations were performed with as much rashness as the early writings were published: the results of both have been alike unsatisfactory; but it is to be hoped that the perfection of the mode of treatment will, now that a foundation of numerous failures has been laid, be at last established. The operation for strabismus is peculiarly one of which the results ought to be calculable with a mathematical accuracy: its frequent performance might have been expected to afford ample illustrations of the pathology of the disease, and of the physiology of the motions of the eye; but the true science of strabismus is as yet very obscure.

The Grave Yards of London, being an Exposition of the Consequences connected with the Pestilential Custom of depositing the Dead in the Midst of the Living. By G. A. WALKER, Surgeon. 8vo. pp. 46. London.

IN Dec. 1839, soon after Mr. Walker published his larger work, “Gatherings from Grave Yards,” we recommended him to “come out in a slenderer shape, with more of modern London and less of antiquity.” This advice he has followed, and has now produced in a pamphlet fully as much evidence as is necessary to establish the importance of remedying the abuse which he has by great industry and vigour exposed. We shall be glad to find the extensive circulation of this cheaper work effecting the good which the author, quite disinterestedly we believe, is seeking: but it is one of the evils attendant on the much greater advantages of the respect paid in this country to vested interests, that even where they are productive of public injury, they are treated very warily, and are rather suffered to die out than destroyed. So it will probably be with intra-mural burials; the beauty and seeming good management of the suburban cemeteries are rapidly bringing churches and church-yards into disrepute for burial; and the change of fashion among the rich will be slowly followed by the provision of similar places of interment for the poor. Mr. Walker’s pamphlet will do much to

expedite the beneficial change ; but it cannot be accomplished at once, nor by any sudden attack upon customs or privileges, however bad.

MEDICAL GAZETTE.

Friday, September 10, 1841.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”

CICERO.

THE SOCIETY OF APOTHECARIES *v.* GREENOUGH.

OUR readers will have perused with some interest this trial, which we published in our last number. They will have found that the Society of Apothecaries is again put in possession of the exclusive privileges of which they seemed for a time deprived by the verdict given after the strange misdirection by Mr. Justice Maule. There can now be no doubt of their power to obtain penalties against any who, not being licentiates of their body, attend or advise patients, and administer medicines, for their own profit. The plea of the unlicensed, that they carry on the business of surgeons, has long been cut from under them: and that of practising as chemists can no longer avail: the worshipful Society has been fully triumphant, and its rights are now as firmly and definitely established by law as they were in the first year of their charter.

But rights of this kind require to be sanctioned by custom as well as by law; for, if they are not, they can be maintained only by the main force of large sums of money continually spent in their protection. They are in this respect like the rights of taxation or of custom duty, or of postage under the old system: few people think themselves the less honest for evading them by a sharp trick; nay, there

is some amount of *eclat* connected with a cunning fraud in these matters, and men talk of it with boasting. And so it will still be with the rights of the Apothecaries' Company. Every one will acknowledge them, and will readily admit that, if he invades them, he may perhaps have to submit to the penalty; but yet, many will still invade them; for each will think that, of the thousands of contraband practitioners, he is not likely to be made the first scape-goat; and each will be sure that, if he be prosecuted and convicted, the public will regard him not so much with contempt for his fraud, as with sympathy, because he is made the victim of what they call monopoly, and for its very name's sake hate. Unless, therefore, they are prepared to maintain an establishment like the excise, for the prevention of the infringement of their charter, or to submit to an outlay of half of their income in going to law, the worshipful Society must be content to prosecute only those who greatly encroach upon their manor: the crowds of those who poach for small game must be allowed still to carry on their illicit practices.

This being the case, the advantage which the public will derive from the re-establishment of the Company's exclusive privileges is less than it should be; for the great body of the mischief is done not by the comparatively few who, like Mr. Greenough, can talk of having three hundred bills to make up for patients, but by the mass of chemists who each practise a little. These evils the Society can never extirpate; nor is it improbable that some other remedy might be found better adapted for their utter prevention.

Let us trace briefly the history of the circumstances which have issued in the present state, in which the great majority of the sick poor who can leave their houses fall under the hands of

mere chemists and druggists. All these, when the charter was first granted, were the patients of apothecaries, who then, in the discharge of such duties as now constitute their ordinary routine of practice, were thought to be rather infringing on the rights of the physicians. But as, with a laudable desire to increase the respectability of their body, the Society constantly demanded more and more of medical knowledge from those to whom they would grant their license, the character of those who entered this branch of the profession gradually changed. They who before would have been physicians, or who would have gone into some different, and, as it had once been, more respectable, profession, now found an ample station in society, and a good prospect of a return for their money outlaid, in becoming general practitioners. And so the race of pure apothecaries died away, and an order took their place better born and better bred, and possessed of a far greater amount of medical knowledge.

Nothing could have afforded less room for aught but congratulation, if all other orders of society had but made parallel progress; if only the means of the patients had kept pace with the increasing respectability of their medical attendants. But it was not so; rather, on the contrary, the patients grew gradually poorer, so much so, that they could barely afford to pay according to the old rate of charges, and would be bankrupted by the payment of a moderate bill on the new scale. For in this as in every other case, increasing respectability can be maintained only by an increasing outlay; and at just the same rate as the style of living of practitioners has augmented, so have their charges. The circumstances, therefore, by which the advantages of the social and scientific elevation of apothecaries are alloyed,

are just these—to maintain their present position their charges must be higher than those of the poorer class can afford to pay; the seeming excellence of their profession, in its most palmy days, attracted more into it than it could now, even if it were in a more favourable state, maintain; and a class inferior to their own, but supplying the most pressing of the public demands, by giving cheap advice and medicine, has risen up, and is now trampling painfully close upon their heels.

In this emergency there seems, as far as the profession is concerned, no present help. The possible remedies are these—the whole class might descend, and put themselves on a level, in all but knowledge, with the chemists; but such a voluntary retreat would be worse than a destructive overthrow. A few have attempted it, and so have degraded themselves, sinking their respectability for the sake of their present profit; but they have very justly reaped the contempt of their brethren. Or, instead of this, the powers of the act might be rigidly and widely enforced, and practice of any kind by chemists prohibited; but this, we have seen, is so little practicable, that none can reasonably look to it for aid. Or, lastly, the evil must be endured till the numbers in the profession are gradually diminished to a degree commensurate with the still remaining patients; and this, as we have often said, is just the course that events are now taking; fewer enter the profession every year, and more, either voluntarily or by compulsion, retire from it. It is our misfortune to live in evil times; but the evil is gradually curing itself.

In this prospect of our own affairs; we must not shut out from view the evils that the public suffer. A large number of them, by the changes

just described, have been deprived of competent medical attendance; they have been unable to meet the terms necessarily demanded by respectable practitioners, and they have been obliged to resort to chemists. The only other resource was in the dispensaries and other establishments, where medical aid is gratuitously afforded; but these, numerous as they are, are altogether insufficient to supply the assistance needed by those who cannot pay. It becomes, therefore, a question whether, if this class of patients be actually removed from the charge of licensed practitioners, some other means should not be devised for their aid; and if there should be, the only plan seems to be the licensing, after a competent examination, of the chemists and druggists. The observations which we made some time since on the Pharmaceutical Society, and the excellent character which the transactions of that association maintain, plainly indicate that (as indeed any one who will observe may see) the druggists are steadily making progress *pari passu* with the apothecaries, and that however great the elevation of the latter, there is really scarcely a greater distance between them and the former than there was at the first establishment of the charter. It is plain, from the present course of events, that the chemists will not be restrained within those limits of their business which do not involve an infringement of the Apothecaries' Act; they cannot be so restrained, nor, if they could, do we think it altogether prudent that they should, if they will only consent to submit to some fair test in proof of their fitness to be legalized for higher functions.

There can be no reasonable objection to a plan which would prevent chemists from exercising their present calling without a security that they are suffi-

ciently acquainted with the nature of the substances in which they deal, to prevent them from doing mischief; nor do we anticipate that any evil would result from a measure which should permit them further (when examination had proved them competent) to advise remedies for the less important and not dangerous maladies. They do so now illegally, and the mischief they inflict on the profession is not great, because very few go to them who can afford to pay a more regular practitioner. The mischief would be no greater if they were allowed to do so legally, nor would the profession be then aggrieved more than now; but, at the same time, the advice given to the poorer classes of the public might be very much improved, and thus at least a considerable amount of benefit would be in the end conferred.

It may be deemed strange that we so often refer the difficulties under which the profession labours to circumstances that have been the natural consequences of their own advancement in the scale of society, and of changes in the conditions of their patients, and other things which nearly all centre in the common ill of deficient pecuniary remuneration, rather than to the abuses and monopolies of corporations, or the existence of more than one division of practitioners, or those other errors of medical polity which engage all the attention of the *soi-disant* genuine medical reformers. But we do wish that those members of our profession, who imagine that all our ills arise from misgovernment, would calmly consider the question, and see how many of those evils they can distinctly and clearly trace to the faults of our governing bodies, and how many of them to the facts of the numbers in the profession having increased in a far greater proportion than the means of the public to remunerate them. In our professional just as in

more general polity, it is far too common, perhaps because it is more easy, to impute all faults to modes of government; whereas, in truth, many of the greatest of them are the results of a natural and traceable course of events, which would exist in as full extent what government soever we might live under. The error is very mischievous; for it prejudices men's minds in favour of any new scheme that the reprovers of the old ones may bring forward, by making them think that at any rate it cannot be worse than they are, and that its supporters, who are so much offended at what is evil, must themselves be singularly good; and, which is yet worse, it draws off men's attention from the real source of mischief, and prevents them from doing their best for its removal. We have never thought or said that the medical government amongst us was free from many faults; but we do maintain that it is not the source of one tithe of the difficulties under which the profession labours, and that those who look to changes in it for improvement in their several personal conditions will be grievously disappointed.

SUBSTANCE

OF A

CLINICAL LECTURE,

Given at St. George's Hospital, June 8th,

BY MR. CÆSAR HAWKINS.

1. *Injury of the head; fracture and depression of the skull? Caries of the bone? Effusion in the pleura. Circulation in the brain, with loss of the cranium.*

AFFECTIONS of the head are always an interesting subject of consideration, and there are several at the present time under your notice, of which I will select three, as they offer some important practical suggestions; and some questions have been asked me, which shew that several points connected with them are not well understood by all of you.

I. The first case is that of a little boy,

William Lyons, ætat. 13, admitted March 24th, into Cholmondely Ward, with caries of the anterior part of the right parietal bone, with an opening through the skin, about the size of a sixpence, exposing the dura mater. Tongue clean; pulse quiet; no pain in the head; sleeps well at night; countenance pale. States that about three months ago he was carrying some chairs on his head, when he happened to fall down, and received a blow on the head from one of the chairs. He was not insensible after the accident, but only perceived a swelling on the head after the blow. This did not disappear, and was opened about a month afterwards, when some pus escaped, since which time it has continued to discharge, but he has not perceived any bone come away. This account is corroborated by the information we have received from the house-surgeon of the Westminster Hospital, where he was admitted a fortnight after the accident, and where the swelling was punctured, and found to contain only pure pus; but this gentleman says that bone was felt deeply situated, as if in the substance of the brain. During all the time he was there he suffered from no symptoms of consequence, and neither brain nor bone escaped through the opening. At the time of his admission our notes say the probe passed under the scalp to some distance beyond the opening, (about an inch and a quarter by an inch, in size); and bone is felt of apparently the natural thickness, with an abrupt margin on each side. In the interval no bone is felt, and only the dura mater (as it seems) presents itself, which is vascular, and bleeds from the touch. The pulsation of the brain is also seen in the whole space where the bone is deficient, as if there was at present no depressed bone in that situation below its natural level.

1. Such is the history which was obtained of this boy when he was admitted, and from it we should gather that the bone had been broken and depressed at the time of the accident, with laceration of the dura mater, followed by slow suppuration: if it were so, there is no account of any bone having ever come away, and therefore it must still remain in, since the surface exposed is at too great a depth to make us suppose that the depressed piece could have remained partially attached to the dura mater; and unless so attached, it, of course, cannot have been absorbed, as the process of absorption of bone requires some living vascular action. If the bone is lodged in the brain, it has, at any rate, excited not the least irritation since the boy has been in the hospital, and it appears that he has not suffered previously from any affection of the brain. I must confess, however, that I have some doubts upon this point, since the living surface, which is visi-

ble, is quite smooth and level, and looks just as the dura mater would have done if it had been uninjured. What can have become of the bone then, you will ask? Why, it is not at all impossible that the injury, and the pressure of blood effused under the pericranium, may have caused the absorption of a portion of bone with caries, which would go on enlarging the opening when once formed, especially in a scrofulous boy, since you know that scrofulous caries, taking place spontaneously, will occasionally produce this result. And the course of the local changes, since he has been in the hospital, is just like such an action, as there is some thickening of the soft parts around the opening, and another spot lower down has supplicated in the scalp, and the caries, which now exists, has been attended, as it often is, by the death of some of the diseased bone; so that in the early part of May I removed several small pieces of dead bone from around the opening, in that part of the bone which preserves its natural level; and some of this may have been felt when the abscess was opened. I will not dwell more, however, upon this part of the case, but will pass on to a second event in his history, which is interesting, though not connected with the brain.

2. The boy was observed on his admission to be very languid, and pale, and weak, and in a few days he had a good deal of cough, with some pain in the side, and we found that he scarcely breathed with the left side, which was quite dull, except just at the upper part; and in a day or two more we found that this side was actually larger than the other: in short, he must have had pleurisy before he came in, with effusion of water or other liquid into the chest, sufficient almost entirely to compress the lung, so as to prevent the ingress of air. The little irritation he had, when our attention was first drawn to the chest, was easily subdued, particularly by blistering; and on the 8th of April our notes tell us that more air seemed to enter the lung, but at the same time that he had more cough, no doubt from the compressed lung beginning to expand a little, as some of the fluid was absorbed. He continued to have a little cough, but no constitutional irritation; and the fluid was gradually absorbed, but without much further expansion of the lung, till, our notes tell us on May 24th, that no air enters the chest below the fourth rib under the scapula, and in front it cannot be heard at all; but now, instead of the chest on this side being larger than the other, the thorax is perfectly flattened, and measures an inch and a half less from the spine to the centre of the sternum than on the other side. The heart at the same time beats over a large space, and is enlarged, as it would appear, from rheumatism. You

have thus an excellent example of the absorption of fluid from the chest without operation, while the lung has only expanded to a small extent; and the effect of this alteration of size is generally to make the spine curve to the side, with depression of the shoulder.

3. I will, however, chiefly dwell in this case on the state of circulation in the brain, as you can see it going on through the aperture in the skull, as it does not seem to be well understood among you all; and there are many important inferences to be derived from a consideration of it, accounting, as this consideration will do, for the obscurity so often observed in injuries and diseases of the brain, in which exactly the same symptoms can often be traced to different, or even exactly opposite causes.

a. Look, in the first place, at the exposed surface of the brain in its habitual, I was going to say in its natural state, and you will see that there is an arterial pulse in it, so that there is an alternate rising and falling exactly corresponding with the systole of the heart: it rises as the arteries beat, and falls again till the next stroke of the heart; in other words, the brain actually contains more arterial blood at one time than at another.

b. Look again, when I have opened an abscess, or taken out a piece of bone, or when he has risen quickly off the bed, or has been subjected to any other kind of excitement, and you will perceive that the cavity below the aperture is lessened, and that there is also more pulsation than in the quiet condition; that the brain is kept permanently higher, rising and falling alternately, but quicker than before, and never falling so low; containing, in short, a little more blood altogether, and having more entering it in a given time.

c. Then make him breathe deeply, or cough, and you will see that the brain rises still higher, so as even to expel the matter in a stream by the impulse of coughing; in fact, besides the beating of the pulse, there is a rising and falling of the surface in correspondence with respiration; that is, not only 80 or 90 times from the pulse, but 18 or 20 times, to a greater degree, still from breathing. During each expiration the chest is depressed, so that the venous blood is prevented from descending, and is accumulated in the veins and sinuses of the brain, so as to elevate the dura mater, which again falls in each inspiration by the weight of the atmosphere, the air entering the air-vessels, and the blood getting into the veins of the chest by the elevation of the ribs and the depression of the diaphragm, out of the distended veins of the brain.

But most of this is only because there is an opening in the skull, allowing the air to

enter and make pressure on the surface of the brain, and you are not to suppose that the natural state of the circulation is exactly of the same kind. When the cranium is entire, the atmospheric pressure can only be felt at the various orifices at the base of the skull, and will there present any such vacuum between the dura mater and the level of the skull as we can now see; and as the substance of the brain is incompressible, there cannot possibly be the variation in the quantity of blood contained in the brain, which there is in our patient Lyons, with such a deficiency of the bone.

a. With regard to the arterial pulse in its natural state, we see more blood in the arteries during the systole of the heart, making the brain rise. So it is also when the bone is entire; but as the brain cannot rise, there must be at that time less in the veins of the brain.

b. So again in the excited condition of the circulation, we can see more arterial blood constantly in the brain. There is also, with an excited pulse, more arterial blood always in the head, if the cranium is entire; but then there must be still less in the veins than in the quiet state of the circulation, as the brain cannot rise upwards as we see it now do.

c. And, with regard to the elevation of the brain in our patient in expiration, there is also a similar impediment to the descent of the venous blood, when the bone has no aperture in it; but if the veins and sinuses contain more blood, there must be less in the arteries, which therefore are not at those times allowed to be so well filled by the action of the ventricle as in inspiration.

Thus, then, in the natural circulation there is a variation in the relative quantity only of the arterial and venous blood, not in the absolute quantity contained in the whole brain; but as this variation is taking place every second of time, you may easily believe that it must be of great importance to the functions of the brain. But this is not all: there are other circumstances to be taken into account in the physiology and pathology of the brain.

d. If the brain contain a large relative quantity of *arterial* blood, this will be circulating chiefly in the anterior of the nervous substance, separating the fibres and elementary particles, and stimulating their substance, it may be, to a healthy performance of its functions; or it may be, to an undue degree, producing inflammation or other mischief.

e. If, on the other hand, there is a disproportionate quantity of *venous* blood, this will be circulating chiefly on the surface of the organ, including the spaces between the masses of the encephalon; whence we may understand the effects of venous congestion in the production of sleepiness, stupor, pa-

ralysis, or other signs of weakening of the functions of the brain, or perhaps, like the influence of poison on the brain, causing delirium, convulsions, apoplexy, and so on, by means of this black blood.

f. But, further, we have seen a difference not in quantity only, or relative proportion of blood of different qualities, in different periods of the same kind of circulation, but in the rapidity also with which the same relative quantity of arterial blood passes through the brain in a given time. Much more arterial blood will actually circulate through the brain with a strong heart than with a full one, or with a quick pulse than with a slow one, the strength of the heart being the same:—or a feeble heart, beating very rapidly, may send more blood through the brain than a stronger one acting more slowly;—and a continued steady impulse on the substance of the brain must be very different in its effects from those of an irregular or intermittent state of circulation through its texture.

g. Then, finally, besides all these varieties of circulation through the whole brain, influencing all the encephalon alike, you must recollect that there is irregularity as to particular parts of these organs. Doubtless, for example, there is a greater state of vascularity in the forepart of the brain in this boy than there is naturally; and if this be so, when there is no aperture in the cranium, and the patient often refers us to one particular spot as the seat of pain when there is an abscess or a tumor, there must necessarily, in accordance with what I have previously said, be less blood than natural in the remainder of the brain.

Now all these circumstances influence our practice in the treatment of cerebral affections of any kind. We take away blood, and give nitre or tartar emetic, and use other measures to lessen the quantity of arterial blood, or moderate the force with which it enters and circulates through the brain, when there is undue action of the heart, or what is called determination of blood to the head. We can relieve the oppressed brain in many cases of plethora, or venous accumulation, by taking away a small quantity of blood, so as to enable more arterial blood to circulate; and thus very often we restore the functions of the brain and of the lungs at the same time, whether the cause of the oppression may have originated in the brain itself, or in the heart, or in the lungs, these several organs being so connected together.

In another case of passive venous congestion we overcome it by stimulating the heart, so as to make it propel more red blood upwards; or even by change of posture, so as to facilitate the passage of the venous blood downwards: the difference of lying down or sitting up being sufficient to

determine a question of life or death. Then, again, we can give stimulants to a feeble heart so as to restore a healthy action to the brain, when insufficiently supplied with arterial blood, whether in absolute quantity, or in rapidity, or force of propulsion. You can any day see the influence of this latter circumstance in cases of delirium traumaticum, in which perhaps the pulse may be beating at the rate of 150 in a minute, with great apparent quantity of blood in the vessels of the head; but where your patient will yet die with furious delirium, unless you increase the force of the arterial impulse, by some of that gin by which his brain has been habitually stimulated, and by which he will directly be quieted of his violent exertions of mind and body, and fall perhaps into a gentle and refreshing sleep. Or, again, if we see a person dying from loss of blood, and insensible because the brain has so little stimulus of its necessary circulation, we may perhaps, when time is not afforded for food or drink to enter the blood, restore his consciousness and save his life, by transferring some from another person, which may directly be sent up to our patient's sensorium, to afford it the requisite and natural stimulus.

[To be concluded in our next.]

MEMOIR OF THE CASE OF A GENTLEMAN BORN BLIND,

And successfully operated upon in the 18th year of his age, with Physiological Observations and Experiments.*

BY J. C. AUGUST. FRANZ, M.D., Leipzig,
M.R.C.S., &c.

MR. F. J., the subject of the present memoir, is the son of a physician; of scrofulous diathesis, but otherwise of robust constitution; of irritable temperament, but of contented and happy disposition; and endowed with an excellent understanding, quick power of conception, and retentive memory. In both the eyes of his father, cataract (with the addition, I suspect, of glaucoma) has manifested itself within the last four years, after a severe attack of influenza. The relatives on the paternal side are predisposed to diseases of the eye; but in the mother, and in the relatives on her side, no such predisposition can be traced. With regard to the cause of the ophthalmic affections which form the subject of this paper, the mother seemed to lay much stress on the following circumstance, which, although it may possibly have had some share in the cause of one of them, can have had no influence, in my opinion, in producing the other. She stated to me that in the eighth

month of her pregnancy, which up to this period had proceeded favourably, she received from her youngest child, which she was carrying in her arms, a severe blow on the eye. This accident caused inflammation of the eye, accompanied with a curious visual illusion, viz. that all objects which she saw, but especially those situated on the ground, appeared of a deep concave form—an illusion which lasted for several months. The fright experienced from the accident also brought on convulsions, which, recurring several times, extended even to the foetus. The recurrence of these convulsions produced in the mind of the mother a continual anxiety and fear for the health of the child, while the pain arising from the ophthalmia, together with the visual illusion just mentioned, gave her fears a direction more especially towards its eyes. Delivery took place at the proper period, when the eyes of the infant, which was otherwise healthy and well formed, were found to present a two-fold defect of organization. The father, to whose statement, on account of his professional knowledge, more weight is to be attached, informed me that both eyes were turned inwards to such an extent, that a portion of the cornea was hidden by the inner canthus, and that in both pupils a yellowish-white discoloration was to be observed, which, being situated behind the iris, could not be the pupillary membrane. That the strabismus and cataract of both eyes in this case were congenital is evident from the testimony both of the parents and of the nurse, whom I have closely questioned on this subject. The latter, who can distinctly remember all the circumstances of the case, told me that when the child was a few months old, she held a light before its eyes, of which it took no notice. I ascertained also from her that the eyeballs had not that restless motion which is generally observed in those who are born blind, but that both eyes were always turned inwards, and that but rarely either the one or the other was moved from the internal canthus.

It was also stated to me that towards the end of the second year the operation of keratonyxis was performed on the right eye, upon which a severe iritis ensued, terminating in atrophy of the eyeball. Within the next four years two similar operations were performed on the left eye, which did not indeed destroy the organ, but at the same time did not remove the opacity in the pupil. The colour of the opacity became in time, however, of a clearer white; and the patient acquired a certain sensation of light, which he did not seem to have had before the operation. Both eyes for a long time retained a disposition to inflammation, and suffered repeatedly from conjunctivitis, whence the vessels of the conjunctiva were increased in number and size

* From the Philosophical Transactions, Part I. for 1841.

to such an extent that it was necessary they should be several times excised.

At the end of June 1840, the patient, being then seventeen years of age, was brought to me by my friend Dr. Swaine, for the purpose of consulting me with regard to the congenital double strabismus, and at the same time to hear my opinion on the more severe ophthalmic affection, which up to this period had been considered incurable: the patient himself regarded his case as hopeless. The following are the particulars elicited on an attentive and careful examination:—On the right side, the eyelids and parts adjacent appeared contracted; they were less in size, and the eye itself was situated deeper in the orbit than the left. At each act of winking spasms of the eyelids were induced, and, when the left eye was turned outwards, the spasmodic twitchings extended over that half of the face. Both eyes were so much inverted that nearly one-half of the cornea was hidden by the inner canthus. The left eye he could move voluntarily outwards or in any direction with certainty, but not without exertion; it returned immediately inwards when the influence of the will had ceased. The motion of the right eye upwards and downwards the patient had under his control, but not so the movement towards the external canthus, in effecting which he only succeeded after many attempts. The left eyeball was of the natural size and elasticity; the right, on the other hand, was at least a third smaller, and felt soft, and like dough; it was also, in the neighbourhood of the rectus internus, flat, or rather pressed inwards. The cornea was less convex, somewhat smaller, but not in proportion to the diminished size of the globe itself; it was clear, and free from opacity, except in the centre, where the keratonyxis had left an opaque spot. The fibrous structure of the iris was irregular; its colour, which was brown, rather lighter than that of the left eye. Different degrees of light produced no effect on the motion of the iris; but when the eye was moved in a horizontal direction outwards, the pupil, in passing the centre of the orbit, contracted a little, and, when approaching the outer canthus, expanded again to the size it held when the eye was in its usual inverted position. On looking from the temporal side into the pupil, a large portion of the opaque capsule was observed in the posterior chamber. The interior of the eyeball presented a brownish black appearance. The patient had not the slightest perception of light with this eye: it was perfectly amaurotic. The left eye presented in the conjunctiva, especially at the inner canthus, a number of varicose vessels, and in the sclerotica a fine vascularity around the outer half of the cornea: this latter membrane was regularly convex, clear, and perfectly pellucid.

The fibres of the iris were rather irregular; its brown colour not equally diffused. The pupil, which was uncommonly large, was not round, but drawn angularly downwards and inwards, neither altering in dimension with the movements of the eye, nor from the stimulus of light. On examining the eye, by looking straight into it through the pupil, the anterior wall of the capsule was observed undestroyed, rendered opaque in its whole extent, hypertrophied in several places, and of a colour and lustre like mother-of-pearl. On looking from the temporal side in an oblique direction into the pupil, there was visible in the anterior wall of the capsule a very small perpendicular cleft of about one line and a quarter in length. This cleft was situated so far from the centre of the pupil that it was entirely covered by the iris, and the inferior border being united to the uvea, it was kept a little open, so that the aqueous humour had free entrance into the cavity of the capsule. Except at the spot where the union of the capsule with the uvea took place (the cause of the angular form of the pupil), these two membranes were not in contact with each other. The patient only complained of an occasional sensation of pressure in the interior of the eye; otherwise the organ was free from pain. With this eye he had a perception of light, and was even capable of perceiving colours of an intense and decided tone. He believed himself moreover able to perceive about one-third of a square inch of any bright object, if held at the distance of half an inch or an inch from the eye, and obliquely in such a direction as to reflect the light strongly towards the pupil. But this I am convinced was a mere delusion; for, from the state of the interior of the eye, as just described, it is evident that all rays of light falling in the direction of the optic axis in the pupil must be intercepted, and reflected by the opaque capsule. By these rays, therefore, a perception of light indeed might be conveyed, but certainly no perception of objects. On the other hand, it seems probable that the lateral cleft in the capsule permitted rays of light to pass into the interior of the eye. But as this small aperture was situated entirely behind the iris, those rays only would have permeated which came in a very oblique direction from the temporal side. Admitting then these rays of light to pass through the cleft, still on account of their obliquity they must fall at a place situated about midway between the ciliar ligament and the centre of the posterior hemisphere, where, from the laws of optics, they could produce but a very imperfect image; and, owing to this imperfection of the image, it was impossible that the portion of the retina upon which these rays impinged could have obtained that acuteness of sensation which is essential for the mental

perception of the image caused by an object. Nevertheless, we will assume that the cleft in the capsule held the same relation to the eye in this instance, as a small hole in a card placed immediately before a healthy eye; in this case the patient would not only have seen an object at the distance of half an inch or an inch, but even at a much greater distance. That he was incapable of this I have satisfied myself by repeated experiments, which have led me to the conclusion that his belief that he really saw objects resulted solely from his imagination, combined with his power of reasoning. In feeling an object, and bringing it in contact with the eyelids and the cheek, while holding it close before his eye, by his refined sense of touch an idea of the object was produced, which was judged of and corrected according to the experience he had gained by constant practice. This opinion is confirmed by the observations of those who have known and watched him for years, and also by a fact which I have myself frequently observed, viz. that all well-educated blind persons, who are not absolutely amaurotic, endeavour to persuade others that they see more than they really can, in order to conceal as much as possible their deficiency in the noblest of the senses, and from a reluctance to be regarded as objects of compassion.

On terminating this inquiry into the condition of the visual organ and the actual state of vision, I may here be allowed to mention that the patient's sense of touch had attained an extraordinary degree of perfection, and that in order to examine an object minutely he conveyed it to his lips. The sensation produced by silk stuffs was most pleasing to him. He was said to possess the power of distinguishing colours by the touch, but this assertion was not confirmed by his own testimony.

After the examination above detailed, I gave my opinion that the defect of the right eye was irremediable; that the patient might obtain sight with the left eye by an operation; and that the disfigurement caused by the inversion of the eyes might also be removed by operation. Though the left eye had been considered incurable like the right, there appeared to me reasonable grounds to hope for a cure, provided I could succeed in keeping down inflammation, which is not easy to be done in an eye already several times operated upon, and especially in a young plethoric subject. The operation was then resolved upon.

On the 10th of July 1840, in the presence of Dr. Swaine, and with the kind assistance of Messrs. F. Fowke and F. Steinhæuser, I made an incision in the cornea upwards, and introducing a pair of fine curved forceps, armed with teeth, into the posterior chamber, I seized the anterior wall of the capsule

by passing one of the blades of the forceps into its small aperture, and attempted, by pulling it slowly, to separate it from its adhesion with the uvea and its peripheral connection, in which I succeeded without producing a prolapsus of the vitreous body, or tearing the capsule, which I now removed. After this proceeding, a large piece of the lens, of an opaque colour, probably the nucleus, presented itself in the pupil, which was easily removed from the eye by means of Daviel's spoon; the pupillary aperture then appeared perfectly clear and black. The patient was now turned with his back to the light, for the purpose of trying a few experiments as to his sight, but from these I was obliged to desist on account of the pain which the light produced in the organ. Both eyes were then closed with narrow strips of court-plaster, and the patient carried to bed. Venesection, local bleeding, fomentations with iced water, continued without intermission for about forty-eight hours, together with the scrupulous observance of the most severe regimen, barely succeeded in keeping down the inflammation, the effects of which in this case, where but one eye offered hope, were much to be dreaded, if it should surpass that degree which was necessary for the healing of the wound in the cornea. This process went on and terminated so favourably, that the cicatrix, situated close to the sclerotica, is now scarcely visible. The patient suffered from *muscæ volitantes* and from a considerable intolerance of light, pain being produced by even a mild degree of light falling on the closed lids. The *muscæ volitantes* were greatly mitigated, and the intolerance of light ceased, after the lapse of a few weeks, by the use of proper pharmaceutical remedies, by local bleeding, change of air, &c., and the employment of the ophthalmic fountain of Professor Jungken, which I have fully described in the *MEDICAL GAZETTE*, vol. xxvii. p. 444. To promote the development of the power of vision, the use of the fountain was continued twice daily, with Pyrmont-water, and latterly with simple spring-water, for the space of three months, when it was discontinued, as it began to irritate the eye.

Before I proceed further, I must again refer to the condition of sight previous to the operation. The right eye was completely amaurotic; in the left the power of vision existed, but, on account of the mechanical defect in the visual apparatus, was very little developed for the perception of light, and not at all for the perception of objects. It appeared to me, therefore, of the greatest interest to observe attentively the progressive development of the sensibility of the retina as regarded direct, refracted, reflected, and coloured rays of light; and also the progress of the visual perception in respect

of the form, dimensions, and distance of objects. I was the more induced to undertake these physiological observations from having the opportunity of conducting them with an individual, who, from his age, mental endowments, and education, offered peculiar advantages for such experiments.

[To be concluded in our next.]

ON THE USE OF COMPRESSION IN THE TREATMENT OF MAMMARY ABSCESES.

By MM. TROUSSEAU AND CONTOUR.

IN this memoir there are two distinct parts ; one devoted to the description of abscesses of the breast, the other intended to recal the attention of practitioners to a curative means too much neglected in their treatment, namely, compression. The following is the mode in which it is to be applied :—It is to be accomplished by strips of plaster, broad, and sufficiently long to go several times round the body. The surgeon, standing by the side of the patient, must first fix one of the extremities of the strip at about the middle of the back, then carry it towards the side of the chest, then pass it over the breast, beginning from the lowest part, then obliquely from below upwards to the outer third of the clavicle on the healthy side, and then obliquely downwards across the back, so as to cover the extremity of the slip already fixed. Following this course several times, he must take care that the portion of the band applied each time covers the two upper thirds of the preceding turn. But it is easy to see that if the bandage is always carried in the same direction, the breast cannot be completely covered ; and that, on the other hand, as its several turns go across the clavicle of the healthy side, the movements of the shoulders would tend to displace it, and the lower part of the breast might soon be uncovered. Other strips of plaster are therefore applied, which, proceeding from the anterior and upper part of the abdomen, ascend, crossing the first obliquely ; then pass under the axilla, and return, after passing over the posterior part of the chest, to the part where they were first applied, and then are carried again along the same track, covering each time the two upper thirds of the strip last applied. The breast is thus completely covered by the bandage, which is prevented from rising by this last described, which ought to cover only the upper part of the breast.

To compression thus employed the authors attribute many advantages. In the first place, it immediately relieves the pain ; it combats and diminishes the inflammatory

engorgement, at whatever period it is applied. When employed after opening the abscess, it decidedly favours the evacuation. And although when employed too long, at a period when the process of suppuration is active, it might have the disadvantage of making the pus extend over a larger surface, yet this may be avoided by removing the bandages at a time when it is probable that matter has fairly formed. If this be done, and the abscess opened, the bandages may be again applied, after two or three days poulticing, with good effect.—*Journal des Connais. Med. Chirurgicales*, Janvier, 1841.

FOREIGN BODIES IN THE JOINTS

DISLODGED BY SUBCUTANEOUS INCISIONS.

By M. GOYRAND.

A MAN, aged 24 years, presented the symptoms of a foreign body in the knee-joint. M. G. having determined its existence, and knowing the dangers which commonly attend the extraction of these bodies, decided on the following plan for the relief of the patient :—At one operation to cut under the skin, the synovial membrane, and the tissue surrounding it, and push the foreign body through the incision into the subcutaneous cellular tissue ; and at a second, to be performed after the cicatrization of the wound made in the first, to extract the foreign body by a simple incision in the skin.

This plan was put in execution in the following manner :—He commenced by fixing the loose body at the upper and outer part of the joint ; then, having made a transverse fold in the skin over this part, he plunged in a bistoury at the base of the fold, with its point directed towards the foreign substance, and, without further dividing the skin, cut through all the tissues covering it. To effect this latter part of the operation he had to make three sections. The bistoury having been withdrawn, it was found that the loose body had not got under the skin, but had slipped between the middle and external portions of the triceps, a short distance above the incision in the synovial membrane. In this situation it was fixed by a circular strap. Notwithstanding the carelessness of the patient, who got out of bed three times on the day of the operation, not the slightest accident occurred, and the wound in the skin healed in 24 hours.

Some days having elapsed, two other bodies of the same kind were found in the joint, but only the larger of them could be fixed. This was therefore cut upon in the same manner as the first, and as soon as the deep tissues were divided, it slipped into the sub-aponeurotic cellular tissue, where it was left. The consequences of this second operation were as simple as those of the first, and the

next day the patient was walking about the hospital without feeling the least pain.

It still remained to execute the second stage of the projected operation. It was determined to practise it first upon the body which had been last dislodged, and eleven days after the first part of the operation it was removed, by a simple incision which suppurated for a fortnight, without the joint participating in the slightest degree in the inflammation. The body which had been first dislodged was left in its place, for it gave no uneasiness whatever; and that which remained in the joint producing no inconvenience, was also not interfered with. If it became troublesome, M. Goyrand proposed to remove it from the joint by subcutaneous incision, and then to leave it in the place into which it might slip.—*Annales de la Chirurgie*, and *Gazette Médicale*, Mai 22, 1841.

PROVINCIAL SCHOOLS.

WE would by no means recommend an exclusively provincial education; but we think it right to remind our readers, that attendance on lectures in London is no longer imperatively required by the College of Surgeons. Our attention has been particularly directed to this point at the present moment, in consequence of having received a letter in which it is remarked, that, at the last examination of the London University, “the only three gentlemen who went up from the Bristol School all passed—one of them in the first class.”

DR. HUNTER.

(From a Correspondent.)

It is always gratifying to us to record instances in which our professional brethren have merited and received testimonials of public esteem. We perceive, in the Glasgow Argus, an account of a dinner given to Dr. Hunter, the new lecturer on anatomy at the Westminster Hospital School of Medicine, by the inhabitants of Glasgow, on his retiring from the chair of anatomy in the Andersonian University of that city. It appears that he had been connected with that institution from the establishment of its medical school, in the course of which period he had sent into the world upwards of 2000 young men. We may say, with the chairman of the meeting, that we trust a prospect of great usefulness is before him, and that the fame and fortune which he anticipates may be opened for him in this metropolis.

ROYAL COLLEGE OF SURGEONS.

LIST OF GENTLEMEN ADMITTED MEMBERS.

Friday, August 27.

John Marshall.—John Henry Cooper.—Henry William Parrell Davis.—Ferdinand William

Hutchison.—Henry Gilbert Luttrell.—Peter Brady.—Robert Muney.—James Phelan.—Thomas Murray Farquhar.—James Butler.—Francis Hastings Baxter.—George Munns.—Thomas M. Evans.—John Waggett.—William Bates.

TABLE OF MORTALITY FOR THE METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 28th Aug. 1841.

Small Pox	7
Measles	23
Scarlatina	17
Hooping Cough	24
Croup	9
Thrush	6
Diarrhœa	15
Dysentery	3
Cholera	2
Influenza	0
Typhus	15
Erysipelas	1
Syphilis	2
Hydrophobia	0
Diseases of the Brain, Nerves, and Senses ..	132
Diseases of the Lungs, and other Organs of Respiration	193
Diseases of the Heart and Blood-vessels	17
Diseases of the Stomach, Liver, and other Organs of Digestion	74
Diseases of the Kidneys, &c.	4
Childbed	7
Ovarian Dropsy	2
Diseases of Uterus, &c.	3
Rheumatism	3
Diseases of Joints, &c.	4
Ulcer	1
Fistula	0
Diseases of Skin, &c.	0
Diseases of Uncertain Seat	100
Old Age or Natural Decay	53
Deaths by Violence, Privation, or Intemperance	25
Causes not specified	3
Deaths from all Causes	745

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N. Longitude 6° 3' 51" W. of Greenwich.

September.	THERMOMETER.		BAROMETER.	
Wednesday . 1	from	49 to 62	29.92 to	29.94
Thursday . 2		39 67	29.80	29.76
Friday . . . 3		47 67	29.67	29.50
Saturday . 4		49 54	29.40	29.72
Sunday . . 5		41 54	29.76	29.74
Monday . . 6		34 57	29.68	29.69
Tuesday . . 7		36 57	29.69	29.54

Wind, SE. on the 1st; SE. and S. on the 2d; SE. and SW. on the 3d; SW. on the 4th; N. and NE. on the 5th and following day; S. by E. on the 7th.

On the 1st, generally cloudy. The 2d, generally clear. The 3d, morning clear, otherwise overcast; distant thunder, accompanied with heavy rain, from about 3 till 7, P.M. The 4th, cloudy; raining nearly all the day. The 5th and following day, generally clear. The 7th, morning clear, otherwise overcast; raining frequently during the afternoon.

Rain fallen .96 of an inch.

CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

FRIDAY, SEPTEMBER 17, 1841.

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF
PHYSIC,

Delivered at King's College, London,

BY DR. WATSON.

LECTURE XLIX.
[Concluded.]

Hay Asthma. Chronic Bronchitis. Its varieties. Morbid anatomy of these affections.

THERE is another kind of catarrh described as depending upon a peculiar local cause, and therefore requiring to be briefly noticed. I have never seen it; but it has been observed and described by several medical men. Dr. Bostock, in the *Medico-Chirurgical Transactions*, gives an account of this complaint as it is apt to attack himself. It is called the *catarrhus æstivus*, and by some the *hay fever*, or the *hay asthma*. In Dr. Elliotson's lectures, as published in the *MEDICAL GAZETTE*, there is also a good deal of curious information upon this malady, contained in letters addressed to him from practitioners in various parts of the country, in consequence of some previous remarks he had made upon it in a clinical lecture, which had also been printed. Dr. Elliotson speaks of it as a combination of catarrh and asthma. It consists in excessive irritation of the eyes, nose, and the whole of the air-passages; producing, in succession, itching of the eyes and nose, much sneezing occurring in paroxysms, with a copious defluxion from the nostrils; pricking sensations in the throat; cough, tightness of the chest, and difficulty of breathing, with or without considerable mucous expectoration. This complaint occurs in some persons only, and in them it always takes place at the same period of the year, in the latter end of May, or in June, when the grass comes into

blossom, or when the hay-making is going on. It seems, in fact, to be produced by some emanation from certain of the grasses that are in flower at that season of the year, of the irritating qualities of which emanations some persons only,—and a very few persons in comparison with the entire population,—are susceptible. The complaint occurs only at that one particular season; and it then attacks persons who are not particularly subject to catarrh at other times, or from the ordinary causes of catarrh; and if they avoid meadows, and hay-fields, and the neighbourhood of hay-stacks, they escape the disorder. Hence going to the sea-coast,—and especially to those parts of the coast that are barren of grass,—offers a means of protection; and when this cannot be done, such persons obtain refuge in some measure from the cause of the irritation by remaining within doors, and shutting out as much as possible the external air, during the hay crop. One lady, who suffered annually from this strange affection, states that a paroxysm has been brought on by the approach of her children who had been in a hay-field; and once this happened when the hay season had been for some time over, upon their joining her at tea, after playing in a barn in which the hay of that year had been deposited. She was in the habit of flying to Harwich, or some other part of the coast, as the dangerous season came on. On one occasion, while walking on the shore at Harwich, she was suddenly attacked by the complaint, to her great surprise, as she was not aware of any grass being in the neighbourhood; but the next day she discovered that hay-making was going on on the top of the cliff at the time she was walking under it. And in another year, she being at Cromer, and an attack that she had suffered having quite subsided, and all the hay-making thereabouts being over, she was suddenly visited by the well-known symptoms, and on going into her bed-chamber perceived that they were making a large

stack of hay in a yard near the house, having brought it from a field five miles distant.

Avoidance, therefore, of this source of the complaint is the best thing that can be recommended to these persons. You may read almost every year in the newspapers that one of our English Dukes has gone to Brighton to escape the hay fever. But it is not in the power of every one to leave home for that purpose; and it has been found that the system is capable of being fortified in some degree against the pernicious effects of these vegetable effluvia. Mr. Gordon, of Welton, in Yorkshire, had communicated some interesting observations to the profession on this subject, before those of Dr. Elliotson were published. You may find Mr. Gordon's paper in the fourth volume of the *MEDICAL GAZETTE*. He supposes that the aroma of the sweet-scented vernal grass, the *anthoxanthum odoratum*, is the principal exciting cause of the complaint. He found the symptoms more speedily and effectually removed by the tincture of lobelia inflata, than by any thing else that he had tried at that time; and he recommended the cold shower bath as the best preservative against the attack. But in a subsequent communication to Dr. Elliotson, he states that the sulphates of quina and of iron, given in combination, had proved completely successful in emancipating the two patients, from whose cases he had principally drawn up his account, from their tormenting disorder; although they had, in spite of all previous treatment, suffered an annual return of it for fifteen or twenty years.

The susceptibility of this troublesome affection of the mucous membrane, from a peculiar cause, which to most people occasions no uneasiness, appears sometimes to run in families; and this is nothing more than one might expect.

Dr. Elliotson, thinking it possible that the chlorides, which have the power of decomposing, and disarming of their noxious qualities, certain *animal* effluvia, might exert a similar control over the *vegetable* emanations that excite the hay catarrh, suggested to one of the sufferers a trial of the chloride of lime or of soda. He desired him to have it placed in saucers about his bed-chamber; to have rags dipped in it, and hung about the rooms of the house; to wash his hands and face with it night and morning; and to carry a small bottle of it with him, to smell to repeatedly in the course of the day: and this plan gave so much relief,—either by destroying the emanations, or by lessening the irritability of the mucous membranes,—that it was tried in other cases; and though it did not succeed in all, it did in most of them. Three patients out of four derived advantage from it. This expedient, therefore, is worth carrying in mind.

There is another vegetable substance, better known to us, which produces in some few individuals symptoms very like those of the hay asthma: I mean the powder of ipecacuan. I recollect a servant employed in the laboratory at St. Bartholomew's Hospital, when I was a pupil there, who had the peculiar ill luck to be liable to this affection. Whenever that drug was under preparation, he was obliged to fly the place. This idiosyncrasy is not very uncommon. A very small quantity of the ipecacuan dust is sufficient, in such persons, to bring on a paroxysm of extreme dyspnoea, wheezing, and cough, with singular anxiety and great weakness. The distress usually terminates by a copious expectoration of mucus.

These effects of a powdered root, and of certain emanations from grass or hay, lend weight to the hypothesis which ascribes the influenza to subtle vegetable matters floating in the atmosphere.

I would suggest a *trial* of the Respirator, as a defence against the particles of ipecacuan, and against the volatile exciting cause (whatever it may be) of hay asthma.

Catarrh is very often met with in a chronic form; in other words, the mucous membrane of the air passages is very liable to be affected with chronic inflammation. The accounts which you may read of this are exceedingly puzzling. Authors have endeavoured to draw nice distinctions between different *species* of chronic catarrh; sometimes according to varying qualities in the matter expectorated: thus you have *chronic mucous catarrh*—*pituitous catarrh*—*chronic pituitous catarrh*—and *dry catarrh*, which after all is *not* dry, but only accompanied by less expectoration than some of the others; and then again there is *symptomatic catarrh*. You will find all these enumerated by Laennec; and the majority of writers since his time have trodden with too much reverence in his footsteps. There are by no means such differences in the symptoms or the treatment of the several varieties of chronic inflammation of the membrane in question, as to make these numerous subdivisions of any practical utility. Chronic catarrh is often a sequela of acute bronchitis; it is a very common accompaniment of disease of the heart; it frequently arises during the course of the febrile exanthemata; it is seldom entirely absent in cases of continued fever; and it is a form of complaint that is full of interest on this account, if on no other, that it has so often been mistaken, and is so liable to be mistaken still, for tubercular consumption; of which indeed it is very frequently the companion.

The *constant* symptoms of chronic catarrh, or bronchitis, are cough, some shortness of

breath, expectoration of altered mucus. The *variable* symptoms, those which are oftentimes of the most importance, as determining the slight or serious character of the disorder, consist in the quantity and quality of the matters expectorated, and the presence or absence of wasting and hectic fever.

You will continually be meeting with cases of *this* kind. A person advanced in years has what he calls a slight cold, in the winter. He coughs, and expectorates a certain quantity of grey or transparent mucus. In the summer his cough diminishes or ceases altogether. The next winter the same thing happens again; and each successive return of the colder seasons of the year brings back in increasing severity the cough and the expectoration: and if you listen to the breathing of such persons, while the cough is on them, you will find crepitation at the lower part of their lungs. Now these are examples, I believe, of a chronic state of slight inflammation of the membrane,—or it may be of passive congestion and effusion,—depending upon slowly advancing *cardiac* alterations. Peripneumonia notha is very apt to supervene on this condition.

But chronic bronchitis may take place at any age, as a sequel to the acute: just as active inflammation of other parts of the body is liable to degenerate into the chronic form; and such cases are sometimes very equivocal and deceptive. Several years ago, a lady became my patient, having cough, expectoration of puriform matter, night sweats, and diarrhoea. She had had whooping-cough a short time before; but though the whooping, and other symptoms proper to that disease had ceased, she continued to cough, and to waste. Gradually she got thinner and weaker, her pulse became like a thread, and beat 120 times in a minute; she took to her bed, the diarrhoea was scarcely restrained by astringents and opiates, and I thought she could not live a week. And, upon being pressed by her brother for my opinion, I said so. She had scarcely allowed me to listen to the sounds in the thorax: but I had once done so fairly, and I could find no morbid sounds, except at the lower part of the lungs. If I had trusted to that circumstance alone, I should have said that she had not tubercular consumption; but I had not then so much faith in the indications afforded by auscultation, nor in my own accuracy of ear in such matters, as I might have now; and I concluded that she *was* dying of tubercular phthisis. Almost on the day, however, on which I ventured to give this prognosis, some slight amendment began: and she did gradually recover, and is alive and quite well at this time. Now it is in cases of this kind that

cures are performed by those who maintain that consumption is curable.

In truth, chronic bronchitis is, in some cases, as incapable of recovery, and as surely and progressively fatal, as tubercular phthisis itself: and even more so than some of the forms of phthisis. So long, however, as *no organic change* has taken place in the air tubes, or in the mucous membrane lining them, these chronic forms of bronchitis that simulate phthisis in their general symptoms, are within the reach of cure. They are to be treated by counter-irritants to the chest—and by such measures as are calculated to relieve the most urgent symptoms. Opiates for cough, or for diarrhoea. Sometimes the patients bear steel well, and then it is almost sure to have a beneficial effect. Sometimes sarsaparilla appears to do good; but as far as I have observed, one of the most effectual restoratives in these cases is to be found, when the weather and the strength permit, in frequent change of air and place; in gentle gestation in a carriage, or in a boat; and in a nourishing but bland and unstimulating diet. When the membrane, and the tubes which it lines, become *altered in structure*, and pour forth a fluid which has all the qualities of pus, hectic fever generally is present, and the chronic disease tends, slowly perhaps, but surely, to death.

There are certain cases of chronic bronchitis which are especially remarkable, on account of the great abundance of the bronchial secretion: so great that the patients appear to die principally from the daily exhausting drain thus made upon the system. There are sometimes no other evident signs of inflammation; so that, as Andral observes, one might be led to separate these fluxes from the truly inflammatory affections. They differ from them apparently in their nature, and certainly in the treatment which they require. Andral has detailed two or three instances of this kind in his *Clinique Médicale*. The patients expectorated every day large quantities—a pint or more—of frothy fluid, resembling weak gum-water in colour and consistence. They had no fever; neither frequency of pulse nor heat of skin; but they were exceedingly pale, like persons blanched by hæmorrhage, and their emaciation and weakness were also extreme. Very little appreciable deviation from the healthy state was detectible when the lungs and heart were examined after death.

It does occasionally happen that even larger quantities—three or four pints daily—are, for a considerable period, spat up, without much wasting.

Andral asks, whether, in such cases as these, which certainly occur, though they are not very common, the first indication of treatment should not be to check and diminish the excessive bronchial secretion; to treat it

as you might treat a gleet of the other mucous membranes, with balsams, administered either by the stomach, or in the shape of vapour. He conjectures that it might have been in cases of this nature that the vapour of tar, and tar water, were once thought to be so useful. Probably the creasote would be well adapted to such cases. Certainly I have seen the excessive expectoration diminish, and the patients gain strength, under the use of the balsams; the compound tincture of benzöes, for example; a form of medicine much employed formerly, and too much neglected, I apprehend, at present. Another remedy from which I have derived great advantage in some cases of the same kind, is the sulphate of iron, given in two or three grain doses, in the compound infusion of roses, thrice daily. When there is any fever present, these remedies are apt to augment it: but when the pulse is quiet, and the skin cool, I am quite sure that they are often of the greatest service; and this you will find to be the opinion also of various practical authors.

There is another very remarkable species of chronic inflammation of the same parts, characterized also chiefly by the matter expectorated. I mean that in which a firm substance, like a false membrane, forms in the smaller bronchi and their ramifications, and is coughed up, from time to time, in fragments. I mentioned in a former lecture that the false membrane of croup sometimes descends a long way into the bronchi; even to their extremities. But I am speaking now of a less acute form of disease, in which moulds as it were, of portions of the bronchial tree are spat up; somewhat like bunches of worms, or the roots of a small plant. This I presume to be uncommon; for I have only once seen it. It has been described, however, by several observers. The first Dr. Warren, has a paper upon it in the first volume of the *Medical Transactions*, where he gives representations of the substances coughed up, which he calls *bronchial polypi*. Dr. Paris has told me that a patient of his coughed up considerable quantities of these branching casts of the ultimate air tubes, now and then, for a long period. A paper of Mr. North's, on the same subject, was read at one of the evening meetings of the College of Physicians. That gentleman possesses some beautiful specimens of these miscalled polypi. Dr. Carswell gives a figure representing them. When the affection is extensive, it is attended with great distress, and dyspnoea, and violent fits of coughing; and the symptoms are wonderfully calmed upon each expulsion of the albuminous matter. The surprise is that such patients should ever recover. In the solitary instance which has fallen under my observa-

tion, the affection was chronic and partial. A stout, healthy, middle-aged barrister, after having had some huskiness of voice for a twelvemonth, suddenly spat a little blood; and soon afterwards he expectorated one or two ramifying masses, of tolerably firm consistence, resembling fibrinous coagula of blood, deprived of most of its colouring matter. They were not hollow. I found slight circumscribed crepitation in the lower and posterior part of his left lung. This trivial degree of hæmoptysis, with the expulsion of what looked like casts of the interior of a bronchial tube, was once or twice repeated within a few days. Mercury was, *inter alia*, prescribed, but as the patient did not feel in any way ill, I believe he soon became tired of physic; and when I last saw him he appeared to be in perfect health.

A word or two, before we separate, as to the morbid anatomy of these tissues.

Chronic inflammation of the aerial mucous membrane may lead to changes in its colour; or to thickening of the membrane; or to ulceration; or to dilatation of the bronchi, and their ramifications. And it is proper that you should be informed respecting these morbid conditions.

In general, when chronic inflammation has existed during life, the mucous membrane is found to be red: but it is not a bright redness; it is rather a livid, or violet, or brownish tint. And what is very curious, in some instances in which all the symptoms of inveterate bronchitis, with *puriform* expectoration, had been present, the inner membrane of the air passages has been found scarcely rosy—or even perfectly white—throughout its whole extent. Of course we are not to infer from this that there has not been *inflammation*; for the same thing is known to occur in the intestinal mucous membrane, in that of the bladder, and even in serous membranes. Where pus is poured forth there must have been inflammation.

One effect of inflammation, as I formerly shewed you, is a softening of the membrane; but this is a much less common result of inflammation in the mucous membrane of the bronchi, than in that of the digestive organs. In regard to ulceration likewise there is a great difference between the two mucous surfaces: in that of the air passages it is comparatively rare.

Thickening of the membrane occurs also in various degrees: but the most remarkable change undergone by the membrane, and the tubes which it lines, is the dilatation of those tubes, and the consequent alteration of the membrane, which expands with them.

There are two or three varieties of this dilatation. In the first of them, one or more of the bronchi present, throughout the whole or the greater part of their extent,

an increase of capacity more or less considerable: so that tubes which result from the 4th or 5th, or even 6th division of the principal bronchus, of each lung, may equal or exceed in diameter that bronchus itself. Tubes that ought not to be bigger than a crow quill may become as large as the finger of one's glove. Sometimes this kind of dilatation is seen in a single branch only, sometimes in many. It may affect the bronchial ramifications of an entire lobe. It is more common in the branches of a bronchus than in the bronchus itself.

It is not very easy to explain the manner in which this sort of dilatation is produced. We might attribute it to simple distension of the bronchial parietes, were it not that these parietes are at the same time thickened, and the circular fibres hypertrophied, as you may see in this preparation, and in Dr. Carswell's plate. But there is another form of bronchial dilatation to which the explanation just adverted to is easily applicable. Instead of the uniform dilatation of one or more bronchial tubes, throughout their whole extent, we find a bellying, or globular expansion, at the extremity of one of them; and the walls of the tube, instead of being thick and hypertrophied, are wasted, and in a state of atrophy. The tissues composing the tube are often so thin, that when the cavity, for such it must be called, is laid open, the colour and structure of the pulmonary tissue may be seen through them. These cavities are generally found filled with a thick, tenacious, straw-coloured, mucopurulent fluid. Now it is easy to conceive how "the straining influence of repeated paroxysms of coughing" may cause dilatation of this kind. The primary branches are more easily cleared of the mucus that fills them: but the pressure which the lung undergoes under a forced expiration, operating on a portion of the same kind of mucus detained in the smaller branches, may be more than the elasticity of the tube is capable of resisting. And, in fact, all dilatation of the bronchial tubes must be in part at least owing to the same influence of pressure by imprisoned mucus against their inner surface. They are seldom met with except after those affections which are characterized by considerable secretion from the membrane, and by much and repeated cough: as after some forms of chronic bronchitis, and after whooping-cough.

A third variety of dilatation is that in which the same bronchus bellies out in different places; is dilated at intervals; so as to present in its course a series of successive enlargements and contractions. Here, again, the walls of the bronchi, though they may be traced in the parts dilated, do not appear to be thickened, but rather are diminished in thickness. We may suppose, therefore, that

these small partial dilatations of the bronchi may result from mechanical distension, by mucus, in those places which offer the least resistance; either on account of diminished elasticity, or of actual thinning. This variety of dilatation is more frequent in children than in adults.

In whatever way the dilatation may take place, one of its obvious and necessary consequences is, the condensation of the pulmonary substance around the dilated tube, the obliteration of some of the cells, and a proportional abridgment of the function of the lung. Accordingly, when it is extensive, dilatation of the bronchi is attended with habitual dyspnoea.

But the most important consideration arising out of this state of the bronchi, is this; that the signs, both general and physical, by which it is accompanied, are apt to be exactly those which are most distinctive of phthisis. And it is on that account that I have now described these changes. I shall revert to them again when I come to the symptoms, revealed by auscultation, of tubercular disease of the lungs.

LECTURES ON THE FUNCTIONS OF THE NERVOUS SYSTEM.

BY W. B. CARPENTER, M.D.

LECTURE X.—(*concluded.*)

Functions of the Cerebrum (continued.)

It is interesting to compare the progressive development of the cerebrum, and of the intellectual powers, which we observe in ascending the scale of Vertebrata, with the degree in which the young of the different classes are respectively assisted by their parents in their development. In fishes and reptiles, the eggs are usually left to the mercy of the elements; in the former they are commonly subjected to a moderate temperature, being immersed in water which is seldom either very warm or very cold; whilst in the latter, they are usually more exposed to the direct solar heat,—and this even before they are deposited. The reptiles whose eggs are laid in water, or in obscure places, are for the most part those which belong to the order Batrachia, and which, beginning life in the condition of fishes, never attain to any very elevated grade of development. There is a curious exception among fishes in the case of the Ray and Shark tribe, many of which retain their eggs within the oviducts until the young are hatched, and these form a new connection with the parent, comparable in some degree with that which takes

place in Mammalia; and in them occurs the only trace of convolutions, which we anywhere meet with among oviparous animals. In Birds, the close attention paid by the parents to the eggs, the constantly-elevated temperature afforded by them, and the assistance subsequently afforded to their young, often for some time after their emersion from the egg, harmonize well with the advanced development of the cerebrum; and it will be observed that this assistance is most remarkable, and is most prolonged, in proportion to the degree of intelligence which the species is ultimately to attain. Thus, the chicken can pick up its own food almost immediately that it is liberated from its shell, and is never directly supplied by its parent; whilst the insessorial and raptorial birds generally sustain their young for some time by the supplies of food which they obtain for them; and the parrot is enabled, by a peculiar conformation, to furnish its progeny with food moistened by a secretion of its own, which much resembles milk. On turning to the Mammalia, we are at once struck with the marked difference in the development of the cerebrum, that exists between the orders in which the young forms a true placental connection with the parent, and those ovo-viviparous tribes (as they have been termed) in which it quits the uterus prematurely; and this difference extends, as already stated, to internal structure, as much as to size. The period of dependence of the young upon the parent is far more prolonged in man than in any other animal; and this not merely absolutely, but in reference to the entire duration of life; during the whole of this time he is unfit to provide for himself; but the development and education of his faculties is continually proceeding, so that he at last advances far beyond the grade of any of those inferior animals, with which he may be reasonably compared (in regard to the manifestation of his psychical endowments) at an earlier period.

Comparative anatomy, then, fully bears out the general doctrine, that the cerebrum constitutes the organ of intelligence, as distinguished from those mere instincts by which many of the lower animals seem to be almost entirely guided. By intelligence, I do not mean, however, the reasoning faculties only, but the combination of those powers which are of an educable character, and which become the springs of *voluntary* action in very different proportions in different animals of the same tribe;—as distinct from those which have an immediate relation to the wants of the corporeal system, and which are *automatic* and invariable in the several individuals of the same species. Observation of the human species exhibits the same distinction. When the brain is

fully developed, it offers innumerable diversities of form and size among various individuals; and there are as many diversities of character. It may be doubted if two individuals were ever exactly alike in this respect. That a brain which is greatly under the average size is incapable of performing its proper functions, and that the possessor of it must necessarily be more or less idiotic, there can be, I think, no reasonable doubt. On the other hand, that a large well-developed brain is found to exist in persons who have made themselves conspicuous in the world by their attainments or their achievements, is, I think, a proposition of equal generality. In these opposite cases, we witness most distinctly the antagonism between the instinctive and voluntary powers. Those unfortunate beings, in whom the brain is but little developed, are guided almost solely by their instinctive tendencies, which frequently manifest themselves with a degree of strength that would not have been supposed to exist; and occasionally new instincts manifest themselves, of which the human being is ordinarily regarded as destitute*. On the other hand, those who have obtained most influence over the understandings of others, have always been themselves persons of strong volitional powers, in whom the instinctive tendencies are quite subordinated to the will, and who have given their whole energy to the particular object of their pursuit. It is very different, however, with those who are actuated by what is ordinarily termed *genius*, and whose influence is rather upon the *feelings* than upon the understandings of those around them; these are frequently very deficient in power of even comprehending the ordinary affairs of life; and still more commonly, they show an extreme want of judgment in the management of them, being under the immediate influence of their passions and emotions, and not having brought these under the control of their intelligent will. The life of a *genius*, whether his bent be upon poetry, music, painting, or upon pursuits of a more material character, is seldom one which can be held up for imitation. In such persons, the general power of the mind being low, the brain is not usually found of any great size. The *mere* comparative size of the brain, however, affords no accurate measure of the amount of mental power; we not unfrequently meet with men possessing large and well-formed heads; whilst their capacity is

* A remarkable instance of this has been recently published. A perfectly idiotic girl, in Paris, having been seduced by some miscreant, was delivered of a child without assistance. It was found that she had *gnawed* the umbilical cord in two; in the same manner as is practised by the lower animals. It is scarcely to be supposed that she had any idea of the *object* of this separation.

not greater than that of others, the dimensions of whose crania have the same general proportion, but are of much less absolute size. Large brains, with deficient activity, are commonly found in persons of what has been termed the *phlegmatic* temperament, in whom the general processes of life seem in a torpid and indolent state; whilst small brains and great activity betoken what are known as the *sanguine* and *nervous* temperament. These distinctions come to be very important, where we proceed further in our inquiries, and attempt to determine the particular modes of development of the brain which coincide with certain manifestations of the mind. To this subject we shall presently revert.

Having now inquired into the evidence of the *general* functions of the cerebrum, which may be derived from examination of its comparative development, we proceed to our other sources of information,—experiment, and pathological phenomena. The effects of the entire removal of the hemispheres have been already described. In these and similar experiments it has been constantly remarked, that injuries of the cerebral substance do not occasion signs of pain, and that they do not give rise to any convulsive movements. Even the thalami optici and corpora striata may be wounded, without the excitement of convulsions; but if the incisions involve the tubercula quadrigemina, or the medulla oblongata, convulsions uniformly occur. This result perfectly accords with what has been observed in man; for it has been frequently remarked, when it has been necessary to separate protruded portions of the brain from the healthy part, that this has given rise to no sensation, even in persons whose mind has been perfectly clear. The effect of pressure upon the brain is well known to be the suspension of all its operations: this has been substantiated by experiments upon animals, and also by similar experiments on persons who have had a portion of the cranium removed, so as to expose the membranes of the brain: the pressure of the finger upon the membranes occasions a state of immediate unconsciousness, resembling profound sleep, which ceases as soon as the pressure is withdrawn. Such pressure will, of course, affect the whole encephalon, and not the cerebrum alone. Experiment does not throw much light on the particular functions of the corpus callosum and other commissures; since they can scarcely be divided without severe general injury. It would appear, however, that the partial or entire absence of these parts, reducing the cerebrum to the level of that of the marsupial quadruped or of the bird, is by no means an unfrequent cause of idiocy. The information afforded by pathological phenomena is far from being definite.

Many instances are on record in which extensive disease has occurred in *one* hemisphere, so as almost entirely to destroy it, without either any obvious injury to the mental powers, or any interruption of the influence of the mind upon the body. But I believe that there is no case of severe lesion of *both* hemispheres, in which morbid phenomena were not evident during life. It is true that, in chronic hydrocephalus, a very remarkable alteration in the condition of the brain sometimes presents itself, which might *à priori* have been supposed destructive to its power of activity,—the ventricles being so enormously distended with fluid, that the cerebral matter has seemed like a thin lamina, spread over the interior of the enlarged cranium. But there is no proof that absolute destruction of any part was thus occasioned; and it would seem that the very gradual nature of the change gives to the structure time for accommodating itself to it. This, in fact, is to be noticed in all diseases of the encephalon. A *sudden* lesion, so trifling as to escape observation, unless this be very carefully conducted, will occasion very severe symptoms; whilst a chronic disease may gradually extend itself without any external manifestation. It will usually be found that sudden paralysis, of which the seat is in the brain, results from some slight effusion of blood in the substance or neighbourhood of the corpora striata; whilst, if it follow disorder of the brain of long standing, a much greater amount of lesion will usually present itself. In either case, the paralysis occurs in the opposite side of the *body*, as we should expect from the decussation of the pyramids; but it may occur either in the same or on the opposite side of the *face*, the cause of which is not very apparent. If convulsions accompany the paralysis, we may infer that the corpora quadrigemina, or the parts below, are involved in the injury; and in this case it is found that the convulsions are on the affected side of the body. Where, as not unfrequently happens, there is paralysis of one side, accompanying convulsions on the other, it is commonly the result of a lesion affecting the base of the brain and medulla oblongata, on the side on which the convulsions take place. Many anomalies present themselves, however, which are by no means easy of explanation, consistently with the present state of our knowledge.

The general result of such investigations is, that the cerebrum is the organ through which all those impressions are received which give rise to *voluntary* actions, and that it affords the power of occasioning muscular contraction in obedience to the influence of the will; but that the fibres composing its medullary portion are not susceptible of being thrown into action by

mechanical irritation, in the same manner as are those of the spinal cord and nerves,—a difference which may, perhaps, be connected with the slight difference of their structure, explained in the first lecture. There is no positive reason for the belief, that the cerebrum is essential to the purely emotional actions; and analogy, as we have seen, applied to the explanation of pathological phenomena, would lead to the belief that *their* channel is different. It can scarcely be denied, however, that in the cerebrum resides that power, by which the attention of the mind is directed to any sensation, and by which, through the medium of a brief reasoning process, a notion is formed regarding its nature: this operation is altogether termed *perception*. Now it will be presently seen, that the formation of such elementary notions in us is often a complex process, though a rapid one; whilst, in many of the lower animals, it appears to be very much simpler—as to all those points, at least, which concern the instinctive actions necessary for their well-being. Such *intuitive* perceptions occasionally take place in ourselves; and I think it will appear, from examination of them, that they are connected either with the mere instincts, or with the emotions.

Some metaphysicians have confounded *perception* with *sensation*; but I think the difference is very evident. In order that a *sensation* should be produced, a *conscious* state of the mind is all that is required. Its whole attention may be directed towards some other object, and the sensation calls up no new ideas whatever; yet it will produce some change in the sensorium, which causes it to be (as it were) registered there for a time, and may become the object of subsequent attention; so that, when the mind is directed towards it, the idea or notion of the cause of the sensation is formed, which constitutes a perception. For example, a student, who is directing his thoughts to some object of earnest pursuit, does not receive any intimation of the passage of time from the striking of a clock in his room. The sensation must be produced, if there be no defect in his nervous system; but it is not attended to, because the mind is bent upon another object. It *may* produce so little impression on the mind as *not* to recur spontaneously, when the train of thought which previously occupied the mind has been closed, and the attention is ready to be directed to any other object; or, the impression having been stronger, it may so recur, and at once excite an idea in the mind. Again, the individual may then be able only to say that he heard the clock strike; or he may be able to retrace the number of strokes. Now, in either case, a simple perception is formed, without his being aware that any

mental operation has intervened. He would say that he remembers hearing the clock strike; but this would not express the truth. That which he remembers is a certain series of sonorous impressions, which was communicated to his mind; and he recognizes them as the striking of a clock, by a process in which memory and judgment are combined,—which process may further inform him, that the sounds proceeded from his own particular clock. If he had never heard a clock strike, and the sound produced by it had never been described to him, he would not have been able to form that notion of the object that gave rise to the sensation, which, simple as it appears to be at the time, is the result of complex mental operations. But when these operations have been frequently performed, the perception or notion of the object becomes inseparably connected with the sensation; and thus it is excited by the latter, without any knowledge on the part of the individual that a mental operation has taken place. Such perceptions are termed *acquired*, in contradistinction to the *intuitive* perceptions, of which the lower animals seem to possess a large number. The idea of the distance of an object, for example, is one derived in man from many sources, and is the result of a long experience; the infant, or the adult, seeing for the first time, has to bring the senses of sight and of touch to bear upon one another, in order to obtain it; but when once the power of determining it is acquired, the steps of the process are lost sight of. In the lower tribes of animals, however, in which the young receive no assistance from their parents, there is an evident necessity for some immediate power of forming this determination, as they would not be able to obtain their food without it. Accordingly they manifest in their actions a perception or governing idea of distances, which could only be gained by man after long experience. A fly-catcher, for example, just come out of its shell, has been seen to peck at an insect with an aim as perfect, as if it had been all its life engaged in learning the art. In some instances, animals learn by intuitive perception that at which man could only arrive by the most refined processes of reasoning, or by the careful application of the most varied experience. Thus, a little fish, named the *Chætodon rostratus*, is in the habit of ejecting from its prolonged snout drops of fluid, which strike insects that happen to be near the surface of the water, and which cause them to fall into it, so as to come within its reach. Now, by the laws of refraction of light, the place of the insect in the air will not really be what it appears to be to the fish in the water, but it will be a little below its apparent place, and to this point its aim must be directed. But the

difference between the real and the apparent place will not be constant; for the more perpendicularly the rays enter the water, the less will be the variation; and, on the other hand, the more oblique is the direction, the greater will be the difference. Now it is impossible to imagine but that, by an intuitive perception, the real place of the insect is known to the fish in every instance, as perfectly as it could be to the most sagacious human mathematician, or to a clever marksman who had learned the requisite allowance in each case by a long experience. In man, the acquirement of perceptions is clearly a cerebral operation; I cannot but regard their intuitional formation, in the lower animals, as one of those to which the ganglia connected with the organs of special sense, which are in them of so great a proportional size, are subservient.

Many physiologists and metaphysicians are of opinion, that *every* sensation actually experienced *may* become the subject of a perception at any future time, though beyond the voluntary power of the memory to retrace; and the phenomena of dreams and delirium, in which these sensations often recur with extraordinary vividness, afford much support to this doctrine. Some of the instances upon record are remarkable, as proving that the sensations may be thus remembered, without any perceptions being attached to them; these sensations having been of such a nature, as not to excite any notion or idea in the mind of the individual. Of the nature of the change by which sensations are thus registered, it is in vain to speculate; and it does not seem likely that we shall ever become acquainted with it. This is certain, however,—that disease or injury of the brain will destroy this power, or will affect it in various remarkable modes. You will not unfrequently meet with cases in which the brain has been weakened by attacks of epilepsy or apoplexy, in such a manner as to prevent the reception of any new impressions; so that the patient does not remember any thing that passes from day to day, whilst the impressions of events which happened before the commencement of his malady recur with greater vividness than ever. On the other hand, the memory of the long-since-past is sometimes entirely destroyed; whilst that of events which have been subsequent to the malady is but little weakened. The memory of particular classes of ideas is frequently destroyed;—that of a certain language, or some branch of science, for example. The loss of the memory of words is another very curious form of this disorder, which you will not unfrequently meet with: the patient understands perfectly well what is said, but is not able to reply in any other terms than yes and no,—not from any paralysis of the muscles of articulation,

but from the incapability of expressing the ideas in language. Sometimes the memory of a particular class of words only, such as nouns or verbs, is destroyed; or it may be impaired merely, so that the patient mistakes the proper terms, and speaks a most curious jargon. These cases have a peculiar interest in reference to the final subject of our inquiry.

That the different portions of the cerebrum have different functions in the complex operations of thought, must, I think, be admitted to be by no means an improbable speculation; and you are well aware that, under the name of phrenology, or the science of mind, a systematic allocation has been made, of what have been regarded as the several fundamental powers and faculties of the mind, to certain parts of the cerebral hemispheres. This was first attempted by Gall, who states himself to have been guided in his determinations, by observing on the heads of those, who manifested any remarkable faculty or tendency, a corresponding prominence; and to have found confirmation of his inferences, by comparing in like manner the skulls of the lower animals with their peculiar powers and dispositions. Both these branches of inquiry have been taken up by numerous observers; and a large amount of evidence has been adduced by them in support of Gall's views, which appears in itself plausible, and which is regarded by many physiologists of much intelligence as quite decisive. Nevertheless I do not think that the system is widely received amongst those whose peculiar attention to the physiology and pathology of the nervous system give them the highest authority on the subject; and much additional proof appears to me to be requisite, before it can take rank as substantially true. It may be freely admitted that mankind is in the habit of forming an impression of an individual's intellectual capacity by the height and expansion of his forehead; and that a low forehead and crown, with great development of the occipital portion of the brain, generally accompanies a character in which the influence of the animal passions is predominant; and correspondencies even more detailed may be admitted, without the inference being then conclusive, that these several parts are the distinct organs of the faculties of which we judge by their relative size. It seems to me to be in regard to the form of the head, very much as in respect to the character of the face,—that we may draw from it a general idea as to the character of the mind, and not unfrequently be able to predicate correctly some minute details; and yet that an attempt to localize the organs more minutely, may be as destitute of truth as were the details of the system of Lavater. It appears to me, also, that a fundamental

doubt hangs over every determination of function, which results from a comparison of the size of the supposed organ or region in different cases. If it be true that the grey matter only is the source of power, and that the white is merely a conductor, I cannot see that we have any right to assume that the total size of the organ affords a measure of its power, until it has been shown that the thickness of the cortical substance can be judged of by the size of the brain, or of any part of it. Certainly there is a considerable variation in this respect among different individuals; and it is yet to be proved that the relation is constant in different parts of the same individual brain. Until this is substantiated, all inferences drawn from correspondence between the prominence of a certain part of the brain, and the intensity of a particular function, seem to me invalid. Moreover, there is unfortunately a considerable uncertainty attending all phrenological observations which are made upon the cranium, rather than upon the brain; this you have seen from the discrepancy between the statements of Gall, and facts ascertained regarding the comparative weight of the cerebellum in castrated and entire horses. It appears to me, too, that comparative anatomy and psychology are very far from supporting the system, when their evidence is fairly weighed. You will hear a great deal said by phrenologists respecting M. Vimont's examination of this question, and of the affirmative decision to which he has come; but you will probably not learn from them, that M. Leuret, from at least equally extensive observations, has arrived at an opposite conclusion. Of these two, if authority is to decide the matter, I should certainly give the preference to M. Leuret, as a man of general eminence, and one who had a reputation to lose; whilst M. Vimont was previously unknown, and has only brought himself into eminence by his advocacy of phrenology. It is a very curious circumstance, that the difference between the antero-posterior diameters of the brains of man and those of the lower mammalia, principally manifests itself in the *posterior* lobes, which are commonly so short as not to cover the cerebellum. As an instance of the want of conformity between phrenological principles and facts, I may direct your attention to the relative dimensions of the brains of the Falcon and the Parroquet. From the character of the former, you would have no hesitation in saying, that it ought to have the organ of Destructiveness (the size of which organ may, I believe, be generally estimated pretty nearly by the transverse diameter of the brain) in a far larger degree than the latter. Nevertheless, on looking at the table already given, you will see that the transverse diameter of the brain of the Falcon

is not only absolutely smaller than that of the Parroquet, but is no larger in proportion to the antero-posterior diameter. Many other such instances might be adduced; and these I think would collectively show the uncertainty, to say the least, of the inferences by many regarded as firmly established. The evidence of pathology, again, tends to show, that particular disorders of function may result from lesions of any part of the cerebral hemispheres; this has been especially noticed, for example, in regard to the loss of the memory of words, which phrenologists locate in the organ of language; there, of course, the lesion might be expected, on their system, to present itself; but this is by no means constantly, or even generally, the case. Phrenologists lay great stress on the effects of local injury in causing loss of memory of a particular subject; but this principle, if carried to its full extent, would require us to regard each organ as split up into a large number of subdivisions,—the organ of language, for example, having one store house for Latin, another for Greek, &c.; either of which may be destroyed, without the other being affected. A very important source of evidence is that afforded by the correspondence between the several kinds of monomania, and the forms of the brains of the persons exhibiting them; and the number of those who, having studied this question, have given in their adhesion to the phrenological view, is, I think, one of the most weighty evidences of its containing much truth. The doubts which I have expressed on the subject would have much less weight, if the coincidence of phrenological determinations of character, with truth, were more constant. The fairest tests of these are to be found, as Dr. Holland has justly remarked, “not in vague and ill-defined moral propensities, but in a few simple and well-marked faculties, such as those of numerical calculation, languages, or music, which have no others in actual opposition to them, and the degree of perfection in which can be clearly defined.” We hear much from phrenologists, as to their successful application of these tests; but we do not hear of the instances of failure. My own experience of their determinations, however, has certainly led me to the belief, that failure is nearly as frequent as success. I am not wishing to set myself up as an opponent to phrenology; but I perfectly agree with Dr. Holland in thinking that an impartial view of it requires, “not that the doctrine should be put aside altogether, but that great abatement should be made of its pretensions as a system.” In particular I think that those who pursue it are bound to make themselves first acquainted with what can be established as the general functions of the brain, before descending to particulars.

CASE OF CÆSAREAN SECTION.

BY

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Manchester.

[Concluded from page 947.]

[For the London Medical Gazette.]

THE following are the principal measurements of the pelvis in its dried state:—

External dimensions.

1. From one crista ilii to the other at their most distant points, $9\frac{3}{4}$ inches.
2. From one anterior superior spinous process to the other, $7\frac{1}{4}$.
3. Between the anterior inferior spinous processes of opposite sides, $4\frac{3}{4}$.
4. Perpendicular height from apex to base of os sacrum, $1\frac{6}{10}$.
5. From apex of sacrum to the top of the fifth lumbar vertebra, 3.
6. Anterior depth from the top of the symphysis pubis to a level with the tuberosities of the ischia, $3\frac{3}{4}$.
7. Distance betwixt the ossa pubis at the junction of their rami with those of the ischia, $\frac{2}{10}$.
8. Distance betwixt the rami pubis half an inch below the symphysis where the urethra passes out, $\frac{4}{10}$.
9. Betwixt the inner limbs of the acetabula, $2\frac{1}{4}$.

Dimensions of the superior aperture, cavity and outlet.

10. Antero-posterior diameter at the middle of the inlet on the right side, $1\frac{2}{10}$.
Ditto left side, $\frac{7}{10}$.
11. Ditto one inch posterior to this—right side, $1\frac{1}{4}$.
Ditto—left side, $\frac{9}{10}$.
12. Greatest antero-posterior diameter, $1\frac{3}{10}$.
13. From the body of the pubis, left side, to the sacro-iliac joint on the right, $3\frac{2}{10}$.
Ditto on the opposite side, $3\frac{5}{10}$.
14. From symphysis pubis to the junction of fourth and fifth lumbar vertebræ, $2\frac{9}{10}$.
15. From the body of one os pubis to that of the other, $\frac{1}{4}$.
16. From the line of this last (fifteenth) measurement backwards to the junction betwixt the fourth and fifth lumbar vertebræ, $1\frac{3}{10}$.
17. Betwixt the plane surfaces of the ischia opposite the centres of the acetabula, $2\frac{1}{10}$.
18. Betwixt the outer limbs of the foraminæ ovalia, $1\frac{4}{10}$.
Distance betwixt their inner limbs, $\frac{3}{10}$.
19. Betwixt the tuberosities of the ischia at their anterior extremities, $\frac{4}{10}$.

20. Ditto at their middle, 1.

21. Ditto at their posterior extremities, $2\frac{1}{4}$.22. Betwixt the spinous processes of ischia, $2\frac{3}{4}$.23. From the apex of the sacrum (or rather from the large bone of the coccyx which is ossified to the sacrum) to the tuberosity of the ischium, left side, $2\frac{1}{4}$.

Ditto, right side, 2.

24. Distance betwixt the point of os coccygis to the tuberosity of ischium, left side, $1\frac{8}{10}$.Ditto, right side, $1\frac{5}{10}$.

The first or large bone of the coccyx was joined to the apex of the sacrum by osseous union, so that the point of the coccyx only was moveable. This circumstance, together with the very considerable advance of the lower part of the sacrum, and the near approach of the tuberosities of the ischia to each other, reduced the outlet to a very small space. The largest circle that could be formed at this part, the sides of it impinging forcibly against the sacro-sciatic ligaments, which are at all times very unyielding, was less than an inch in diameter.

With these dimensions few, I apprehend, could conscientiously recommend delivery by craniotomy in preference to the means that were adopted. For, had such an operation been practicable, the amount of suffering to the patient during its performance must have been infinitely greater, and the chances of recovery very far less in such a subject; and this, too, in the outlets, at the expense of the life of a healthy child. It would be useless burthening this paper with a number of quotations as to the rules which ought to guide us in cases of extreme difficulty: on this subject most writers agree in the statement made by Burns, in his Principles of Midwifery—"that the crotchet cannot be used when the child is of the full size, unless we have a passage through the pelvis measuring fully an inch and three-quarters in the short diameter, and three inches in length; or, if the child be premature and soft, an inch and a half broad, and two inches and three-quarters long. It is in this extreme deformity even questionable whether extraction be not as dangerous as the Cæsarean operation; and we always ought to consider well before we give the preference to mutilation in such cases." But I do not believe it would have been possible, had the short diameter been fully an

inch and a half, or even an inch and three-quarters, the other measurements being as they were, to have delivered by the crotchet in this case; for the external opening was so small that it was with the greatest difficulty a man's hand of the smallest dimensions could be at all introduced. By steadily persevering, however, for some time, part of the hand might be got within the pelvis, but the attempt always produced the most acute suffering, and manipulation was utterly impossible, as there was scarcely room for the movement of a finger, and none for the passage of an instrument with which to operate. The antero-posterior diameter of the upper aperture (one inch and three-tenths in the skeleton), would only admit two fingers placed together, being considerably narrowed by the soft parts, especially the bladder, the neck of which descended exactly at this part. The space posterior to this was very much encroached upon by the *psoæ* and iliac muscles, and the other organs situated in this region. Thus, by the addition of the soft parts, the dimensions of the upper aperture must have been considerably less than those of the skeleton as given in the above table: what the exact difference was cannot now be known; but I feel no hesitation in saying that there could have been but little more than one inch betwixt the pubis and last lumbar vertebra at their most distant opposing points, and that all the other measurements in an antero-posterior direction were still less. The great depth of the pelvis, too, was a serious inconvenience, and also the contraction existing, not only at the upper and lower apertures, but through the entire extent of the pelvic cavity, the planes of the ischia (see measurement 17) being little more than two inches apart, and the distance between each of these points and the lumbo-sacral joint considerably less than an inch and a half. The pendulous position of the uterus was another and very serious obstacle to manual interference by the natural passages. The abdomen hung forward like a bag, resting on the upper parts of the thighs, and descending to some extent; so that the axis of the uterus took a direction downwards and forwards; a position which, were it possible to exist in a subject where the pelvis had the ordinary normal dimen-

sions, might foil, perhaps, an experienced practitioner in the more simple operation of turning, or even in the extraction of an adherent placenta.

It was thought by some who examined the pelvis before the operation, that its dimensions were not less in 1840 than they were in 1834, when delivery was effected by *embryulcia*, and that the disease of the bones must have been arrested. As to the state of the patient in these respects, no one who had noted the symptoms and appearances accurately could remain in doubt. From having so frequently examined the pelvis myself during the preceding three years and a half, I felt no hesitation in saying that the deformity had gone on increasing during each pregnancy, and especially during the latter period of the last. The contraction had increased considerably at the upper aperture, but much more in the cavity betwixt the planes of the ischia, and at the outlet. That the disease had progressed also during the same period, the state of the system and the effect produced on the bones sufficiently attest. All the symptoms of which she complained were as decided, and at times as severe, as those manifested when the disease commenced. Her habit of body was cachectic to the last degree. The complexion was sallow, skin thin and loose, muscular system soft and extremely attenuated, the adipose tissue completely absorbed, and the whole body reduced to an extreme degree of emaciation. The bowels were irregular in their action, being sometimes too much confined, sometimes in the other extreme; and now and then considerable quantities of worms were voided. The perspirations were often profuse, and seldom general, being at one time limited to the head and neck, or to the chest and abdomen, and sometimes to one side of the body, the rest of the surface remaining comparatively cool and dry. The urine was large in quantity, and occasionally deposited a whitish cretaceous sediment. The pains in the back and around the pelvis had become greatly aggravated during the latter three or four months of the last pregnancy, and her powers were so feeble as to disable her from walking, without assistance, across her bed-room. While utero-gestation was going on, the stomach always became particularly irri-

table, and when able to take a tolerably good meal could seldom retain it. In addition to these evidences, the state of the bones may be cited as another and perhaps more decided proof that the disease was actively progressing at the time of the operation. The pelvis is very light and thin, the ossa ilia being quite transparent at the middle of their alæ, where they are perforated with several holes. Had the disease been arrested, the bones would have received some repair, and have become strengthened; the result, probably, of some modification in the absorbent process, and improvement in that of nutrition. In short, the osseous system must have regained its natural constituent proportions to have enabled it to sustain the weight of the body and its ordinary movements without injury. Two pieces of bone were taken, one from the crest of the ilium on the left, and the other from the thigh-bone on the right side, which together weighed 115 grains. These, having been previously well dried, were repeatedly macerated during several weeks in strong acetic acid, until nothing more was deposited on evaporation. The earthy material obtained by these processes weighed 45 grains; this remains to be further analysed. From this result it appears that the relative proportions of the animal and earthy constituents of the bones in this case are nearly reversed.

In this extremely reduced condition the human body is but ill calculated to resist the invasion of disease, the nature of which will of course be determined by idiopathic predisposition, or by the peculiar effect of any exciting cause which may be more immediately brought into operation upon it. Is it not probable that the new action in the function of nutrition, which is established during pregnancy for the peculiar purposes of gestation, being deprived of its necessary supplies, which we suppose are received through the same channel which furnishes nutrition for the maternal system, should have a tendency to abstract them from another source, and thus become one of the principal causes of malacosteon as it occurs during this period?

It is reasonable to suppose that, as the fœtus in utero is wholly dependent upon the mother for whatever is necessary to its sustentation and growth, an additional demand is made upon

the system of the latter during the period of utero-gestation, and that this is effected through the medium of the absorbent vessels. When the digestive organs perform their functions well, and the diet is of the proper kind, the lacteals alone undergo any considerable alteration in their action, and the condition of the mother is preserved in its vigour from the beginning of the period to the end. But when, on the contrary, digestion is deranged, and the appetite for food impaired, or when the food is deficient in quantity, or of an improper quality, the body having been previously reduced by want, disease, and other causes, not only the lacteals, but the whole absorbent system, is brought into undue activity: the supplies under these circumstances being mainly furnished by the system at large. Extreme emaciation of the soft parts is an early result of this change, and softening of the bones supervenes, owing probably to an inordinate abstraction of their earthy ingredients by the absorbents, or to a suspension or derangement of the secreting functions by which the balance in their constituent principles is naturally maintained.

I think it highly probable, however, from what has been said by authors on this subject, and also from some unpublished facts and observations with which I am favoured by an eminent professional friend, and which I hope to have an opportunity of considering more fully, and of communicating hereafter, that the bones in malacosteon are not deficient in earthy matter merely, but that the whole osseous texture undergoes a complete change in its animal as well as in its earthy structure and proportions.

The immediate cause of softening of the bones, as it is met with in early life under the form of rickets, has been supposed to depend upon a deficiency of the phosphoric acid by which the earthy matter of bones is naturally held in a fixed state, and to the development of a free acid similar to the oxalic. This is said to be generated during digestion, is taken into the circulation, and enters into combination with the lime of the bones, which is carried off chiefly through the urinary organs. This opinion would appear to be borne out by the fact, that most of the secretions, and especially the urine, in those affected with rickets,

is found to contain oxalate of lime, sometimes acetate of lime, and a portion of phosphoric acid uncombined. This theory, which was first broached by M. Bonhomme, is ingenious, and may be correct so far as regards the disease in early life, although I have doubts about it: the same causes cannot be assigned for it, however, in the adult. Dr. Hall, in the Appendix to his translation of Baudeloque's memoirs on the Cæsarean operation, remarks that "we have no good reason to believe that the oxalic or the acetous acid ever prevails in the fluids of patients afflicted with malacosteon *adultorum*, much less that an acid of this kind acts upon their bones, dissolving the lime which enters into their composition, and rendering them softer and yielding. It has never been shewn by experiment, I believe, that the blood contains either these or any other acid uncombined." In early life, when the secreting functions are active in proportion to the demand of the body for those materials necessary for its increase, it is possible that the fluids may become readily changed in the manner above indicated; but in after life, when the solids have attained their full growth and consistency, and the secreting function is in a measure suspended, or much less active at least than formerly, it is not so likely that an acid state of the fluids ever exists to any important amount, or if it should, that it ever exerts any considerable effect upon the osseous textures. "In some very bad cases, however, (remarks the author above quoted), it is probable that the secretion of ossifying substances may be lessened, and the absorption of them increased at the same time: and, as has been intimated above, even the cartilaginous or membranous base of the bones has been sometimes removed. Perhaps in some cases of malacosteon, the lime, entering into the composition of the bones, undergoes a decomposition."

The predisposing causes of malacosteon consist in whatever has a tendency to undermine the general health by impairing the assimilative and nutritive functions, and disturbing the balance in the action of the absorbents: such are deficiency in the quantity of food, or taking such as is faulty in quality; want of proper air and exercise; peculiarity of occupation; exposure to an

impure atmosphere, &c. I shall remark on one of these topics only at present, and that as briefly as may be, as I intend to dwell upon each of them more at length hereafter.

Few diseases are more insidious in their incipient stages than malacosteon, and the fact of its occurring principally amongst the lowest orders of society excludes it from that attention which it would elicit in those whose means enable them to take advantage of whatever the best skill can avail them, and in whom the slightest deviation from a healthy condition would immediately excite alarm. It is true our public hospitals are numerous and well conducted, and the facilities of obtaining admission, as out-patients at least, sufficiently great; but the number of applicants for relief is so considerable as to render it next to impossible that a strict and satisfactory investigation can be made in every case. Moreover, the symptoms of malacosteon are so similar to those of another complaint of very common occurrence, and of comparatively slight importance, namely, chronic rheumatism, that the one may be confounded with the other by the most experienced practitioner. On this account it is that malacosteon has so frequently been overlooked in its commencement—at the period when medical treatment might have proved of most essential service in averting the disease; and, thus, in preserving the body from deformities which are not only inconvenient and unsightly, but on account of which the sufferers are often excluded from sharing in many of the comforts and pleasures of social life, which under other circumstances they might long have enjoyed. Whenever rheumatic pains of a kind similar to those which have been alluded to, are complained of, especially during the period of pregnancy, the circumstance ought always to excite alarm, and the case should never be passed over without undergoing the strictest investigation. But in hospital practice, should it even be ascertained that disease of the bones is commencing, or does already exist, it is out of the power of the physician or surgeon to furnish the patient with what he would almost invariably prescribe: the disease being very frequently induced, in the poorer people, by a deficiency of the ordinary necessities of life. The husband of

Mary Anne Jones was by occupation a spinner, his earnings about twelve or fifteen shillings per week; but owing probably to his being almost devoid, naturally perhaps, of the ordinary bodily power and mental energy, he was often without employment. His wife was weak and unhealthy, and, I fear, naturally an unthrifty person, contributing nothing to his income; and his family consisted of three daughters, too young to earn a penny. For the first four or five years after their marriage they were in tolerably comfortable circumstances for people in their rank of life, but afterwards became extremely poor and destitute. During the period of each pregnancy, from the first to the last, the wife experienced a peculiar longing for dry oatmeal and butter-milk, which, as they were easily obtained, she usually had without restriction. On these articles she almost entirely subsisted. I am not in possession of sufficient data to enable me to state how far the morbid changes manifested in this patient could be attributed to any specific effect produced by the oatmeal; I am inclined to think, however, that this article exerts a peculiar influence on the solids when long and extensively used; but how, and to what degree, I am not prepared to say. Several causes contributed, no doubt, to the production of softening of the bones in this case, amongst which, a meagre diet, and that consisting chiefly of oatmeal, formed, in my opinion, the principal. The same evils might possibly have resulted from the exclusive or excessive use of any other kind of food in itself equally innutritious. The reader is not to understand by these remarks that I regard oatmeal as an improper or unhealthy food; on the contrary, when taken in suitable quantity and properly cooked, it forms a very useful article of diet, especially for children and working people. One-half of the diet of farmers' servants, and of most country people, in some districts, consists of oatmeal in some form; and no class of people look better, or are in reality more healthy. But these farinaceous meals are alternated, once a day at least, with one of a more substantial kind, in which a portion of animal food is used, and often of malt liquor. This variety makes a considerable difference. To

be wholesome the diet ought to be constantly varied*. The long-continued use of any article, whatever proportion of nutritive matter it may contain, will, after a certain time, if unmixed with others, cease to afford that sustenance which was at first derived from it†. The blood would appear to become surcharged with one principle, the solids lose their constituent proportions, absorption and nutrition are interrupted, and indigestion and its consequent evils are the result.

These hints are thrown out for the consideration of the medical reader as suggestions merely, and not as facts upon which we may place unqualified reliance.

With regard to the treatment that was adopted in this case, little requires to be said after the detail that has

* *Hunc oportet varium habere vitæ genus. Nul-
lum cibi genus fugere, quo populus utatur.—
Celsus.*

† Some very interesting experiments bearing upon this subject are recorded by M. Magendie in his *Compendium of Physiology*, from which I shall take the liberty of making a few extracts.

"A dog, eating at discretion pure wheaten bread, drinking at pleasure common water, does not live above fifty days: he expires at that period with all the known marks of final decay."

"A rabbit or guinea pig, fed upon one single substance, as wheat, oats, barley, carrots, dies with all the appearances of inanition, ordinarily after the 15th day, and sometimes a good deal sooner. Fed with the same substances, simultaneously or successively, at short intervals, these animals both live and thrive."

"An ass, to which I had ordered to be given dry rice, and afterwards rice boiled in water, because he refused the former, only survived fifteen days."

"Some dogs fed exclusively upon cheese, and others upon hard eggs, lived a long time, but they were weak and meagre: they lost their hair, and their appearance announced an imperfect nutrition."

"One of the most remarkable facts which I ascertained was the following:—If an animal has lived during a certain time upon a substance which, taken alone, cannot nourish it, wheaten bread, for instance, during forty days, it will be in vain at that period to change the diet, and return to its ordinary regimen. The animal will devour with avidity the new meats presented to it; but it will continue to decay, and its death will nevertheless arrive at the period at which it would have happened, if the creature had continued its exclusive regimen without interruption."—*Compendium of Physiology*, 4th edition, translated by Dr. Milligan.

‡ The results of some experiments in which I am at present engaged have suggested what has been stated in the text; but as these are yet incomplete, and would be too long to introduce in this paper, I shall reserve them for an article which I am preparing for publication in a different form. In this I intend to dwell at greater length on some of the more important points in the present case, and shall be able to illustrate the subject with a few plates, which may render it more interesting as well as more useful.

already been given. In a subject so ill calculated by nature, and by previous bad health, for withstanding the shock of a capital operation, and possessing in so small a degree the necessary reparative power, the most unfavourable results only could be anticipated. Her diet was very carefully regulated, and was of the most nutritious kind; every article of food which she took during the whole period being supplied from the author's own table. The administration of opium in sedative doses, after the plan lately pursued in cases of extensive wounds of the large cavities, and first recommended, I believe, by Sir B. Brodie, was of great service. It seemed to tranquillize the system, and to preserve it from that excitement and febrile irritation, of which an enfeebled constitution is peculiarly susceptible, and which, in all probability, would have proved fatal at an early period, had it prevailed, in this case. I think it advisable to promote the healing of as much of the upper part of the wound, at an early period, as can possibly be effected: the lower fourth of it may be left open for some days, to allow free exit to the secretions from the wound in the uterus, and to any accumulation that may take place in the interior, although this occurrence, I think, is far less likely to be the case than was formerly supposed by the early authors on this subject. Adhesion between the parietal and visceral peritoneal surfaces took place early—indeed, it seemed to be the first effort of nature—and thus the general peritoneal cavity was completely excluded from the external wound, which was thus reduced to one comparatively simple in its character. This circumstance, I am convinced, from what I have seen in other instances, is the first reparative process in all wounds of this important cavity, and the knowledge of this fact may prove of considerable importance if strictly attended to. The difficulty of keeping the edges of the wound in apposition during the first eight or nine days was owing to a defect in the apparatus employed. The common adhesive plaster spread upon canvas, which was first used, is altogether unsuited to cases of this kind: it soon becomes softened, from the heat and moisture of the parts, and slips from its attachments. The plaster

which was found to answer the purpose in every respect, and which was adopted at the instance of Dr. Robinson on the ninth day after the operation, was one composed of the Empl. Labdani Co. and Empl. Saponis, in equal proportions, spread on strong canvas. It neither irritates the skin nor becomes softened by the warmth and moisture of the surface. Nor is either the circular or the tailed bandage admissible. The bandage preferable to all others is the labouring man's belt. It is firm and unyielding; and on account of its fastening by means of straps and buckles in front of the body, is conveniently loosened and reapplied without disturbing the patient.

Whether this case will be ranked by the profession generally among the successful ones, it is difficult to say. It was certainly so far favourable that, should a similar one come under my care again, with even considerably larger dimensions in the pelvic cavity than existed in this, I should prefer the Cæsarean operation far before that of mutilation and extraction of the child piecemeal. There is no doubt that the hip-joint disease was the immediate cause of dissolution; but, as this was a mere casual occurrence, not depending directly either upon the operation or its treatment, but upon the peculiar nature of the disorder under which she laboured, and of the causes of which enough has already been said, no importance can be assigned to it as an objection to the operation. My opinion is, that the Cæsarean section, as a means of delivery in cases of extreme difficulty, is decidedly preferable to that of embryulcia, if performed in proper time; for, by this latter means, the child must first of all be destroyed, and extracted by a very tedious process, piecemeal, and the mother often dies from exhaustion, or from inflammation in consequence; or, if she recover, it is generally after a long and tedious period of illness, and frequently with some defect consequent upon the operation, which is an annoyance to her through the remainder of her life.

In conclusion, it only remains to observe, that the objections commonly raised in this country against the more general adoption of the operation have been founded on the want of success

which has attended it in the majority of instances where it has been performed. For this failure, however, very adequate reasons can be assigned. The operation has been undertaken only after the patient has been in active labour during several days. Her powers have thus been allowed to become exhausted, and the soft parts so injured by pressure as to render it extremely doubtful if recovery would have succeeded, had delivery been effected under these circumstances, without any operation at all. On the continent it has been attended with the most favourable results, having been successfully repeated even a third time on the same patient. This is undoubtedly owing to the operation having been performed at an early period, soon after the commencement of labour, and while the patient retained her full vigour, and the soft parts remained uninjured. Dr. Hoëbeke has performed the operation sixteen times, in eleven of which the patients have perfectly recovered. This amount of success fully equals that which has usually attended any of the more important capital operations—such as lithotomy and amputation.

It may be as well to remark, that the much dreaded danger from inflammation of the peritoneum was not at all apparent in this instance; and considering the favourable progress that was made during the first three or four weeks, little doubt exists in my mind that, had not a complication of so serious a nature as inflammation of the most important joint of the body prevailed, the case would have arrived at a favourable termination. Indeed, looking at the whole of the circumstances, and bearing in mind the successful issue of Mr. Knowles's case, of Birmingham, in both which an early period was chosen for the procedure, before serious injury of the soft parts, or exhaustion had come on, I should entertain a very confident hope that the Cæsarean section will hereafter be adopted as a substitute, in extreme cases, for the not less dangerous, and far less humane or satisfactory one of embryulcia.

The child, who was christened Cæsarina, lived nearly eight months: she died from an attack of fever, succeeded by marasmus.

I am glad to avail myself of this

opportunity of tendering my thanks to my esteemed friends Dr. Robinson, Mr. Fawdington, Mr. Robertson, and Mr. Wilson, for their kind and valuable assistance on this anxious occasion.

Oxford Street, Manchester,
Aug. 23, 1841.

TREATMENT OF BURNS.

To the Editor of the Medical Gazette.

SIR,

CONSIDERING the vast number of accidents which daily occur by burning, it may not perhaps be unacceptable to the profession to have their attention directed to an application which I have been in the habit of using in such cases for some time past, with very decided advantage. The plan of treatment which I adopt, and here allude to, is nothing novel, and may, perhaps, be employed by many, if not known to most. It is, therefore, solely with the view of detailing what appears to me a most valuable form of application, in cases of burning, as well as of inviting its more general use (seeing that, in this neighbourhood at any rate, it is not generally employed), that I now trouble you with these remarks. The more common of the applications generally employed appears to me liable to many objections. The common sweet or train oil is a remedy frequently resorted to, in the hurry and bustle of the moment attending these accidents, by friends or neighbours of the patient, who may happen to be present at the time. This application, too, is not unfrequently sanctioned by the practitioner. Granting the occasional *ultimate* benefit of this application, and waving its filthy nature, the strong objection to its use is this—that it seldom or never affords instant relief to the suffering patient. The same, I think, holds true in respect of the linimentum aquæ calcis, or Carron oil, so very frequently employed. The common raw cotton, or wadding, has, of late years, and perhaps deservedly too, held especial favour as a local application. It must, however, be admitted, that the patient derives no *immediate* relief to his sufferings upon its first application; but, on the contrary, not unfrequently complains of augmented

pain for some time afterwards. This may very easily be accounted for, both by its acting as an irritating body, by its direct contact with the red or raw surface, as well as by increasing for a time the temperature of the part to which it may be applied. Besides, it in a few days becomes saturated with discharge from the secreting surface, and consequently not only proves annoying to the patient, but also a means of keeping up discharge in more trivial cases. These objections are in a great measure got rid of by using *common soap*, which, besides its great value as a local application, commands the additional advantage of always being at hand in cases of emergency. The mode in which I am in the habit of employing it is this:—A common shaving box may always be procured, from which a good lather may, in the course of a minute or two, be easily obtained. This lather is then gently laid over the burnt surface by means of a shaving brush, and repeated so soon as the first coat begins to dry, or the pain return. This practice ought to be repeated occasionally during the first day, or until such time as the pain is relieved. The benefit accruing to the patient is *immediate*, and the result of the practice highly satisfactory; for in more superficial burns, if early applied, vesication is prevented, and, in the course of a few days, desquamation of the cuticle follows, without leaving a raw surface. Of course, this, as a remedial measure, is most applicable to superficial burns; but even in such cases as involve destruction of the more deep tissues, it is not used without advantage, in so far as the personal comfort of the patient is concerned. In such cases, after the lapse of a few days, the crust formed by the soap is easily removed, so as to permit the employment of other remedies, if necessary. I am not prepared to say whether the benefit and instantaneous relief, following the application of the lather, are to be ascribed to its chemical composition, or simply to the fact of its affording some degree of protection from atmospheric agency, or both. Were it necessary, I could easily adduce many cases in illustration of its extreme value as a local application; but as this would be needlessly encroaching upon the space of your journal, I leave it to those of the profession who have not yet tested its efficacy, to substantiate,

by the result of their own experience, the truth of the preceding statements.

I remain, sir,

Your very obedient servant,

THOS. WILLIAMSON, M.D. Edin.

One of the Physicians to the Leith Dispensary,
Edinburgh and Leith Humane Society, Casualty Hospital, &c. &c.

Leith, Sept. 7, 1841.

M. LAFONTAINE'S EXHIBITIONS.

To the Editor of the Medical Gazette.

SIR,

You will much oblige me, and will, I think, at the same time be doing a service to the public, by inserting in your valuable journal, the following short *exposé* of the mesmeric exhibitions of M. Lafontaine.

It having been through the medium of the daily press that the absurdities carried on at Hanover Square Rooms, and in the immediate neighbourhood, have attracted so much attention, the accompanying communication was forwarded to the editors of most of the morning journals at the time it was written, but by all was its insertion refused. Trusting that the subject had met with the fate it deserved, I had determined on allowing it to rest; but the renewal of the performances of Dr. Elliotson and M. Lafontaine so loudly calls for exposure, that I have forwarded you the letter which I formerly addressed to the daily papers, and shall be much obliged by its insertion in an early number of your journal.—I remain, sir,

Your obedient servant,

JAMES BLAKE, M.R.C.S.

7, Cork Street, Burlington Gardens,
Sept. 7, 1841.

From the attention I had given to the subject of animal magnetism, when it was formerly before the public in 1838, I went to the exhibition of M. Lafontaine on Monday last, with the expectation of meeting with a large share of imposture, set off by a slight admixture of truth: nor was I disappointed. The first subject produced was a girl, who was soon sent to sleep, and who in this state was said to be quite insensible to pain. This was tested by M. Lafontaine pretending to run pins into the hands: from the violent manner in which this appeared

to be done, M. Lafontaine apparently using great force in thrusting them into the hand, the impression produced on the audience was, that the pins penetrated deeply into the flesh; as, however, no blood followed the wounds which M. Lafontaine seemed to be inflicting, I had my suspicions that he was not so confident of the insensibility of his patient as he pretended to be. On examining the girl's hand after she had been awoke, these suspicions were fully confirmed, as not the slightest wound was to be found on the hand. Prompted undoubtedly by an *excess of humanity*, M. Lafontaine had avoided, by the exercise of a well-acted legerdemain, inflicting the slightest wound, even when he appeared to be thrusting pins into the flesh with considerable violence.

I again witnessed the exercise of the same legerdemain, when M. Lafontaine pretended to be thrusting pins into the boy's forehead; for, on this occasion, although the greatest violence appeared to be used, not even the cuticle was penetrated: as I was standing by the side of the boy at the time, I was perfectly enabled to see the trick. There can be no doubt, however, that the pins were really introduced into the boy's hands, for here blood followed, as it must do on every occasion where pins are violently thrust into the flesh. The pain caused by the wounds was certainly borne most stoically, as were also the punctures made in the fleshy part of the thigh, a species of infliction to which the victim had not been accustomed: but far from proving that the boy was insensible, the large drops of perspiration that appeared on his forehead plainly convinced me that he was suffering acutely. The faintness of the boy, on M. Lafontaine pretending to *recall* him to a state of sensibility, served to confirm my opinion as to his sufferings; and the immediate discontinuance of the performances, evidently proved that the boy had had enough of it. The explanation offered by one of the mesmeric agents of the *Times*, that this faintness was owing to the loss of a tea-spoonful of blood, is quite absurd; and the assertion of a lancet having been thrust into the upper part of the calf of the leg, I believe to be quite unfounded; for I was standing by the boy during the time that the wounds

were being inflicted on his leg, and I can assert that I saw no such instrument used. The explanation of this accusation will probably be found in the excuse it will afford, in case of a repetition of these shows, for not allowing any person to interfere with the tricks.

Were further proofs required of the imposition practised at these exhibitions, they are furnished by the following facts, which took place in connection with the application of the ammonia to the nostrils. From the manner in which this was applied, I saw that it was only used as another means for duping the audience; for the bottle was held under the nostrils only for a few seconds; and it is a well-known fact that a person can hold the breath for more than a minute, so that not the slightest portion of ammonia shall enter the nostrils. Feeling convinced that this test, if properly applied, would furnish a strong proof of the absence of sensation, I requested M. Lafontaine to allow me to apply it myself. This was apparently assented to with the greatest readiness, and the boy was sent to sleep; but, unfortunately, at the very moment I was about to put the ammonia under the nostrils, some convulsive movements occurred, on which M. Lafontaine declared that it was necessary that the boy should be immediately awakened. Trusting that the occurrence of these convulsions was merely incidental, I again requested M. Lafontaine to send the boy to sleep. This was accordingly done; but at the moment I was about to apply the dreaded ammonia test, the convulsive movements again came on, and the boy was obliged to be awakened. Being determined to persevere, however, I had him sent to sleep a third time; but, on the convulsive farce again shewing itself, at the moment I was about to apply the ammonia to the nostrils, I relinquished the attempt as hopeless.

It would be an insult to the common sense of your readers, to offer any comments on the above facts, proving, as they do, how gross is the imposition that is attempted to be practised on the public, by the professors of this mysterious science—a science which promises to overthrow the present foundation of all knowledge—to reconstruct from the ruins a new and far more

beautiful temple to science; and — to abstract five shillings from the pockets of those who will allow themselves to be duped.

August 5, 1841.

MEDICAL GAZETTE.

Friday, September 17, 1841.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”
CICERO.

ON THE STUDY OF SPECIAL PARTS OF MEDICINE.

IN speaking a short time since on the study of insanity, we urged the propriety of adopting some plan by which all who are likely to be called upon to take charge of many insane persons, might, by previous education, render themselves competent for immediately undertaking the task. There can be no doubt, that in a part of medical science which is so necessarily made a subject of special practice, there should be opportunities for special study; without it we cannot conceive that there can be either security for patients, or a prospect of the advancement of the science. But it is of much interest, in relation to the best modes of medical education that can be established, to determine whether special courses of study in this or any other branch of medical science should be required of those who are to become, in the widest sense of the term, general practitioners; that is, who, in the ordinary course of their practice, will have to treat either medical or surgical diseases of all kinds.

It may be, and doubtless is, quite true that no man can be a good surgeon who is ignorant of that which is more particularly called medicine; or a good physician, who does not know any thing of surgery; but it is not less true, that the natural course of things which, in a

case of this kind, is but another expression for necessity, has divided the common science of medicine into many more parts than these two; and that just in proportion as a person gives his attention, not merely to one of the main divisions, medicine, surgery, or midwifery, but to one of the numerous subdivisions of either of the three, in just the same proportion is his chance of obtaining renown and a property. And although we should rarely be inclined to take the voice of the public in medical matters, as the expression of a sound judgment, yet in these cases we cannot reasonably deny that public opinion is right, and that, *cæteris paribus*, the more a man, having received a good education in the generalities of medicine, gives his chief or sole attention to one department of it, however small, the more is the confidence which he in that department deserves. In the propounding of which opinion we should be open to the accusation of uttering an obvious truism, if something very like the contrary were not held by a number of thinking men in our profession.

But, if what we hold to be true, are there any specialities in medicine which ought to be particularly studied by all? The question is one only of expediency, and not to be answered without a fair allowance being made for the time and expense which all, or the average of students, can bestow on their education; and giving due attention to these, we cannot long hesitate to answer the question in the negative. For, in the first place, if that which is obvious be granted, namely, that *all* the specialities of medicine cannot be studied, it would be a matter of extreme difficulty to determine which of them all most deserves attention. Is the study of the surgery of the eye, the ear, the rectum, or the urethra, the least dispensable? We cannot tell of our own knowledge,

though we have some special acquaintance with them all: and still less are we able to balance between the encomia of their respective professors, each vaunting loudly his own favourite. Or is the medicine of the chest, or the mind, or the skin, to be chosen for special study? Still we cannot tell, though we have the like knowledge, and hear the same praises of each. But in a case like this, however unwise the conclusion may be in others, the difficulty of making choice can be overcome only by a general exclusion; and so, by their being absolutely impracticable, our negative of special studies is confirmed.

Again, in determining on the propriety of enjoining the special study of certain departments, the amount of the need of them can be regarded as equivalent only to the surplus of advantage which patients receive from one who has so studied them, above that which they receive from one who is acquainted with them only through his knowledge of general principles. This greater advantage is doubtless, in every case, worth consideration, and in some, of great moment; but in the large majority of cases it is not much, in a practical view. If the number of patients injured or lost, whose health or lives would have been saved had they been under special practitioners, could be ascertained, it would probably be very small; or if the number of days of illness endured under the hands of a universal, or of a special, practitioner, could be compared, the real difference would, we doubt not, seem very little, and in no degree commensurate with the labour which the latter had devoted to his chief object. So little is, in the main result, the power of medicine! so little the difference between the degrees of good done by the moderately judicious treatment, and by that which, as far as our knowledge yet goes, is deemed the

best possible! And thus again we are forced to the conclusion, that though it would be very admirable if all could have as much knowledge of every department of medicine, as the best in each now have of their own severally, yet the public advantage would not be so great as to warrant the risking of any present manifestly good arrangement for the chance of obtaining it; in other words, that it is not worth while to attempt to enforce the general study of any or of all these specialities of medical science.

But although, looking without partiality upon them all, we estimate the practical advantages of each of these special studies at a lower rate than its professors would, yet we should be far from thinking the general enforcement of their study unnecessary, if we did not believe that a great part of these advantages were already constantly attained in the ordinary course of medical education. Of the whole mass of medical students, probably not one half content themselves with that amount of education which is imperatively demanded to fit them for examination: the remainder voluntarily devote a year or two more, subsequently to obtaining their diplomas, to these very studies. Indeed it is too often true that the real and profitable part of medical study is not commenced till the examinations—the professed tests of competency for practice—are passed; for then it is that many students first begin to study diligently by the bed-side, and to acquire both the general and the special practical knowledge of their profession. It is at this time also, that all who are not obliged at once to commence practice, undertake the particular study of that to which their taste, or their prospect of profit, most inclines them; and when we call to mind the great number of those who thus occupy themselves, we feel sure, both that the study of each

department of medicine is not neglected, and that there is no need whatever of making it compulsory. For the general standard of medical education must be no higher than the lowest can attain, compatibly with the public safety; and all studies beyond those which are necessary to obtain it must be left to the conscience, the taste, or the ambition, of those who choose to engage in them.

There is, besides, a manifest advantage in these studies being postponed to a late period of medical education; to a period, indeed, at which it is presumed that the student is *generally* qualified to practise. For just as a knowledge of medicine in general is essential, not merely to the safe practice of any branch of it, but much more to such a prosecution as may lead to the improvement of that branch, so is a certain amount of knowledge necessary to a profitable study of the minuter parts of each division of the whole science. Paradoxical as it may sound, no man can study well any part of medicine till he has learnt a great measure of the whole. A knowledge of general principles is the only safe guide in the study of the finer details. A good oculist, or a good aurist, ought to be something more—not much less, than a good surgeon; nor are any but surgeons fit to be students of these, or any other divisions of their science.

We should not have thought it necessary to discuss this subject, had not our attention been of late several times attracted by paragraphs expressing regret that this or that department of practice should not be generally taught in our schools. For example, in the work by Dr. Jacobi, already noticed, Mr. Tuke says, in a note, that it is much to be regretted that lectures on insanity are not regularly delivered in the English medical schools, as they are at Bonne, and other German univer-

sities; and we could easily refer to passages in other works, equally urging the teaching of ophthalmic or aural surgery, and many other special subjects. We regard it, however, as in no degree a matter of regret that these things are not taught otherwise than they are now, or than they might be, without interfering with the general medical education. Rather, we believe that already too many subjects are required to be studied; and we should deeply regret to find them still further increased. There is, probably, nothing in which the English curriculum of education more excels, in its practical advantages, those of all other countries, than in its present simplicity; for although we think it even now too complex for the average of students, it certainly, in comparison with others, stands pre-eminent.

There is but one advantage attendant upon the more general teaching of all the smaller departments of medicine, which is customary in France and Germany; and that has relation more to the teachers than to the taught. This is, that it serves to the former for a school of lecturing; for, on this plan, very few rise to be lecturers on the more extended and important subjects, who have not already served an apprenticeship in some other special courses. The consequence is, that with the additional advantages of the *concours* (to which, for this purpose, nothing but praise can be given), it is as rare to hear a lecture ill delivered in France, as it is to hear one well delivered in England. Whether he knows any thing of his subject or not, it matters little to the French lecturer, and not much to the German; he can always talk about it fluently, and with perfect ease and vivacity, or solemnity (as the need of the question discussed may demand), for his full hour; and if he do not instruct his audience, he does

not put them to sleep. This is unquestionably a great advantage: it involves more important considerations than the comfort of the students; the amount of knowledge that they receive is in great measure dependent on the mode in which it is conveyed. If, therefore, any special studies are to be enforced, we know of none that would be more generally profitable than that of lecturing, through a practical course of which each aspirant to the honour of instructing should be made to pass. We want a *normal school* for medical teachers.

SUBSTANCE
OF A
CLINICAL LECTURE,

Given at St. George's Hospital, June 8th,

BY MR. CÆSAR HAWKINS.

[Concluded from page 956.]

2. *Injury of the head. Inflammation or effects of debility.*
3. *Scalp wounds. Secondary abscesses. Fracture of the basis of the cranium.*

II. No wonder, then, that we should be offered some great similarity of symptoms from apparently opposite causes; because, in fact, these opposite causes may in effect produce the same state of circulation in the brain, in one of the several circumstances which we have been considering. No wonder, moreover, that we should often be at a loss in our diagnosis of these causes, and actually puzzled whether we should bleed or stimulate our patient—whether we should increase or lessen the vigour of his circulation. To illustrate this point let me next draw your attention to another case now in the hospital; it is that of Jane Looker, æt. 32, admitted May 24th, into Wellington Ward, with these symptoms:—Complains of great pain in her head, equally diffused over the whole of it; pulse 90, regular, and rather weak; tongue clean and moist; bowels open; no anxiety of countenance, or frowning; not much appetite. She states that about a month ago she received a blow on the upper part of the head, which stunned her for about ten minutes, ever since which time she has had great pain in the head. She was cupped a week since, which relieved her for a time, but owing to working hard and drinking some porter, the

headache returned, and is now as bad as ever. She is at present suckling a child eight months old, and has been living poorly of late, and was getting thin and out of health before the blow.

Here, then, is a case that might at first be regarded as an instance of inflammation after concussion, from the acuteness of the pain the woman complained of. You find that this view was taken of the case before her admission, and she was actually relieved by the cupping which was ordered. So it is, however, in many cases of weak and irregular circulation, and the temporary relief is succeeded by an aggravation of the pain. You find again that she attributes her return of pain to the stimulus of porter; but neither does this prove that the stimulus was injurious, since she was at the same time imprudent enough to return to hard work, which would naturally make a weak person suffer.

The whole state of the patient, however, left me little room to doubt that the pain was not likely to be inflammatory, and particularly the colour of the countenance, and the absence of anxiety and frowning, which inflammation almost always produces; the absence of sharpness and force of the pulse, although it was somewhat quick and jerking, and then the discovery of the fact of her having suckled up to the present time, and that in a condition of great privation. This long-continued suckling is, without any injury, a fertile source of derangement of the health generally, and of amaurosis, pain in the head, or mania in connexion with the brain.

I let her lie quiet, then, with some cold lotion on the forehead, till the day after her admission, giving her only some more nourishment. The pulse the next day, the 25th, was only 80, and soft; and the pain was a little better, but still much complained of. I therefore ordered her some ammonia draughts, and a small blister to the nape of the neck. On the 29th our notes say the pain was rather better.

On June 1st I gave her some bark and aromatic confections, the countenance being more cheerful and a little more florid, but the pulse still weak, and gave her also a little porter.

On the 5th it seemed as if this was rather too much, the pain in the head being still complained of as much as before; and I gave her some Sp. Ammoniae Foetidus and Camphor Mixture instead of the bark, letting her diet remain as good as it had been; and now you may see her convalescent.

There are many such cases as this which will come under your notice after injuries of the head, in which, if you are in any doubt, you should do little, but rather watch your patient; or if your opinion is a little inclined to either side, you should proceed very cau-

tiously, whether with the plan of depletion or of stimulation, lest you should find yourselves in error, or lest a change of plan should proceed a little too far; and there are many cases in which you will require even a combination of both principles of action. A man, for instance, fell from a ladder, and was bled after his admission for the symptoms of concussion, the house surgeon not being aware that he had also been bled just before his admission and very soon after the fall: he was for many days in a state of complete oppression of brain, with a slow and laboured pulse, and nearly total insensibility, and from want of nervous power there was the same torpor of bowels which is so much more common in diseases than in injuries of the brain. Now the state of this man was much more owing to large loss of blood than to the fall, and his tongue was thickly coated with a white cream that indicated a very disturbed state of circulation in the brain, but is not usual as a consequence of injury; but although this man required a good deal of light nourishment, and some diffusible stimulants to hasten the passage of the blood through the brain, he was also much benefited by blisters to the neck, and by small doses of calomel and opium.

In some of these cases you will find even a cupping glass, or a few leeches to the forehead or temples, when there is intense pain, not at all incompatible with moderate stimulation of the general system; local congestion or inflammation being actually the consequence of feebleness of the circulation, and requiring the same local remedies, and the same effects on the capillaries of the part, which would be requisite in another case in which the general circulation was in an opposite condition; and for this purpose a blister, which I gave to our present patient, is very efficacious.

III. In the next place you have had several scalp wounds in my last accident week under your notice, all of which, however, seem to have done well, except one case, in which the patient died yesterday; and you will presently have an opportunity of examining into the causes of his death.

This was a boy, twelve years of age, named Robert Sims, who was admitted May 17th into Oxford Ward, with a small scalp wound on the posterior part of the head, and a slight graze on the right side of the forehead. The bone was slightly exposed under the posterior wound. He was kicked on the back of the head by a horse, the force of the blow knocking the forehead against a wall. He was insensible for a short time after the injury, and vomited previous to admission. Countenance pale; pulse weak.

18th, 10 A.M.—Has vomited during the night, and also this morning. Reaction

having taken place, he has now much pain in the head; skin hot; pulse hard and full; tongue white.

V. S. ad 3vj .

R Hydr. Chloridi, gr. iij.; P. Antim. gr. iv. statim. H. Sennæ post hor. iv.

1 P.M.—Pain in head much relieved. Pulse more soft, but still sharp. Blood not buffed.

19th.—Tongue clean; very little pain in head; pulse quick; wound rather sloughy.

Poultice.

20th.—Tongue clean; pulse quiet; matter from the wound rather foul.

Broth diet; green dressing.

24th—Pulse quiet; no pain in head; tongue foul.

R Pil. Hydr. gr. iv. h.s.s. Haust Rhei cras mane.

28th.—Some pain in the head; skin hot; pulse quiet.

R Cal. gr. iv. hss. H. Rhei mane. H. Salinus.

31st.—Having been somewhat better, he last night was found to have had slight shivering, and to-day his wound is foul, and the scalp somewhat œdematous around it. His mouth is observed to be drawn to the left side by paralysis of the right portio dura. Pulse 104, small and weak, as it had been since the first inflammation on the 18th; tongue coated; countenance pale. An incision was made down to the bone, which was found exposed to some little extent.

Beef-tea.

R Hydr. Chloridi, gr. iss.; Opii, gr. $\frac{1}{8}$ 6tis horis. Omit. Haustus.

June 1st.—Pulse 130, weak; skin very hot; some anxiety of countenance; has been wandering in the night. Some inflammation around the wound; complains of pain in the left knee.

Pergat.

2d.—Delirious during the night. Wound in head clean, without œdema. Complains of obscure pain about the thigh and hip. Skin hot; pulse as quick, but weaker.

3d.—Tenderness and pain in groin continues; less in the knee.

4th.—Countenance yellow and anxious; much enlargement in the upper part of the thigh at the anterior part, but there is not so much pain as there was. Tongue dry, and rather brown; pulse quick and weak.

5th.—Countenance more yellow; has had some shivering; swelling in groin somewhat increased; tongue brown.

He gradually sunk, and died yesterday morning, June 7th.

Here, then, is a case of apparently trifling

scalp wound, with symptoms of pure concussion, which yielded easily to common treatment on the day after the injury; after which the boy continued quite well for about twelve days, when a fresh set of symptoms commenced, which have ended fatally in about a week more; and it is to these symptoms that I will direct your attention. The symptoms were those of low inflammation somewhere, producing irritative fever, with delirium and rigors, indicative of a debilitated state of nervous system: where, then, was the chief cause of these symptoms?

In the first instance they might have depended on inflammation of the wound itself, which became foul, and the scalp swelled; but the incision relieved this state of the part, but only very partially alleviated the general condition, and therefore some other situation was to be looked to. Did the symptoms depend on low inflammation of the membranes of the brain? From the increased inflammation of the external parts it was of course not at all improbable that this might spread through the bone to the dura mater on its inner surface, and so to the inner membranes and substance of the brain, which extension of inflammation is so common in scalp wounds, and so often fatal. The only symptom directly referrible to the head was paralysis of the portio dura, but this is not a frequent result of inflammation of the brain, and by itself would be of little importance; and, from enquiry, there seemed no doubt that he had had this dragging of the mouth for a long time before the accident, and it may have arisen from some local cause. Then, again, the boy was very drowsy and sleepy, and he was sometimes delirious when left alone or quiet during the night: these symptoms are very common in diffuse inflammation of the membranes of the brain, but not at all conclusive; for you saw the boy in the opposite bed, with abscesses of the leg and foot, with just as much sleepiness, and with some delirium also, although his head was perfectly free from mischief. We cannot therefore be certain upon this point, and we only know that there was fever, in which some of the functions of the bones participated. Next, we observed that he complained of very severe pain about the knee-joint: was an abscess taking place there, secondary to the suppuration of the scalp? Motion caused much pain, but there was no swelling, and little tenderness, and the next day the thigh and groin were equally complained of and the knee got better, and from the swelling of the thigh it was equally clear that suppuration had taken place there; but whether it is in the vein or in the hip-joint, or diffused among the cellular tissue between the muscles, is not very clear; nor, indeed, whether there is not matter in the joint and external to it, and possibly in the knee-joint likewise,

as these abscesses often give little local sign. But may there not be suppuration elsewhere? It seems to me not at all improbable that there may some in the liver, as he became quite jaundiced for two or three days before his death, and had a little tenderness over this part. Finally, though we have no direct evidence of suppuration in the brain, and on the whole I think there is not, yet there may be some there also, in addition to what we know is about the hip, and what we suspect may be elsewhere.

The symptoms, then, with which this boy has died are those of suppuration of a low kind, and the exact seat of this we shall presently see: but why is it that these secondary abscesses are so common after scalp wounds of apparently very trivial importance? Their immediate cause seems to be the absorption of a poison generated in the secretions of the injured part, or formed even without injury, as in erysipelas; this poisonous material being apparently an unhealthy purulent lymph rather than common pus. I do not mean that there is (as some have imagined) an absorption of pus from one part, and a deposit of the same pus elsewhere, though it is not even impossible that some pus may sometimes actually pass through the kidney; but I mean only that the poison getting into the blood induces such a state of this fluid, that the capillaries of various parts become disposed to suppurative inflammation, in the same manner as a few drops of pus containing the syphilitic poison make the small vessels of various parts and different textures put on a morbid action, which may pervade almost the whole body.

Some deny this absorption, and undoubtedly it is not a necessary circumstance in theory; we have something analogous to it, for instance, generated in typhus fever, where what are called critical abscesses are formed in various parts of the body in consequence of some general condition of the blood produced by the fever; or again it may be analogous to the diseased condition of the system in scrofula, where a number of local abscesses are formed; or analogous to the state of system in malignant disease, where a common cause is in operation on the capillaries of many parts at once. It is very possible, therefore, that the irritative fever attending the injury of a scalp wound may produce secondary abscesses, without direct passage of any secretion into the blood; but I think the balance of evidence is immensely in favour of such absorption, and in many cases you may actually see the purulent liquid in its transit.

There are two ways by which the pus may get into the blood; one is, by the absorbent vessels, and I have seen in a case of diffuse inflammation of the uterus, after premature

parturition, an immense number of the absorbents of the uterus and adjacent parts quite filled with pus of this unhealthy character quite up to the receptaculum chyli. This mode of transit, however, does not seem to be nearly so frequent as a second mode, namely the passage of pus through the veins. Some persons believe that there is always inflammation of the veins before these secondary abscesses form, but I am not certain that this is the case, though generally some veins may be found inflamed in connection with a wound from which secondary abscesses have arisen. Sometimes the great veins are filled with lymph and coagulum mixed with purulent matter, but this is not often the case in comparison with the numerous small ones immediately around the wound, in more or less of which some inflammation may almost always be found by careful examination. Without much inflammation of their inner membrane, however, the veins may yet form a channel of communication with the blood; some of them being opened by the wound, or by incision, or by ulceration, or possibly allowing a poisonous influence even to transude through their coats.

You may ask, perhaps, why such an event does not happen when the matter of an abscess is absorbed, so that the pus must get into the blood; the answer, however, would be, that it is not pus, properly so called, but unhealthy purulent lymph, which produces this poisonous effect upon the blood. The reason of the unhealthiness of the secretion would appear to be a peculiar state of the constitution in those who receive the injury, so that while it is very seldom that you meet with secondary abscesses in one so young as this boy, they are very common in the miserable state of so many of our patients, who have ruined their constitutions by intemperance. Then, again, the circumstance is undoubtedly much under the influence of atmospheric causes, of the same kind exactly as those which occasion erysipelas and inflammation of cellular tissue and serous membranes; so that at one time almost every case is in danger of dying of one of these affections, while you will, at another time, see several months elapse without witnessing a single case.

These secondary abscesses are peculiarly fatal—perhaps I should say the state of irritative fever which accompanies them, or rather precedes them for a few days; so much so, in fact, that you will seldom see your patient recover, when once the rigors and perspiration which indicate their formation have been observed. Still I have known cases recover even after abscesses have actually formed; and occasionally when even a vital organ, such as the liver or lungs, is attacked by the inflammation which ends in these abscesses, the patient may eventually

recover: I have more than once seen much tenderness of the liver and complete jaundice recovered from, though certainly it is a rare occurrence. I have a lady under my care at the present time, on whom I operated for hernia about five weeks ago, succeeded by some sloughing and foul ulceration of the omentum and other parts covering it, who has had very copious foul suppuration suddenly bursting from one lung, to the amount of nearly a pint in the day, who is nevertheless recovering from it; the abscess having, no doubt, I think, been of this description. Commonly, however, they are much too numerous to be thus got rid of.

It only remains for me to speak of the treatment applicable to these local secondary inflammations, which, from what I have just said of their fatality, need not detain us long. One thing appears to be quite established, namely that the inflammation is of a low character, which does not bear general depletion, though local bleeding by cupping or by leeches may probably be of service: certain it is, I think, that a patient will die much earlier who is weakened by antiphlogistic means, and that a moderate system of support by nourishment and medicine seems to offer a much better chance to the patient of struggling through the irritative fever of these abscesses, or of the poison which causes them; the stimulus you give being of course proportioned to the degree of debility and prostration present in each case. In this boy no depletion could possibly have been thought of, and the depressed state of circulation in the brain, shown by the sleepiness and low delirium, and rigors, required moderate stimulus, and that equally whether the membranes were inflamed or not. Besides the common rules of treatment applicable to the symptoms present, I am inclined to think that much good is done by small doses of calomel and opium, which may act on the capillaries in such a way as to prevent suppuration; and this is almost the only thing that I much trust to. Possibly, too, another medicine may be of service which also seems to influence the capillaries, as in iritis and some cases of peritonitis, viz. turpentine; I have not tried it, however, often enough to speak with the same confidence that I can of the other. Besides these means I have seen much good done by the application of blisters to the part inflamed; to the nape of the neck in a case of scalp wound, or over the lung or liver when those organs are the seat of the disease.

Post-mortem examination after the lecture, thirty hours after death, copied from the case-book.—On removing the scalp the pericranium was found considerably separated from the bone opposite the part where the posterior wound had been. The roof of the cranium being taken away a small quantity

of purulent lymph was perceived on the dura mater, just under the superior angle of the occipital bone, and also some in the groove for the superior longitudinal sinus. On raising the dura mater some pus and yellow turbid serum were seen at the posterior part of the left hemisphere of the cerebrum, and some thick viscid pus on the corresponding part on the right side; this also extended into the fissure between the two hemispheres. There was a fissure in the occipital bone, which extended from the protuberance down to the foramen magnum, and was much more distinct on the inner than on the outer table. Some coagulated lymph was adherent to the inside of the longitudinal sinus, and two or three deposits of purulent matter were seen in the substance of the dura mater.

In the femoral vein of the left side, near the hip, was a small quantity of coagulated lymph adherent to the inner membrane. The hip-joint was full of pus, while the cartilages were apparently healthy. The periosteum of the neck of the femur, and for some way down the shaft of the bone, was separated from the bone by a little fluid. There was suppuration in both sterno-clavicular articulations, but none in the knee-joints. The viscera appeared to be all healthy.

MEMOIR OF THE CASE OF A GENTLEMAN BORN BLIND,

And successfully operated upon in the 18th year of his age, with Physiological Observations and Experiments.

BY J. C. AUGUST. FRANZ, M.D., Leipzig,
M.R.C.S., &c.

[Concluded from page 959.]

ON opening the eye for the first time on the third day after the operation, I asked the patient what he could see; he answered that he saw an extensive field of light, in which every thing appeared dull, confused, and in motion. He could not distinguish objects. The pain produced by the light forced him to close the eye immediately. Two days afterwards, the eye, which had been kept closed by means of court-plaster, was again opened. He now described what he saw as a number of opaque watery spheres, which moved with the movements of the eye, but, when the eye was at rest, remained stationary, and then partially covered each other. Two days after this the eye was again opened; the same phenomena were again observed, but the spheres were less opaque and somewhat transparent; their movements more steady; they appeared to cover each other more than before. He was now for the first

time capable, as he said, to look through the spheres, and to perceive a difference, but merely a difference, in the surrounding objects. When he directed his eye steadily towards an object, the visual impression produced by the object was painful and very imperfect, and no clear visual perception of it took place, because the eye, on account of the intolerance of light, could not be kept open long enough for the formation of the idea as derived from visual sensation. The appearance of spheres diminished daily; they became smaller, clearer, and more pellucid, allowed objects to be seen more distinctly, and disappeared entirely after two weeks. The *muscæ volitantes*, which had the form of black, immoveable, and horizontal stripes, appeared, every time the eye was opened, in a direction upwards and inwards. When the eye was closed, he observed, especially in the evening, in an outward and upward direction, an appearance of dark blue, violet, and red colours; these colours became gradually less intense, were shaded into bright orange, yellow, and green, which latter colours alone eventually remained, and in the course of five weeks disappeared entirely.

As soon as the intolerance of light had so far abated that the patient could regard an object without pain and for a sufficient time to gain an idea of it, the following experiments were made in the presence of Dr. Swaine. The first experiments were of that class in which the idea of a visible object is derived merely from pure visual sensation; the succeeding, of that kind in which the idea, in ordinary cases, depends upon the sense of sight combined with the sense of touch, and is gained by reflecting on the impressions made on the organs of both senses. It was necessary to perform these experiments on different days, as otherwise they would have distressed the eye too much.

1st Experiment.—Silk ribands of different colours, fastened on a black ground, were employed to show, first the primitive, and then the complementary colours. The patient recognized the different colours, with the exception of yellow and green, which he frequently confounded, but could distinguish when both were exhibited at the same time. He could point out each colour correctly when a variety was shown him at the same time. Grey pleased him best, because this colour he said produced an agreeable and grateful sensation; the effect of red, orange, and yellow was painful, but not disagreeable; that of violet and brown not painful, but very disagreeable; the latter he called ugly. Black produced subjective colours,* and white occasioned the recurrence of *muscæ volitantes* in a most vehement degree.

2nd Experiment.—The patient sat with

his back to the light, and kept his eye closed. A sheet of paper, on which two strong black lines had been drawn, the one horizontal, the other vertical, was placed before him, at the distance of about three feet. He was now allowed to open the eye, and, after attentive examination, he called the lines by their right denominations. When I asked him to point out with his finger the horizontal line, he moved his hand slowly, as if feeling, and pointed to the vertical, but after a short time, observing his error, he corrected himself. The outline in black of a square, six inches in diameter, within which a circle had been drawn, and within the latter a triangle, was, after careful examination, recognized and correctly described by him. When he was asked to point out either of the figures, he never moved his hand directly and decidedly, but always as if feeling, and with the greatest caution; he pointed them out, however, correctly. A line consisting of angles, or in other words, a zigzag, and a spiral line, both drawn on a sheet of paper, he observed to be different, but could not describe them otherwise than by imitating their forms with his finger in the air. He said he had no idea of these figures.

3rd Experiment.—The windows of the room were darkened, with the exception of one, towards which the patient, closing his eye, turned his back. At the distance of three feet, and on a level with the eye, a solid *cube* and a *sphere*, each of four inches diameter, were placed before him. Allowing him to move the head in a lateral direction no further than was necessary to compensate the point of view of the right amaurotic eye, I now let him open his eye, and requested him to state decidedly what he observed. After attentively examining these bodies, he said he saw a *quadrangular* and a *circular* figure, and after some consideration he pronounced the one a *square* and the other a *disc*. His eye being then closed, the cube was taken away, and a disc of equal size substituted and placed next to the sphere. On again opening his eye, he observed no difference in these objects, but regarded them both as discs. The solid cube was now placed in a somewhat oblique position before the eye, and close beside it a figure cut out of pasteboard, representing a plane outline prospect of the cube when in this position. Both objects he took to be something like flat quadrates. A pyramid, placed before him with one of its sides towards his eye, he saw as a plain triangle. This object was now turned a little, so as to present two of its sides to view, but rather more of one side than of the other: after considering and examining it for a long time, he said that this was a very extraordinary figure; it was

neither a triangle, or a quadrangle, nor a circle; he had no idea of it, and could not describe it; "in fact," said he, "I must give it up." On the conclusion of these experiments, I asked him to describe the sensations the objects had produced, whereupon he said that immediately on opening his eye, he had discovered a difference in the two objects, the cube and the sphere, placed before him, and perceived that they were not drawings; but that he had not been able to form from them the idea of a square and a disc, until he perceived a sensation of what he saw in the points of his fingers, as if he really touched the objects. When I gave the three bodies (the sphere, cube, and pyramid) into his hand, he was much surprised that he had not recognized them as such by sight, as he was well acquainted with these solid mathematical figures by his touch. These experiments prove the correctness of the hypothesis I have advanced elsewhere on the well-known question put by Mr. Molyneux to Locke, which was answered by both these gentlemen in the negative, and has been much discussed since their time.

4th Experiment.—In a vessel, containing water to about the depth of one foot, was placed a musket-ball, and on the surface of the water a piece of pasteboard, of the same form, size, and colour as the ball. The patient could perceive no difference in the position of these bodies; he believed both to be upon the surface of the water. Pointing to the ball, I desired him to take up this object; he made an attempt to take it from the plane of the water, but when he found he could not grasp it there, he said he had deceived himself, the objects were lying in the water; upon which I informed him of their real position. I now desired him to touch the ball, which lay in the water, with a small rod; he attempted this several times, but always missed his aim; he could never touch the object at the first movement of his hand towards it, but only by feeling about with the rod. On being questioned with respect to reflected light, he said that he was always obliged to bear in mind, that the looking-glass was fastened to the wall, in order to correct his idea of the apparent situation of objects behind the glass.

When the patient first acquired the faculty of sight, all objects appeared to him so near that he was sometimes afraid of coming in contact with them, though they were in reality at a great distance from him. He saw everything much larger than he had supposed from the idea obtained by his sense of touch. Moving, and especially living objects, such as men, horses, &c., appeared to him very large. If he wished to form an estimate of the distance of objects

from his own person, or of two objects from each other, without moving from his place, he examined the objects from different points of view by turning his head to the right and to the left. Of perspective in pictures he had of course no idea; he could distinguish the individual objects in a painting, but could not understand the meaning of the whole picture; it appeared to him unnatural, for instance, that the figure of a man represented in the front of the picture should be larger than a house or a mountain in the back ground. All objects appeared to him perfectly flat: thus, although he very well knew by his touch that the nose was prominent, and the eyes sunk deeper in the head, he saw the human face only as a plane. Though he possessed an excellent memory, this faculty was at first quite deficient as regarded visible objects; he was not able, for example, to recognize visitors, unless he heard them speak, till he had seen them very frequently. Even when he had seen an object repeatedly, he could form no idea of its visible qualities in his imagination, without having the real object before him. Heretofore, when he dreamed of any persons, of his parents, for instance, he felt them and heard their voices, but never saw them; but now, after having seen them frequently, he saw them also in his dreams. The human face pleased him more than any other object presented to his view; the eyes he thought most beautiful, especially when in motion; the nose disagreeable, on account of its form and great prominence; the movement of the lower jaw in eating he considered very ugly. Although the newly-acquired sense afforded him many pleasures, the great number of strange and extraordinary sights was often disagreeable and wearisome to him; he said that he saw too much novelty which he could not comprehend. And even though he could see both near and remote objects very well, he would nevertheless continually have recourse to the use of the sense of touch.

On the 21st of September I operated, in the presence of several medical gentlemen, in one sitting, on both eyes for the congenital strabismus. The lids were fixed by the fingers of an assistant, the ball of the eye by a pair of forceps, and the tendon of the muscle divided by a pair of curved scissors. The rectus internus of the right eye was, like the organ itself, atrophied. The conjunctiva of the left eye was thickened at the inner angle; the muscle was uncommonly broad and thick; its tendon had a very broad attachment to the ball, and behind it was a separate bundle of muscular fibres attached to the sclerotica. The pupils of both eyes assumed immediately after their operation their proper position in

the orbits. No inflammation ensued; not even in the left eye, which, from the prior operation, was still rather sensitive. The *muscæ volitantes* became less irksome, and the violent spasms, which previously had affected not only the eyelids, but also the whole left side of the face, disappeared entirely. The right eye, which had been amaurotic, gained by this operation the power of perceiving light, so that when the left eye is closed, the patient can now distinguish light and shade, on the hand being moved before this eye. The sight of the left eye likewise was considerably improved in acuteness and clearness, both as regarded near and distant objects, but especially the latter. Objects now, however, appeared in a different situation to that which they really held; when, for instance, he directed his eye to an object situated immediately before him, he saw it more to his right, and, if he attempted to grasp it, he moved his hand in this wrong direction*. For this reason in walking across a room he always took a direction to the right, and, consequently often came unawares in contact with articles of furniture, &c. This appearance of objects in false positions lasted for two months, after which time he was also capable of walking forwards in a straight direction. The right atrophied eye, which before the operation was deeply sunk in the orbit, is now more prominent, and appears therefore fuller and larger, so that the difference of the two eyes is less perceptible; he has consequently gained considerably in personal appearance. On one occasion when I was honoured with a visit from Mr. Lawrence, Dr. Watson, Dr. Kerrison, and several other medical gentlemen at my residence, I introduced him to them for examination.

In the middle of October I let him try several pair of spectacles at Mr. A. Ross's in Regent Street. With a double convex lens of $5\frac{1}{4}$ inch focus, he saw both near and remote objects of large size most clearly and distinctly, but for small objects he could find no glasses that improved his sight. He recognized the capitals of a large print with his naked eye, and on looking through a pin-hole made in a card held close before the eye, he could distinguish even the small letters of a very minute print. He had not yet learned to read. The reason for the condition of his sight with respect to small objects, and that his vision is better on cloudy days, is no doubt to be

* This phenomenon I have observed in all eyes operated upon for strabismus of a great degree and long standing, when the other was closed. I have mentioned it in the *MEDICAL GAZETTE* for June 1840, vol. xxvi. p. 540, where I have also given an explanation of the physiological cause.

sought in the enlarged pupil and the immobility of the iris.

In the middle of November he was able without spectacles to read the names over the windows of the shops in the streets, and to tell the time to the minute by St. Paul's clock. Walking along in the crowded streets, especially in the City, he found very tedious. He said, seeing so many different things, and the quick movements of the multitude of people, carriages, &c., confused his sight to such a degree, that at last he could see nothing; that the sensation produced by the object last seen had not yet disappeared from the retina, when the next object made its impression thereon, by which means confusion of ideas, great anxiety, and even vertigo, were occasioned, from which he could only free himself by closing his eyes for a few moments.

In the middle of December an experiment was again made with spectacles. A lens of seven inches focus was now of the same service as one of $5\frac{1}{4}$ inches had been two months before. After the operation for the strabismus, he was accustomed, in speaking with any person, to turn his eye away from the face, as otherwise he said he felt disturbed by the looks of the person; he had now at length learnt to look at the eyes of those with whom he conversed. The old habit of using the sense of touch to examine the objects he had not yet entirely lost.

In the middle of February 1841, a third experiment was made with spectacles. A lens of ten inches focus was of the same service as one of seven inches had been on the last occasion, and one of $5\frac{1}{4}$ inches four months ago. This proves a slow, but positive amelioration of sight, and permits us to expect a still greater improvement; the more so as the patient has not passed the period of puberty. If the employment of spectacles were begun at the present period, although it is now more than seven months since the operation was performed, there would be no further amelioration of sight; the development of the visual apparatus would be arrested. I am therefore of opinion that the use of spectacles is not to be permitted, until it is, as it were, mathematically demonstrated by similar experiments with lenses, that the sight is no longer improved; by which means the faculty may in time, perhaps, reach such a degree of perfection as not to require any lens at all for remote objects.

This is the only case on record within my knowledge wherein, with a person born blind and afterwards successfully operated upon at a period of life as far advanced as in this instance, such experiments have ever been made. In the well-known case of Cheselden, published in the *Philosophical Transactions* for the year 1728

(page 447), the patient was only in the fourteenth year of his age, and although the case contains many highly interesting physiological observations, no series of systematic experiments was instituted. Beer has also made some interesting observations, which, however, like those made in rather a superficial manner by Janin and Daviel, tend principally to describe the impressions which the newly-acquired sense had made on the mind of the person operated upon. In Ware's case the patient was not born blind, but had become so at an early period of life. In the present paper I have merely given the simple history of the case, without making any remark on several points interesting to the pathologist and physiologist, to which I shall advert on a future occasion; the explanation and philosophy of the foregoing experiments as to the sense of sight I shall attempt in another paper, which I purpose to lay before this society.

CAUSE OF CILIARY MOTION.

BY EDWARD FORBES, M.W.S., For. Sec.
B.S. &c.

WHEN, through the elementary animal tissue, (the passive gelatinous tissue, seen in the sponges), granules are interspersed, it becomes active, and presents motions of undulation, contraction, and extension. This granular tissue, in its simplest form, is seen in the Hydra, or fresh-water polype, and in the bodies of the Sertularian hydroid polypes. In the Arachnodermata (or Medusæ) we find the inactive gelatinous tissue becoming cellular, and constituting the greater part of the animal's body, but the motions of that body are effected by rings, bands, and processes, composed of the active granular tissue. In the order Ciliograda of that class we see the largest known examples of those remarkable organs the vibratile cilia. These cilia are lanceolate, bent, flattened processes, not tubular, as some have stated, but solid. Neither are they webbed together, as they have been figured, but separate. They are placed in transverse rows on short bars of the granular tissue, which, by the way, is always translucent, while the gelatinous tissue of a tegumentary state, of which the cilia themselves are composed, is always transparent. That the vibratile motion does not originate in the cilia themselves, is proved by the fact, that if one be cut away from its translucent base, it always remains immoveable; and that the motion properly resides in the base composed of granular tissue, is evident, since, if the smallest portion of that tissue remain attached to one of the cilia, when it is cut away, it continues to vibrate. Now, if we suppose a ciliiferous

bar to present regular undulatory motions in one direction, such a motion as is seen in the bodies of some of the lowest trematode Entozoa, the *Tetrastoma Playfairii*, for example, we have at once the explanation of the phenomena of ciliary motion in the *Ciliograda*. Such an explanation will also account for the ciliary phenomena presented by the wheelbearing animalcules and other infusoria where the undulations need only to be propagated in a circle to produce the revolving appearance. It may be questioned, however, whether there may not be a difference in the cause of motion between the voluntary cilia of these animals, and the involuntary cilia seen on certain membranes in others. But when we consider that the involuntary movements of the cilia round the margins of the remarkable cup-like processes which stud the branchiæ of the *Echiurus*, (discovered last winter by Mr. Goodsir and myself,) appear to originate in the same organization, we may speculate on the probability of the same causes operating in both cases. A minute inquiry into the nature of the involuntary vibratile cilia seen on mucous surfaces among the higher animals is most desirable.—*Edinburgh Monthly Journal of Medical Science*.

CHRONIC RHEUMATIC ARTHRITIS OF THE JAW.

MR. ADAMS presented specimens of chronic rheumatism of the joints, or, as it has been termed, chronic rheumatic arthritis. He had on former occasions exhibited specimens of the disease in almost every joint of the body; for as persons affected in this way generally die of other diseases, an opportunity is afforded for making post-mortem examinations. For the specimens which he was about to lay before the society, he was indebted to the kindness of Dr. Kirkpatrick, and Dr. Duncan. In this instance the patient died of acute inflammation of the larynx. The appearances before him were those of an advanced stage of the disease, but in which the morbid alteration was not complete. Mr. Haygarth, of Bath, the only author who has alluded to this form of disease, has termed it nodosity of the joints. He has described the disease in the hands, and it has been also noticed by Sir B. Brodie. Mr. Adams exhibited a cast of a hand affected with this disease, and observed that no one had as yet described a similar condition of the foot or of the jaw. The disease was said to occur only in persons advanced in life, but it has been seen in persons from thirty to forty. In the case before him there was deformity both of the hand and foot. The disease was attended with pain, frequently of an acute cha-

racter, and the usual phenomena were observed after death, osteitic deposits and destruction of the articular cartilages. Mr. Adams said he would not detain the meeting with any remarks on this subject, as the disease had been fully described by him in the *Cyclopædia of Anatomy and Surgery*, edited by Dr. Todd. He believed, however, that the state of the lower jaw had not been described by any writer, as far as his experience went. In the case before him the jaw was affected in a very remarkable manner; and in order to convey a correct notion of the appearance of the parts during life, he would beg leave to send round a drawing taken by Mr. Conolly. On examining the lower jaw, Mr. Adams found the head of the bone greatly enlarged; the condyle was also augmented in size, being nearly an inch longer in its vertical direction than it is in the normal state. The glenoid cavity, instead of retaining its natural shape, was quite oval, and like the glenoid cavity of the scapula; and instead of the maxillary eminence, in front, there was one uniform oval depression. The aspect of the patient, although contorted, differed very remarkably from the case brought forward by Mr. Smith; in Mr. Adams's case there being hypertrophy of one side instead of atrophy. The disease is not always confined to the ends of the bone, but sometimes extends to their shafts. Mr. Adams said he thought that this affection of the temporo-maxillary articulation had not been noticed by writers. In the case which he brought before the meeting, the progress of the disease had been watched, and the subsequent dissection had proved the diagnosis to be correct. He would not make any farther observations on the subject, as he had already published his views in the *Cyclopædia of Anatomy and Surgery*.—*Dublin Journal of Medical Science*.

NEW APPARATUS FOR OBLIQUE FRACTURES OF THE SHAFT OF THE FEMUR.

BY M. FOCACHON.

THE patient being placed in a horizontal posture, a bandage is applied from the toes to the groin. This done, two double bands, half of each of which remains loose, are placed by the sides of the limb, and extend from within a short distance of the seat of fracture for about a yard downwards. These are intended to furnish the principal fixed point for the continued extension, which M. Petrequin, the inventor of this method, calls *permanent parallel extension*.

To fix them firmly in this position, they are starched, or covered with a layer of dextrine, and then rolled round with another

bandage ; and then another quantity of starch or dextrine is applied over the whole together. An assistant constantly keeping the limb in a horizontal position, and at the same time maintaining some extension of it, the other half of each of the bands, which was hitherto left loose, is now applied along either side of the limb, and arranged like the first, so as to go to the same distance beyond the foot as it does. Two fresh rollings of the bandage, and another layer of starch, serve to fix this band in just the same manner as the first. Six or eight very narrow and flexible splints, almost as long as the femur, are next applied over the fracture, and are kept in place by a bandage ; and if necessary, a second layer of splints, and another bandage, are added ; and then the whole are well starched, to fix them all in one firm mass. Long rigid splints, with two pads, fix the limb laterally, as in the method commonly employed, till the solidification of the apparatus is completed ; and two flexible splints are placed behind, and two in front of the thigh, in order that the compression may be made methodically and equally over every part of the limb. The leg, since it does not naturally lie in the same plane as the thigh, must be slightly raised by a little cushion, so as to bring it to the same level. The apparatus being thus disposed, and having dried, the constant extension of the limb is made by means of a weight, greater or less according to the circumstances of the case, which is fixed at the lower extremity of the two bands already described as passing from below the fracture to beyond the foot, and which are carried over a rod at the bed's foot, and made to slide on it as on a pulley.

The advantages of this mode of extension are, that it acts over a considerable extent of the limb without fatiguing the patient ; that it acts only on the lower portion of the fractured bone, and thus more effectually and more regularly ; that it draws uniformly in the direction of the axis of the limb, and parallel to the fractured bone ; that it prevents shortening, and that the patient cannot escape from its influence by sliding down lower in the bed.—*L'Expérience*, Mars 11, 1841.

[The mode of permanent extension by a weight is far from novel, but the apparatus described is rather better for the purpose than any we have seen. It might be at the same time improved and simplified, without altering its essential excellences.]

ROYAL COLLEGE OF SURGEONS.
LIST OF GENTLEMEN ADMITTED MEMBERS.
Monday August 30.

Edwin Thomas Wait.—John Little.—Frederick John Ffolliott Payne.—Matthew George Painter.

—George Alexander Waters.—George Robert Ridley.—William Edward Charles Nourse.—John Bateman Wheelhouse.—Thomas Holt.—Samuel Hare.

Tuesday August 31.

Thomas O'Beirne.—William Huggins.—William John Bowden.—Robert Malcomson.—William Theodore Elliott.—John Whitmore.

TABLE OF MORTALITY FOR THE
METROPOLIS.

Shewing the Number of Deaths from all Causes registered in the Week, ending Saturday, the 4th Sept. 1841.

Small Pox	9
Measles	23
Scarlatina	18
Hooping Cough	29
Croup	7
Thrush	9
Diarrhœa	20
Dysentery	4
Cholera	2
Influenza.....	0
Typhus	36
Erysipelas	3
Syphilis	0
Hydrophobia.....	0
Diseases of the Brain, Nerves, and Senses ..	137
Diseases of the Lungs, and other Organs of Respiration.....	221
Diseases of the Heart and Blood-vessels	21
Diseases of the Stomach, Liver, and other Organs of Digestion	71
Diseases of the Kidneys, &c.....	3
Childbed	4
Ovarian Dropsy	0
Diseases of Uterus, &c.	4
Rheumatism	2
Diseases of Joints, &c.	4
Ulcer	1
Fistula	0
Diseases of Skin, &c	0
Diseases of Uncertain Seat	91
Old Age or Natural Decay.....	45
Deaths by Violence, Privation, or Intemperance	22
Causes not specified	2
Deaths from all Causes	788

METEOROLOGICAL JOURNAL.

*Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 6° 3' 51" W. of Greenwich.*

<i>September.</i>	THERMOMETER.		BAROMETER.	
Wednesday 8	from 49 to 64		29.67 to 29.78	
Thursday . 9	51 65		29.95	29.97
Friday . . 10	51 68		29.95	29.97
Saturday . 11	55 71		29.99	29.97
Sunday . . 12	57 75		29.90	29.83
Monday . . 13	55 75		29.83	29.84
Tuesday . 14	59 74		29.82	29.78

Wind, W. on the 8th ; S. on the 9th ; SW. on the 10th ; E. on the 11th ; SE. on the 12th ; E. on the 13th ; SE. on the 14th.

On the 8th, generally clear. The 9th, cloudy ; a little rain fell during the morning. Since remarkably clear.

Rain fallen .215 of an inch.
CHARLES HENRY ADAMS.

WILSON & OGILVY, 57, Skinner Street, London.

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